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> *"To the solid ground*
> *Of Nature trusts the mind that builds for aye."*—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, NOVEMBER 3, 1870

THE GOVERNMENT OF THE ROYAL SOCIETY

WE have so often maintained in these columns that Science cannot now be propelled on its onward course by the efforts of unassisted individuals only, and that the State must itself, sooner or later, put its shoulder vigorously to the wheel, that there is some danger lest we should be thought to undervalue the force of private enterprise. We, on the contrary, attach very high importance to such enterprise, which exists amongst us in England more abundantly than perhaps in any other country in the world. It exists in two forms—in that of detached individual effort, and in that of voluntarily associated bodies, the Scientific Societies. To the latter only we propose now to address ourselves.

The services that have been rendered to science by these societies infinitely surpass in kind and in amount all that has been done by means of all other agencies. By bringing together men struggling for the same goal, though often by different routes, by submitting to the arbitrament of open discussion opposite views relating to the same subject, by publishing theoretical speculations however divergent, and experimental results however discordant, and by rewarding pre-eminent services, the truth has the fairest possible chance of being elicited, and the non-scientific classes become the recipients—though often the unconscious, and therefore ungrateful, recipients—of benefits, material as well as intellectual, immeasurable in value. Sweep away what has been done for Science in England by Scientific Societies, and scarcely a trace of Science would remain. For it must not be forgotten that individual labourers, working in however isolated a manner, are largely indebted to the stores of knowledge garnered in the Proceedings of the Societies, for the very tools with which they operate.

Such being the vast importance of these bodies, their constitution and system of internal government are questions of the highest interest. We propose to confine our remarks on the present occasion to the Royal Society, the highest of all, and that which should be the pattern to all others.

In former days, election to the Royal Society was an easier matter than it is now. At present, personages of Royal blood and peers of the Realm alone have special facilities for admission. The ordinary candidates are submitted to an ordeal of considerable stringency. Virtually they are elected, though nominally only selected, by the Council, whose decisions, however, are almost invariably ratified by the Society at large. The claims to admission of each candidate are carefully and fully discussed in Council, and fifteen only are nominated by ballot each year. The number of candidates is usually about fifty. Though the Council's list of fifteen does not always give universal satisfaction, yet it must be allowed that names rarely, if ever, appear on that list whose bearers cannot point to actual scientific work performed by themselves. Such a thing as the election of a thoroughly unscientific or unintellectual man is unknown in the present day. It follows that the standard of mental power to which the Fellows of the Royal Society must, as a body, have attained, is very high. It may, indeed, be safely asserted that no corporation in the kingdom, or even in the world, can be for a moment compared for mental power with the Royal Society. So much, in briefest terms, for its constitution.

The system of internal government by which the affairs of a body like this are regulated becomes a matter of the deepest moment, not only to the Society, but to the nation and to civilisation itself.

The governing body, the Council, is composed of a President, five Vice-presidents, and twelve ordinary members. These are all honorary posts. Two ordinary Secretaries and one Foreign Secretary, members of the Council with votes, are paid, the first two 300*l*., and the last 100*l*. per annum, out of the funds of the Society. There is also a Treasurer, a member of Council, but unpaid. An Assistant Secretary and a Librarian, not members of the Council, and of course both salaried, perform all the necessary routine duties.

The Vice-presidents and ordinary members of Council sit two years only, and then retire by rotation. They cannot be re-elected until a year has elapsed since their retirement. The Presidentship is not limited as to duration, nor are the posts of Secretary, Foreign Secretary, and Treasurer.

There is a wide-spread feeling that this form of government admits of improvement, and as the actual occupants of the posts in which an alteration is thought desirable stand deservedly very high in the estimation, not of the scientific world only, but in that of the community generally, the reform of which we are about to speak can fortunately now be discussed without personality, and without any fear of the acrimony to which, under less auspicious circumstances, such a discussion would inevitably lead.

The proposed alterations are of the very simplest kind, namely, that the tenure of office of the President should coincide with that of the rest of the Council, and that the Secretaries and Foreign Secretaries should be unpaid.

The inconveniences of the present arrangement, on which our space only admits of a few words, are, first, that however efficient, impartial, and undespotic the President and officers may be, their permanent tenure of their posts for a number of years in succession must tend to constitute them, in a Council undergoing yearly change, more or less an *imperium in imperio*. Indeed, their very efficiency and mastery of rule and precedent, in themselves most valuable attributes, aggravate, as well as generate, this tendency. The practical effect necessarily is, that the President and officers naturally and unavoidably get into the way of acting together, and of bringing before the Council matters for deliberation in somewhat of a cut and dried condition. At the opening of the Session, the new members, it is well known, are naturally diffident of expressing views adverse to those thus prepared for their acceptance by such experienced hands; and it is a common remark that it is only in his second year that a member serving on the Council for the first time usually declares his sentiments with independence and freedom. The choice, therefore, seems to lie between the experience which results from long service in the chair and secretariat, and the greater scope for deliberative activity, which limited service in those posts would afford.

In deciding between the two alternatives, the character of the Council must be considered. It contains a small selected section drawn from a large highly select body, the very *crême de la crême* of the science and intellect of the kingdom, men who, one and all, are supposed to have gained their position by the most severe intellectual discipline, and who value that position as one of great responsibility and high honour. If chance, or favouritism, or money, or rank, had any appreciable influence on their election, the case would be very different. Some dry nursing might then be not amiss. But in the actual case, a Council composed of the flower of English intellect may safely be left to deliberate with unfettered republican freedom.

Another inconvenience attending the permanent, or rather unlimited, Presidentship, is one which may be indicated without in the slightest degree applying it to the present distinguished occupant of the chair, namely, the extreme difficulty, without causing a scandal, of removing an inefficient or undesirable President.

A third inconvenience consists in the tendency towards an unduly Conservative policy, which a permanent President is liable to betray; and a fourth disadvantage is, that the particular department of Science to which the President is devoted is apt to be kept too continuously prominent. These tendencies are opposed to the vigorous progress and the wide expansion of scientific thought which it is the purpose of the Royal Society to foster.

We have but lightly touched upon the salient features of the question, which is one admitting of a vast variety of opinions, some of which, we trust, will be elicited by our remarks, for the appearance of which in these columns we feel that no apology is necessary.*

THE GEOLOGY OF THE DIAMOND FIELDS OF SOUTH AFRICA

In the September number of the *Cape Monthly Magazine* is an interesting article on the above subject, by Dr. John Shaw, Gold Medallist in Geology at Glasgow University, from which we have made the following extracts:—

"In February 1869, I published a paper in the *Grahamstown Journal* on the geological structure of the Vaal Region along the line where diamonds were found. This was chiefly intended as a reply to Mr. Gregory's denial of the veritability of the discovery of diamonds on various grounds, mainly geological and mineralogical, after a journey of exploration in the region.

"Since that time the finds of surface diamonds have increased, the stretch of country supposed to be diamondiferous has extended, and, at the present time, systematic digging and washing for diamonds are being carried on with an enthusiasm which success alone can have created, by upwards of 1,000 white men in different parts of the Vaal Region, but principally at Klipdrift, near Poreil.

"In July of this year I made considerable observations in the Vaal Valley, which show that the rocks are chiefly trappean, metamorphic, and conglomerate in character. I detected no pure granite formation, but syenite is, however, developed extensively, and seems to be the base of the whole system of rocks at Klipdrift. A very singular rock appears in the shape of isolated boulders on the summits of the Kopjes, and especially of the celebrated Old Kopje. This I take to be graphic granite (binary granite), or what Dana would call 'granilite,' consisting solely of quartz and large crystals of felspar.

"Above the syenite is a trap conglomerate in some places, in others are amygdaloids, and protruding through these again, basalt, assuming everywhere the hexagonal structure, and arising in some places into insulated and compacted columns.

"In some of the Kopjes there are remains of stratified rocks—clay schists, sandstone, chalk (or something very like it), which are evidently the last vestiges of a vast series of sedimentary strata, which formerly covered the whole present contour, but which have gradually given way to denudation and cataclysm.

"Such is the character of the present rock system at Klipdrift, and with a few additions (mainly supercumbent) of the whole rock series of the Vaal region.

"On the summits of the Kopjes, and as a matter of course, in the crevices between the basaltic boulders, is an alluvial gravel. In this are found the diamonds, and on the surface some have been found, indicators of the wealth beneath. The pebbles of sandstone, quartzite, crystalline sandstone, granite, clay slate, agate, tourmaline, iron pyrites, garnet, garnet spinel, &c., which compose this alluvium, are all roundedly polished and waterworn, and are imbedded at Klipdrift in a brownish, fatty earth.

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* The foregoing article, received from a valued contributor, is of so much importance that we have given it this prominence without committing ourselves to an approval of the precise course proposed, we rather invite discussion.—Ed

"The question arises, Is this alluvium of recent or ancient formation? Did the majority of the pebbles exist in the form of a conglomerate, aggregated from the alluvium of a former age? Or have the Kopjes at no very late period been the bed of the river?

"It is my opinion that the water-worn gravel has been under the influences of running water prior to the last great changes which formed the present landscape. The greater number of the water-worn pebbles and boulders are of the basalt of the Kopjes. Many of them are a crystalline sandstone, others are water-worn fragments of clayslate, sandstone, &c., of the sedimentary rocks which exist in the Kopjes. The agates, tourmalines, and garnets are undoubtedly from some supercumbent conglomerate sandstone which has yielded to denudation and no longer exists at Klipdrift, and also to a considerable extent from the amygdaloidal trap everywhere prevalent. I have in my possession from the Vaal a single fragment of red sandstone containing garnets, but I have not succeeded in tracing this to its source.

"It will, therefore, be sufficiently apparent that there must have existed, at a remote geological period, a series of metamorphic and sedimentary rocks which lay above the present rock system of the region, and that, through successive disturbances and persistent denudation, these have been worn away, forming in great part the alluvial soil of the present surface. In some few spots remnants of this series still exist, as in the clay-slaty crystalline sandstone and conglomerate of Sitlacomies Valley, in the thin layers of claystone, sandstone, and micaceous sandstone of some of the Kopjes now worked for diamonds, and generally in the fragments of sedimentary rocks scattered over the surface along the whole Vaal Valley.

"I am decidedly inclined to think that the diamonds have not been washed down from some higher region. I hope to show in another article that the Free State possesses an independent diamondiferous centre, and that there no river has existed at any time, for there is no evidence of water-wearing, and the soil is not alluvial. Diamonds have been discovered two hours' distance from Potchefstroom, and all down the Vaal to its junction with the Orange River, and thence to ten hours' distance below Hope Town. This is a stretch of at least 500 miles. I believe the diamonds have come from some rock which may now have vanished, but which existed formerly throughout the whole region.

"In concluding at present, I have to make some observations on the position of the gravelly soil which is now being washed for diamonds. The old diggers are in favour of the summits of the Kopjes. They have tested this belief, or rather formed it, from their experience of the old Kopje. How can it be explained that the soil is alluvial and yet deposited far above the influences of the river? For two or three miles inland, which I investigated, there is everywhere on the heights the same deposit.

"There are certain facts which enable me to point out the geological history of these Kopjes. The summits are all basalt. This has been protruded through the amygdaloidal and conglomerate traps. At a subsequent period, however, there must have been another elevation for the blocks and columns radiate from a centre, so that the crevices are wedge-shaped, or expanding outwards to the surface. This subsequent upheaval was evidently not simultaneous throughout the whole region, but successive, and therefore the bed of the stream was changed from place to place. The present bed of the Vaal cannot be an old one, and the whole surface of the country as far as the alluvial soil extends was, at different previous times, under the wearing and breaking influence of the river. Granting, then, a series of rocks such as have been described undergoing water-wearing by the ancient Vaal, which by intermittent and successive upheavals was compelled continually to change its course, and the presence of alluvial gravel on the summits of the Kopjes far and wide is easily explained.

"In the hollows no gravel is apparent, because a thick covering of sand, the accumulation of present denudation, lies over the gravel. Diggers do not care to undertake the labour of carrying off the surface sand at present. In time this will be done, and I am convinced there will be found more diamonds than on the Kopjes. And when the day comes when the bed of the stream shall be searched by deflecting the water in canals through the many flats which abound in the Valley of the Vaal, a superior diamondiferous gravel will be worked. From all I saw and for the reason I have now advanced, the present diamond digging of South Africa is only trifling in comparison to what it should and will ultimately be."

THE QUARTERLY WEATHER REPORT

Quarterly Weather Report of the Meteorological Office, with Pressure and Temperature Tables for the Year 1869. Part I. January—March, 1869.

IT is an arduous undertaking to establish and work a system which shall give us a perfectly full, trustworthy, and continuous account of the meteorology of even so small a part of the globe as the British Isles. The Meteorological Committee of the Royal Society are therefore deserving of credit in the systematic effort which they have made to establish the weather records of these isles upon a scientific foundation. Nor must we forget that our Government has been very liberal in this matter, and that a grant of 10,000*l.* a year devoted to meteorology represents a very handsome contribution from that national purse which is, alas! so often shut when it ought to be open, and so often open when it ought to be shut.

Let us now consider how far the Committee have succeeded in advancing our knowledge of British Meteorology, and in what respect, if any, they have fallen short of that which they might have been expected to accomplish. For this purpose let us divide the labours of the Committee into three heads, and consider separately their system of obtaining information, their system of discussing it, and, in the last place, their system of publication.

In the first place, and with respect to their observational system, it is hardly necessary to state that they have established seven observatories in which the various meteorological elements are registered continuously by means of photography, or that the Kew Observatory has undertaken to examine the records from these various outlying observatories before they are sent to the central office. Nor is it necessary to detail the other steps which have from time to time been taken by the Meteorological Committee to insure instrumental and observational accuracy; for men of science have only to examine the various publications of the office to be convinced that a large amount of accuracy has been already achieved.

In addition to the observations from self-registering instruments, other records of a less complete nature come to the office in continually-increasing quantity; for, evidently, the records from only seven stations, however completely equipped, are insufficient to give us a true view of the very complicated meteorology of these isles. It is, therefore, an important duty of the chief officer of the Committee so to increase this stock of observations as to obtain in time a complete and trustworthy meteorological record. There seems reason to believe that this will ultimately be done, and it will be a great boon to meteorological science when it is accomplished.

But, if the observational system is important, the method of reducing observations is a point of equal importance. The condensed account of the quarter's weather, and of its easterly storms, by Mr. Scott, are exceedingly useful summaries, and form, as it were, the first step of the ladder which leads from facts to laws, and it is hardly necessary to state that such summaries have a practical as well as a theoretical importance.

We pass on from these to consider next the tables of averages for the year 1869, which have been given in this Quarterly Report. As far as the air-temperature and pressure are concerned, there can be no objection to tables giving average results. These are two meteorological elements of a nature sufficiently simple to render averaging desirable; and the five-day means of those elements given in page 41 form, perhaps, the best way of accomplishing this. But surely the readings of the wet-bulb thermometer do not represent any simple meteorological element! The moisture is best represented by ascertaining the mass of vapour present in a cubic foot of air, this forming its legitimate expression in terms of *mass* and *volume*, which are fundamental physical conceptions. On the other hand, the temperature of the wet bulb, while it forms the easiest and best observational method of obtaining continuous information regarding moisture, is yet in reality a very complicated joint function of the temperature of the air, of its pressure, and of the mass of vapour present in one cubic foot. To give five-day readings of the wet-bulb thermometer cannot, therefore, we think, lead to any good result.

We are just beginning to know a little about the motions of the atmosphere and its variable components, and if we wish to extend our knowledge in this direction, it seems perfectly essential that the physical meteorologist should choose proper methods of reduction. His method ought not to be one which, when accomplished, *may possibly* increase our knowledge, but one which, from its very nature, *must necessarily* do so. He ought to seek to have the same certainty which the astronomer possesses, that in treating his observations after a particular method, the results will infallibly extend his knowledge of celestial motions.

We have dwelt so long upon this part of the labours of the Meteorological Committee, that we can only briefly allude to their system of publication. The reduced graphical representations of the observatory records given at the end of the volume, while hardly enough for the wants of meteorologists, are yet extremely valuable and useful. It is impossible to say what benefit to science may not result from bringing before the public such a speaking epitome of weather, and we owe many thanks to Mr. Francis Galton, the member of the Meteorological Committee who invented the instrument which has given us these admirable plates.

BALFOUR STEWART

BEET-ROOT SUGAR

On the Manufacture of Beet-Root Sugar in England and Ireland. By William Crookes, F.R.S., &c., Editor of the *Chemical News*. Illustrated with ten engravings. Pp. 290. (London: Longmans, 1870.)

THIS work is founded on a series of articles by M. Julien M. Deby, C.E., published about a year ago in the *Scientific American*; these articles have, however been very much extended, and much new matter added, in order to bring the subject down to the present date, and so increase its usefulness in assisting those who may wish to establish beet-root farms and sugar factories in this country. The experiences obtained abroad, and investigations made in England and Ireland, show that it would be quite possible to grow sugar-beets with profit in the United Kingdom. The beets might be used as fallow crop and cultivated, instead of the roots grown in such great quantities as food for cattle, since the beet-root pulp after the extraction of sugar is even more valuable for this purpose.

During the year 1867, beet-root sugar of the value of 1,600,000*l*. was imported into this country, and it would appear that this might readily have been produced here. In the first chapter we have a description of the beet, and of the qualities that can most profitably be used for sugar making; the weight of each root should not be less than 1¼lb., nor more than 2lbs.; smaller roots are frequently woody, while larger ones are watery and poor in sugar. The juice should have a specific gravity between 1·060 and 1·070, though sometimes, when very rich in sugar, it rises to 1·075 or 1·078. The percentage of sugar in the roots varies considerably, the minimum quantity given in a long list of analyses being 3·62, while the maximum is 13·47. The next number below this maximum is 13·19, and is interesting as representing the amount of sugar found in red beet manured with London sewage. Peligot obtained as much as 18 per cent. from some French beets, and some American specimens have produced 17·6 per cent. It has been found in Ireland that from 16 to 40 tons of roots may be grown on one acre, so that satisfactory results might be anticipated in that country. Chapter II. treats of the culture of the beet, the climate, kinds of soil, manure, and all the necessary directions to the agriculturist to ensure a profitable return. Chapters III. to VII. contain a detailed description of the mode of extraction of the sugar, and a very useful statement of the cost of the different pieces of apparatus required for working up 150,000lbs. of beet-root per twenty-four hours during five months, which would be the average yield of a 500 acre farm. Chapter VIII. gives the quantity of water required in such a factory, amounting to no less than 113,190lb., or 1,882 cubic feet per hour; the expense of labour for one year at 5,190*l.*, the total annual expense being 13,980*l.*, the total receipts being estimated at 20,470*l.*, leaving a profit of 6,490*l.*, assuming that 8 per cent. of sugar is extracted from the roots. It is however probable that this percentage might be raised to 10 per cent., when the profit would be 10,090*l.* The first outlay for the establishment of the factory is calculated at 10,845*l.* Mr. Baruchson estimates the profit at 24¾ per cent. on the outlay when 6½ per cent. of sugar is produced, each additional ½ per cent. increasing the profit 7½ per cent., so that if 8 per cent. could be obtained the profit would be no less than 48 per cent. The ninth chapter describes the concreting process of Mr. Fryer as applied to the raw juice, so as to enable the refinery to be carried on during the whole year instead of only during crop time. Chapter X. is devoted to the application of the spent beet-root pulp. As far as chemical analysis indicates it will prove, when mixed with other materials, a more useful food for cattle than ordinary mangolds or even than the original roots, though it must be admitted that no comparative experiments on feeding have yet been made. The remaining five chapters describe the manufacture of spirit from beet juice, which has been found very profitable on the Continent; the sucrate of lime process which dispenses with the employment of animal charcoal; the manufacture of potash salts from the residues; Excise regulations, and Dr. Schiebler's calcimeter for the determination of the quantity of carbonate of lime in animal charcoal.

We commend this valuable work to all interested in the subject; and wish the author success in his endeavours to introduce and encourage the extensive cultivation of beet-root in this country, and thus place us on a level with our neighbours on the Continent who have so successfully carried it out.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Dr. Balfour Stewart's Opening Lecture at Owens College, Manchester

DR. BALFOUR STEWART concludes his excellent Lecture lately delivered at Owens College, Manchester, and published in your number for Oct. 20th, with a broad classification of experimental and observational work into work requiring much time and work requiring comparatively little time for its execution. This appears to me a very useful suggestion. Dr. Stewart then goes on to say that the work requiring short periods of time "may be furthered with much advantage in institutions such as Owens College." And he adds that the same objects are at present aided by the Government grant of 1,000*l.* so carefully administered by the Royal Society; but he thinks this grant might be advantageously increased. In all this I quite agree with him.

Dr. Stewart then proceeds to deal with the other class of scientific work. And here I had better quote his own words. He says:—"But when we come to experiments and observations requiring great time, the case is very different. Certain experiments, whether from the great time they require, or the great expense they demand, cannot be well performed in a College; while routine and long-continued observations, such as those connected with the various branches of cosmical physics, are of such a nature as to require a central establishment to superintend their organisation and reduction. There is thus, I think, the necessity for a central establishment of some kind, devoted to that class of experiments and observations requiring great time, great space, and great expense for their completion."

In every word of this also I agree with Dr. Stewart. But I think a few words more are wanted to tell us by whom or how these institutions should be founded and supported. I, who have had the advantage of very frequently discussing the question with my friend Dr. Stewart, infer, without any hesitation, that he considers this should devolve on the State, which, as I have often stated in public, is my own opinion. But I think it a pity that, at a time when the question is attracting in scientific circles so much attention, this should have been left to inference by one so well qualified to speak with authority, and on an occasion which afforded so excellent an opportunity of educating public opinion, on a subject which, outside scientific circles, is so little understood.

ALEX. STRANGE, Lt.-Col.

The Aurora Borealis

A PRETTY bright display of the aurora, which was witnessed here last evening, exhibited such peculiar phenomena, that although I am told that they are common accompaniments of the aurora here, a description of them may yet be new to some of your readers. The times given in the description are by estimation from the striking of town clocks; the night being dark, and the light of the aurora not sufficient to enable me to consult a watch.

At $8^h\ 25^m$ a straight double beam of faint white light extended from Altair in the west, across γ Andromedæ overhead, nearly to the E.N.E. horizon. The northern branch of the arch was the brightest, about 3° broad, and it was accompanied at a distance of 10° or 12° on the southern side by a parallel and fainter arch. I was prevented from watching the duration of this appearance and the further progress of the aurora until a few minutes after ten o'clock. At that time a few columnar streamers of white were visible in the west, one of which, very bright, extended from α Sagittæ to ζ Cygni, and was accompanied by fainter streamers from β to between γ and ϵ Cygni, and from Altair to δ Cygni. I noted their direction, and that of a few other streamers later, in opposite parts of the sky, in order to determine the position of their centre of convergence. But this was at the same time clearly shown by a patch of nebulous white light, 10° or 15° wide, from the centre of which, at β Andromedæ, and to some distance beyond its borders, faint rays spread outwards, and mixed themselves with faint streamers which rose in close array, from the north and east, towards them. While watching this small light-cloud, its light and that of the surrounding streamers became rapidly and brightly intermittent. It soon faded, and on reaching an eminence where I could command the whole northern sky from east to west, I found that all the features of the aurora were undergoing very rapid changes. Two of the many broad and bright streamers which rose in that direction at about $10^h\ 25^m$ were directed from β Ursæ minoris to γ Cephei, and from θ Aurigæ to Algol, having their bases at the former stars, and connected, apparently, by no regular fringe or arch below, but appearing at different heights between the zenith and the horizon, and occupying chiefly the north-west to north-east quarter of the sky. The full extent of the intermittent phase, of which I had before obtained only a confined and obstructed view, was now also visible. Waves of light coursed each other over the whole extent of the streamers in no very regular direction or succession, but so as to give the general impression that conflicting currents of air, chiefly from the north, blew out and carried along with them the light of the streamers towards their highest points, or from one streamer to another. Hardly more than a second was occupied by the waves in spreading from the horizon to the zenith; and in their number, appearing simultaneously, they appeared to vary from a quick succession of ripples to a single wave. This agitation of the streamers subsided at about $10^h\ 30^m$. The centre of convergence of the streamers between β and γ Andromedæ was variously marked at about this time by a faint corona, without enclosed light, or by a nebulous light-cloud composed of irregularly radiating beams. It was traversed, apparently, by the waves from the north, east, and west, as rapidly as other portions of the sky; and but little indications of the aurora were visible to the southward from this point. For a space of about ten minutes the aurora appeared to be fading, but at about 10^h 40^m a bright streamer, 5° or 6° broad at its base, rose upwards from the "pointers" to above the Polar star, while the whole northern half of the sky was again covered with fainter streamers. Waves of light flashed rapidly along the principal beam, from its base to near the zenith in about one second, and at the same time drifted upwards over the other parts of the aurora in extremely vivid and rapid succession. The progress of the disturbance continued the same, and was watched for about ten minutes, during which time occasional bright streamers rose and faded, and all the beams of the aurora were equally lighted up by the flitting waves. The motion of the latter appeared to be in parallel lines rising upwards from the N.N.E. horizon, and where in that direction the bases, or brightest parts, of the streamers were arranged in a continuous succession of altitudes from near the horizon to the zenith, the waves appeared to be propagated in the most regular and unbroken manner. Tall streamers at a considerable distance east or west of the magnetic north were lighted up very rapidly from their bases to their summits, as if directly confronted throughout their whole lengths by the advancing waves. At about 10^h 45^m, the cloud-like apex of the aurora was somewhat nearer to γ than to β Andromedæ, and it was lighted up like the occasional tall streamers in the east and west, by almost momentary flashes of pale, phosphorescent light. The impressions of a luminous vapour, like that which floats over phosphorus, of the *ignis fatuus*, or of the disturbed surface of a phosphorescent sea, blown by the wind, were most vividly suggested by the flickering changes of brightness in portions of the auroral cloud overhead. At 10^h 50^m the disturbance ceased, and the streamers gradually resumed their steadiness, some appearing soon afterwards in the south-west, from between θ Pegasi and α Aquarii to α Pegasi; and others, in the south-east, across Aries and Taurus. The auroral apex was faintly visible, at this time, near γ Andromedæ. At about 11^h. a third disturbance among the auroral beams occurred for a few minutes overhead. A slightly curved arch 2° or 3° broad, extending towards the east and west about 20° on each of the apex, and lancelike streamers in the east and west, which, together with the arch, were in their ordinary state invisible, were repeatedly lighted up suddenly and very brightly, and were immediately again extinguished; the light sometimes appearing in the beams and sometimes in the arch, as if it were bandied to and fro between them. The streamers in the north were at this time very faint, and those in the south-east and south-west were almost entirely hidden by clouds, which a rising wind now drove across them from the south. From 11^h 8^m to 11^h 10^m a rapid succession of horizontal waves and wavelets of light rose in parallel lines above the N.N.E. horizon, drifting, apparently, overhead towards the south. As they appeared to catch the beams, and the arch which still remained extended across the apex towards the east and west, these were suddenly lighted up, and immediately again extinguished, as before; the flickering and dancing effect of the light which they produced resembling that reflected on the clouds in the south from distant iron-smelting furnaces upon the opposite bank of the Clyde. A repetition of the flashing lights, which, I presume, must have been those described by ancient writers as *capræ saltantes*, and by mariners familiar with displays of the aurora in high latitudes as "merry-dancers," occurred again among the beams overhead between 11^h 12^m and 11^h 14^m. Soon after this, thick clouds came over from the south, and the sky very shortly afterwards became overcast.

The beams of this aurora were uniformly white, without any trace of colour. But the farthest east and western beams of a bright aurora seen here from 8^h to 11^h on the evening of Thursday last, the 20th inst., were of a rich crimson red, and one tall streamer of that aurora, reaching nearly to the zenith, exactly in the north, was tinged with crimson at the top. A south-west wind, accompanied by rain, succeeded that aurora on the following day. Last night a south-west gale sprang up, and there was a considerable fall of rain here this afternoon I heard no crackling sounds during the brightest flashing of this aurora; but such sounds might very possibly be produced in Arctic regions by the cracking of ice, which great pressure, or a change of temperature in a gale of wind, would be not unlikely to occasion as a concomitant of the aurora, if, as was recently suggested by the late Admiral Fitzroy, auroral displays in these latitudes accompany, and are pretty certain indications of the existence of, very stormy weather at a distance.

A. S. HERSCHEL

Andersonian University, Glasgow, Oct. 26

A WONDERFULLY fine auroral display took place last night, very far exceeding in extent and brilliancy that of the 24th ult., as seen from this place. It began to show itself soon after sundown, attained its maximum about 8 o'clock, and had not wholly disappeared at 11. At about 8 o'clock more than half the visible heavens was one sea of colour, the general ground greenish, yellow, and pale rose, with extensive shoals of deep rose in the east and west, and from the north; streaming upwards to and beyond the zenith, tongues and brushes of rosy red so deep that the sky between looked black. The spectroscope, a direct-vision one, showed four lines in the rosy portion and one in the greenish; one strong red line near the C, one strong pale yellow line near the D, one paler near the F, and one still paler beyond, with a faint continuous spectrum from about the D to beyond the F. The C line was very conspicuous and the brightest of the whole, intermediate in position and colour to the red of the lithium and the calcium, with both of which I am familiar; plainly there were two spectra superposed, for while the red portions of the aurora showed the four lines with a faint continuous spectrum, the greenish showed only one, near the D on a faint ground. Of course, no numerical accuracy was attainable with so simple an instrument, only the judgment of the eye; but the conviction was very strong that the rosy hue was owing to hydrogen, possibly resulting from decomposition by electrical discharges of the excessively attenuated watery vapour existing in the higher regions of the earth's atmosphere, which Tyndall has shown to be capable of producing the blue colour of the sky, and by the consequent loss of which the blackness of space was discernible.

T. F.

St. Mary Church, Torquay, Oct. 25

SHORTLY after sunset this evening an ill-defined auroral arch was seen in the north. At about 7·45 patches of rose-coloured light were visible about the constellations Auriga, Ursa Minor, Ursa Major, &c., and at about 8 o'clock brilliant crimson rays shot up to the zenith, and the sky seemed one vast mass of fire. The auroral light was visible as far south as Cetus and Aquarius. The crimson tint passed from time to time into a greyish light.

When the most brilliant portions were examined with the spectroscope, two bright lines were visible, one a greenish-grey line situated about the middle of the spectrum, and the other a red line looking very much like the C hydrogen line.

London, Oct. 24 W. B. GIBBS

DURING the recent brilliant auroral displays (Oct 24th and 25th), I observed four bright lines in the spectrum of the crimson beams of the corona.

1. A broad and well defined red band near C.
2. A bright white band near D (? the same as Angstrom's line with wave-length = 5567). I have frequently seen this line even during very faint displays; on the 25th it was visible in every part of the sky.
3. A faint and rather nebulous line, roughly estimated to be near F.
4. A very faint line about half way between 2 and 3.

The red band was absent from the spectrum of the white rays of the aurora, but the other lines were seen.

Bedford, Oct. 29 THOS. G. ELGER

ON the night of the 25th a most gorgeous aurora borealis was visible at North Shields. I first observed it about 6 P.M., when it formed a splendid boreal crown, of which the centre was about 25° south-east of the zenith. Rays of brilliant crimson converged to it from all directions, especially from N.E., S., and S.W. To the north the light was more of the ordinary colour. They appeared to rise from an irregular circle, extending round the whole horizon, and slightly arched in the N.W. Below this was the usual dusky cloud. When the rays, or rather sheets, of crimson were at their brightest, they were streaked with yellowish light. At times the centre of convergence was dark, at others it was occupied by luminous clouds of twisted forms, reminding me of those of some of the nebulæ. The rays seemed to have a slow motion towards the south.

About eight o'clock the crown gradually faded, and the light of the centre flickered with a tremulous motion. At 8.15 an arch shot across the sky from N.E. to S.W., passing just north of the pole star. It slowly drifted south, and at 8.30 was in the zenith. At 10 the boreal crown had reappeared, but was of the ordinary yellowish colour.

The spectrum of the red rays contained a brilliant red line, more refrangible than Hα, in addition to those usually seen. It was situated about ·4 of the distance from C to D. If any other observer noted the position of the arch observed at 8 15 P.M., I shall be glad to be informed, in order to calculate the height.

Clementhorpe, N. Shields, Oct. 27 HENRY R. PROCTOR

ANOTHER display of aurora borealis occurred this evening. It was not to be compared in splendour with that of the night preceding, but it had some interesting and instructive features. The sky was not clear at any time, and the masses of red light, which occupied generally similar situations to those of the preceding night, were interrupted in many places by dense clouds. I observed it at about half-past six P.M., and at that time the most remarkable feature was that streamers (generally not of a red colour) radiated from every part of the north horizon accurately to a point defined very nearly by one of the stars σ Cygni or υ Cygni, which were then near the meridian. I did not see both stars, and I am therefore in doubt, as they are of equal magnitudes, which was the star nearest to the point of convergence of the streamers.

The radiations were so well marked and so accurately directed to one point, that I mentally compared them to the ribs of an expanded umbrella. This did not last long; in a few minutes fine streamers went from the N.W. horizon towards the south-east, to the east of this point, which was then covered with red light without streamers. The largest masses of red light were, as in the preceding evening, south of the zenith, and in the south-east and south-west quarters of the heavens.

Radcliffe Observatory, Oxford, Oct. 25 ROBERT MAIN

A MAGNIFICENT auroral display was visible here on the evening of the 24th, between $8^{h}\ 0^{m}$ and $8^{h}\ 30^{m}$.

The maximum of intensity must have occurred between $8^{h}\ 0^{m}$ and $8^{h}\ 10^{m}$, but, being otherwise engaged, I did not observe anything myself until $8^{h}\ 25^{m}$, when the E. and W. regions of the sky, more especially the latter, were illuminated with a crimson or reddish glow, somewhat resembling the reflection of distant conflagrations, but on neither side did this glow appear to reach the zenith by many degrees. Shortly after the time mentioned ($8^{h}\ 25^{m}$) both disappeared, after which a phosphorescent whitish light was observed nearer to and on the S.E. of the zenith. The barometer had, during the previous day or two, shown considerable variation in atmospheric pressure.

Another display was observed on the following evening (25th), which commenced about $6^{h}\ 0^{m}$ and continued visible more or less until $7^{h}\ 0^{m}$.

The first indication that I noticed was a fiery glow similar to that seen on the previous evening, but considerably higher, and almost immediately after a magnificent broad stream of light, consisting of reddish and light tints, was observed in the N.E. extending upwards for 50° or 60°.

About $6^{h}\ 20^{m}$ the whole of the northern region of the sky extending to E. and W., and about 15° S. of the zenith, was more or less illuminated, and I should say the maximum of intensity occurred at this time. The principal luminous streams and coruscations appeared between N.E. and E N.E. appearing first in the latter direction and increasing towards the former.

On one occasion I noticed faint luminous streamers rising from different northerly directions and converging in the zenith; these, together with the coloured bands of light before mentioned, formed a magnificent and imposing spectacle.

The northern sky afterwards presented an appearance of twilight until about $10^{h}\ 45^{m}$.

During the display the barometer stood at 29·53 (corrected for temperature). Temperature of air 49°. The minimum temperature registered during the night was 42°.

Meteorological Observatory, Twickenham JOHN J. HALL

COLLINS, in his "Superstitions of the Highlands" has these lines:—

> As Boreas threw his young Aurora forth
> In the first year of the first George's reign,
> And battles raged in welkin of the North,
> They mourned in air, fell, fell rebellion slain!

The Editor (Routledge's edition) in a note states "By 'young Aurora' Collins undoubtedly means the first appearance of the Northern Lights, which happened about the year 1715; at least it is highly probable from the peculiar circumstance that no ancient writer had taken any notice of them, nor even any modern previous to the above date." Can any of your readers state whether this is correct.

C. POCKLINGTON

Poole, Oct. 27

AN aurora borealis was visible at this place on the evening of the 25th inst., between the hours of 7 and 8.30 P.M. A beautiful crimson glow was first observed towards the north-east, veiling, but not hiding, the larger stars, and the Pleiades had the appearance of a wedge of pale yellow mist behind the veil. On the horizon, looking due north, was a semicircular luminous space of clear pale light, of the colour of eastern sky just before dawn, and from this there darted at intervals over the crimson glow long slender rays of yellowish light, giving an exceedingly beautiful appearance to the phenomenon. Clouds, which had been hanging about during the day, gathered over the scene towards 9 o'clock, and when they afterwards dispersed before midnight, the glow, though still perceptible, was fading away. A falling star was observed at about eight, but considerably to the south of the aurora. There had been an aurora observed on the preceding evening, but of a less striking character. The weather has been for the last ten days extremely unsettled, sirocco (S.E.) winds prevailing, and an unusual rainfall the result, accompanied sometimes by hail, and by thunder and lightning. But clear bright days occur in the intervals of these storms, when the sky is of an intense blue, against which beautiful forms of cloud mass themselves by degrees as the day goes on, and become at length the subjects of those gorgeous atmospheric effects which make the autumnal sunsets of the bay of Fiume rivals of those of Rome.

Fiume, Oct. 28 A. M. SMITH

[In addition to the letters printed above, we have received from many other correspondents interesting and valuable descriptions of the magnificent display of the aurora, which the demands of other subjects on our space alone prevent us from publishing.—ED.]

The Aurora of Sept. 24

IT may interest your readers to know that the very brilliant aurora of the 24th and 25th September last was also visible in Canada. Mr. W. B. Dawson, writing from Montreal, notices the occurrence of a very bright aurora on both nights, flashing much, and often bright crimson. It was also seen at Quebec, and attracted much attention. He observes that its appearance was simultaneous with the division of a very large spot on the sun. Its crimson colour agrees with the red hue of your other correspondents; and is somewhat remarkable, as I have often noticed, in Canada, that the red usually alternates with green in vivid displays.

GEORGE M. DAWSON

Royal School of Mines

Hereditary Deformities

THE alleged instances of hereditary deformity produced by your correspondent in NATURE for Oct 20 do not seem at all satisfactory. They may all be referred either to an hereditary disease of the part affected, as in the suppuration of the cow's horn; or to coincidence, accompanied by a slight stretch of imagination on the part of the first narrator, as in the cases of the scar on the forehead and the crooked finger.

Prof. Huxley, in his lectures on Natural History at the Royal School of Mines in 1864, after speaking of the short-legged breed of Ancon sheep, and the six-fingered Maltese, Gratio Kelleia, said that although natural malformations were thus transmitted, artificial malformations never were; and instanced the fact of the mutilation produced by circumcision never being transmitted to the offspring. This, of course, is a negative argument, but it has great weight when we consider how many thousands have undergone that mutilation without an instance of its having been inherited by their children.

Faversham, Kent, Oct. 25 WILLIAM FIELD

The Cefn Reptile and the "Times"

THE remarkable paragraph in the *Times* of the week before last relating to the discovery of "a huge beast of the lizard tribe," in a cave at Cefn near St. Asaph, implies a belief on the part of the editorial staff that such an addition to the British fauna was not impossible, and its wide circulation proves the astonishing credulity of the public:—

"In the Vale of Clwyd, at a distance of two miles from the cathedral city of St. Asaph, are situated the Cefn Caves. It had been rumoured of late that parties visiting this place had on several occasions seen some strange animal creeping in its dark recesses, and on Saturday visitors reported having had a good view of him, and stated it was a huge beast of the lizard tribe. On the Monday following Thomas Hughes, from Rhyl, went to try to capture him. Armed with a stout stick he approached its reported lair, but not seeing it he decided to remain in ambush at the mouth of the cave, sheltered by a projecting ledge. After having thus waited an hour his patience was rewarded with success. He could hear in the far end a hum as of a hive of bees. The sound growing louder, and now apparently quite close, Hughes peeped round the ledge, and saw the monster within three yards of him. He (Hughes) sprang towards him, and dexterously wielding his stick he dealt him a well-aimed blow upon the neck just behind the head, which caused him to stagger and reel. One more blow in the abdomen finished him. Hughes carried him home in triumph, and is now making a profit out of the affair by exhibiting him at Rhyl. The monster is of the lizard tribe, as mentioned above. Only that our country is destitute of those creatures we should have said it was a young crocodile. It measures from the nose to the end of its tail exactly 4ft. 7in., the tail being rather more than half that length. Its limbs measure 12in.; the front ones have five toes; and the hind ones four; it is web-footed. Above it is black and white beneath. Its coat is mailed, quite hard, and protruding in sharp corners and angles, like the crocodile's. The head is low and flat, the mouth large and round at the end, measuring 7in. by 3in.; the teeth are numerous, but small, and bear great resemblance to those of a large codfish. There is ample scope here for naturalists to investigate the how and wherefore this strange amphibian came to be discovered in the present epoch among the hills of North Wales."

Such is the vivid account of the capture given in the *Times*, and reprinted in several local papers; and so far as I can judge by my letters, believed in by many simple-minded people. It is altogether a most impudent hoax. The man Hughes is a sweep, who purchased a reptile which happened to die in a travelling menagerie at St. Asaph, and exhibited it at Rhyl as having been caught in the Cefn Caves, until at last it became a public nuisance, and was committed to the earth. The story related in the *Times* was invented merely to make the exhibition lucrative to Hughes the sweep. Its wide circulation, which incidentally shows an astonishing ignorance of natural history, is the only excuse for my writing this letter.

W. BOYD DAWKINS

MAN AND NATURAL SELECTION

THE following reply to M. Claparède's "Remarques à propos de l'ouvrage de M. Alfred Russel Wallace sur la Théorie de la Sélection Naturelle," was written some months ago, and was intended as an appendix to a French translation of my "Essays" by M. Lucien De Candolle, to be published by Reinwald, of Paris. As it is now very uncertain when the translation will appear, and as M. Claparède's critique has been highly spoken of in several English periodicals, I think it advisable that my answer to it should be no longer delayed.

In the "Archives des Sciences de la Bibliothèque Universelle," for June, 1870, M. Edouard Claparède has done me the honour to make my "Contributions to the Theory of Natural Selection" the subject of some critical remarks. To these I now propose briefly to reply.

I must premise that I do not intend to discuss here any of those difficulties which my critic finds in the theory of sexual selection, and which apply as much to Mr. Darwin's views as to my own. because, in his new work now announced, that theory will, I have no doubt, be fully developed, and be supported by a mass of facts and observations, in the absence of which further argument is useless. I proceed therefore to the objections that apply more especially to my own views.

At p. 15 of his "Remarques" M. Claparède says, "Son étude est consacrée á la coloration des oiseaux et, absorbé dans son sujet, l'auteur oublie que d'autres facteurs peuvent, aussi bien que la couleur, attirer l'attention des ennemis sur la gent ailée. Un nid couvert d'un dôme volumineux échappera tout aussi peu, grâce à ses dimensions, à l'œil d'un animal en quête de proie, que quelques plumes brillament colorées. Les gamins de nos villages en savent quelque chose, comme l'a remarqué M. le Duc d'Argyll, et ils ne réussissent que trop, à la présence d'un gros nid, à deviner l'oiseau caché et sa couvée." This objection does not seem to me very serious, because in the first place, nests, however large, generally harmonise in colour with surrounding objects, and are not so easily seen at a little distance as a bright patch of colour; and, secondly, because "gamins" are not the chief natural enemies of the feathered tribes, while hawks and falcons do not break open nests, although they do seize and devour birds.

After giving (p. 23—25) what I must allow to be a very fair abstract of my reasons for believing that Natural Selection is not the only power that has operated in the development of man, M. Claparède intimates that I have so completely abandoned my own Darwinist principles that the reader will easily refute my arguments. He therefore confines himself to certain "reflections." I regret that he did not think it necessary to do more than this, because I have as yet in vain sought from my reviewers for any other than general objections to my arguments on this subject, and am at a loss to know how they can be so easily refuted. M. Claparède's "Reflections," however, do, fortunately, take the form of arguments. He says (p. 25), "M. Wallace n'a pas reculé devant l'explication de la formation graduelle du chant de la fauvette et du rossignol par voie de selection naturelle. La chose est toute simple, bien fou serait celui qui voudrait recourir ici à l'intervention d'une Force supérieure, ami du Beau! Les fauvettes femelles et les rossignols de même sexe ont toujours accordé de préférence leur faveurs aux mâles bons chanteurs. C'était la conséquence de leur goûts musicaux et des aptitudes harmoniques de leur oreille. Malheur aux pauvres mâles à registre peu étendu ou à timbre fêlé! les douceurs de la paternité leur ont été impitoyablement refusées; ils sont morts de jalousie dans la tristesse et l'isolement. Ainsi s'ést formée la race des bons chanteurs qui peuplent nos bocages. Pourquoi n'y a-t-il pas des chanteuses? Sans doute que les oiseaux mâles ne se sont jamais souciés de la voix de leurs épouses, soit parcequ'ils n'avaient pas l'oreille juste, soit plûtot, car cela sera contradictoire, parceque leurs gouts musicaux étaient suffisament satisfaits par leurs concerts personels. Peut-être aussi les femelles n'avaient-elles point d'aptitude virtuelle au perfectionnement de la voix; peut-être avaient-elles atteint l'extrême limite de développement vocal compatible avec l'organisation d'un oiseau du sexe féminin; ou bien enfin la sélection naturelle produite sous l'influence des poursuites exercées par des ennemis de toutes sortes contre les belles couveuses, sélection favorable, selon M. Wallace, à la production de couleurs sombres, a-t-elle mystérieusement éteint même l'éclat de sa voix? Quoiqu'il en soit, il est évident pour M. Wallace que la sélection sexuelle, en d'autres termes le goût des dames fauvettes pour la musique, a amené le grand perfectionnement de la voix des virtuoses de l'autre sexe. Mais dans l'espèce humaine, la chose aurait-elle pu se passer ainsi? Le chant harmonieux et enchanteur d'une *prima donna* aurait-il pu naître et se perfectionner par voie de sélection? Le goût musicale des auditeurs pourrait-il avoir eu une influence selectrice sur ce phénomène? Jamais, au grand jamais! Seule l'intervention d'une Force supérieure a pu amener un résultat pareil, car jamais homme primitif n'a eu de goût pour la musique. M. Wallace le sait bien: il a vécu si longtemps parmi les sauvages qui ont pu le lui dire! Au contraire, les femelles fauvettes primitives et les femelles rossignols primitives, avaient déjà le goût musical longtemps avant que leurs époux eussent appris à chanter.

Comment M. Wallace le sait-il? Le lui ont-elles dit? N'importe, il le sait."

It is a pleasure to read anything so brilliant as this, but it hardly seems to touch the point of my argument. Male birds *do* sing at pairing time to the females. Mr. Darwin says in his "Origin of Species," "All those who have attended to the subject believe that there is the severest rivalry between the males of many species to attract, by singing, the females." Female birds do *not* sing. These are facts, and they perfectly accord with the theory of the perfection of song having been developed, in the *males*, by sexual selection. In man the facts are all different. Savage women have generally no *choice* as to their husbands, as has been so fully shown by Sir John Lubbock; and in the few cases where a choice is open to them, there is not a particle of evidence to show that a musical voice ever determines that choice. Still less reason is there to think that this quality determines the male savage in choosing his wife. Yet a wonderful musical organ has been developed in both sexes, of which the use to man in his struggle for existence has not yet been shown. Surely here is a difficulty which required facts and arguments for its elucidation rather than a brilliant display of wit.

Again, in reply to my arguments as to the total absence of hair from the back of man, we are told that it should be no difficulty to a person who believes that *hairy* mammals and *feathery* birds have been derived from *scaly* reptiles ("Remarques," pp. 27, 28). But surely this is not the argument of a Darwinian. For the hair and the feathers are *useful* to their several possessors, just as the scales were to their ancestral reptiles; whereas the very essence of my difficulty is, that the nudity has *not* been shown to be *useful* to man. M. Claparède thus concludes his remarks on this subject:—"Que M. Wallace soit au moins conséquent dans la question de la chute des poils. Si l'intervention d'une Force supérieure lui semble nécessaire pour épiler le dos de l'homme, qu'il sache se résoudre à la faire agir de même sur l'échine de l'éléphant, du rhinocéros, de l'hippopotame ou du cachalot." But the four mammals here mentioned are *thick-skinned* animals, one *aquatic*, one *amphibious*, the other two inhabitants of *hot* countries, lovers of *shade* and of *marshes*. Can anything be more clear than that, in all these cases, the hair was little or not at all wanted, and, owing to their habits, was very probably even injurious, and has therefore partially disappeared by means of natural selection? while the extinct mammoth and woolly rhinoceros are instances which prove that it always re-appeared when the needs of the animal required it. If the hair disappeared from the back of tropical man by the action of the same law which caused it partially to disappear from the tropical elephant, we must ask why it did not re-appear in the arctic Finns and Esquimaux, as it re-appeared in the arctic mammoth? It is rather for me to say—"Que M. Claparède soit au moins conséquent dans la question de la chute des poils."

The last point on which my critic remarks is my argument, that the brain of savage man is in advance of his needs, and therefore could not have been acquired by natural selection; and he asks, why I do not apply the same reasoning to many other cases, especially to that of the great group of birds with a complex larynx, comprising all the singing birds, yet having many species which do not sing. He says (p. 29), "Ces oiseaux possèdent dans leur larynx un organe beaucoup trop bien conformé pour l'usage qu'ils en font. Il est donc nécessaire d'admettre l'intervention d'une Force supérieure pour façonner cet appareil, inutile aux oiseaux qui le possèdent, mais calculé en vue de générations nouvelles qui, dans un avenir plus ou moins éloigné et dans des conditions déterminées apprendront à chanter. Que M. Wallace aurait-il à répondre à une semblable argumentation?" My answer is, that the cases are not parallel or similar; if they were so, I should certainly adopt the same conclusion in both. To make them logically comparable, it would be necessary to prove that all the earlier forms of the group had the vocal organs fully developed, but did not sing; or what might be held to indicate this, that at present only a few species sing, while the great mass do not. But so far from this being the case, the majority of the species of the group have musical or sonorous voices, and there is no evidence to show that the vocal apparatus was fully developed before the power of singing began to be exercised. Man, on the contrary, stands alone in the development of his brain, and M. Claparède does not rebut the evidence I have adduced to show that the brain in savage and prehistoric man was in advance of his requirements.

In concluding his remarks, M. Claparède endeavours to impale me neatly on the horns of a dilemma, as follows:—"Ou bien M. Wallace a eu raison de faire intervenir une Force supérieure pour expliquer la formation des races humaines et guider l'homme dans la voie de la civilisation, et alors il a eu tort de ne pas faire agir cette même force pour produire toutes les autres races et espèces animales ou végétales; ou bien il a eu raison d'expliquer la formation des espèces végétales et animales par la seule voie de la sélection naturelle, et alors il a eu tort de recourir à l'intervention d'une Force supérieure pour rendre compte de la formation des races humaines." These are his last words, and they seem to me to be the weakest in the whole paper, being a pure begging of the question. They assume that man presents no phenomena which differ in kind from those presented by other animals, whereas I have adduced a number of such phenomena which my critic has neither disproved nor denied. My whole argument is founded on certain facts, and on these facts only. My critic admits the facts, does not refute my arguments, yet maintains that I should give up my conclusion, because the theory of Natural Selection *must* apply equally to man and the rest of Nature, or to neither. But why must it do so? Darwin himself claims no such universality for it. He admits that even the common origin of animals and plants rests only on analogy, and that "it is immaterial whether it is accepted or not." But M. Claparède is more Darwinian than Darwin himself, and would, I presume, say that, either all animals or plants must be descended from one common ancestor or, that no two species are thus descended. I maintain, however, that man is descended from a lower animal form, but I adduce facts which go to prove that some other law or power than Natural Selection has specially modified him. If Darwin is not anti-Darwinian in admitting, as he does, the possibility that animals and plants may not have had a common ancestor, I may surely deny that I am anti-Darwinian when I show that there are certain phenomena in the case of man that cannot be wholly explained by the law of Natural Selection.

I must not conclude without thanking M. Claparède for the very flattering terms in which he has spoken of the larger portion of my work, and also for the general accuracy and fairness with which he has condensed my views and arguments in the last essay, to which he especially takes objection.

A. R. WALLACE

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*THE NATURAL HISTORY OF MAN**

IN the two handsome volumes before us is contained such a mass of interesting information concerning our less cultivated brethren as has surely never yet been collected by one writer or in one work. The first volume is occupied with Africa, that vast, and, as recent researches show, densely populated land, whose peoples present a greater variety of manners and customs and languages than any others upon the globe, and the second treats of the American tribes, the inhabitants of Australia and New Zealand, with India, China, Japan, and Siam. A short notice is also given of the long-perished lake-dwellers of Switzerland.

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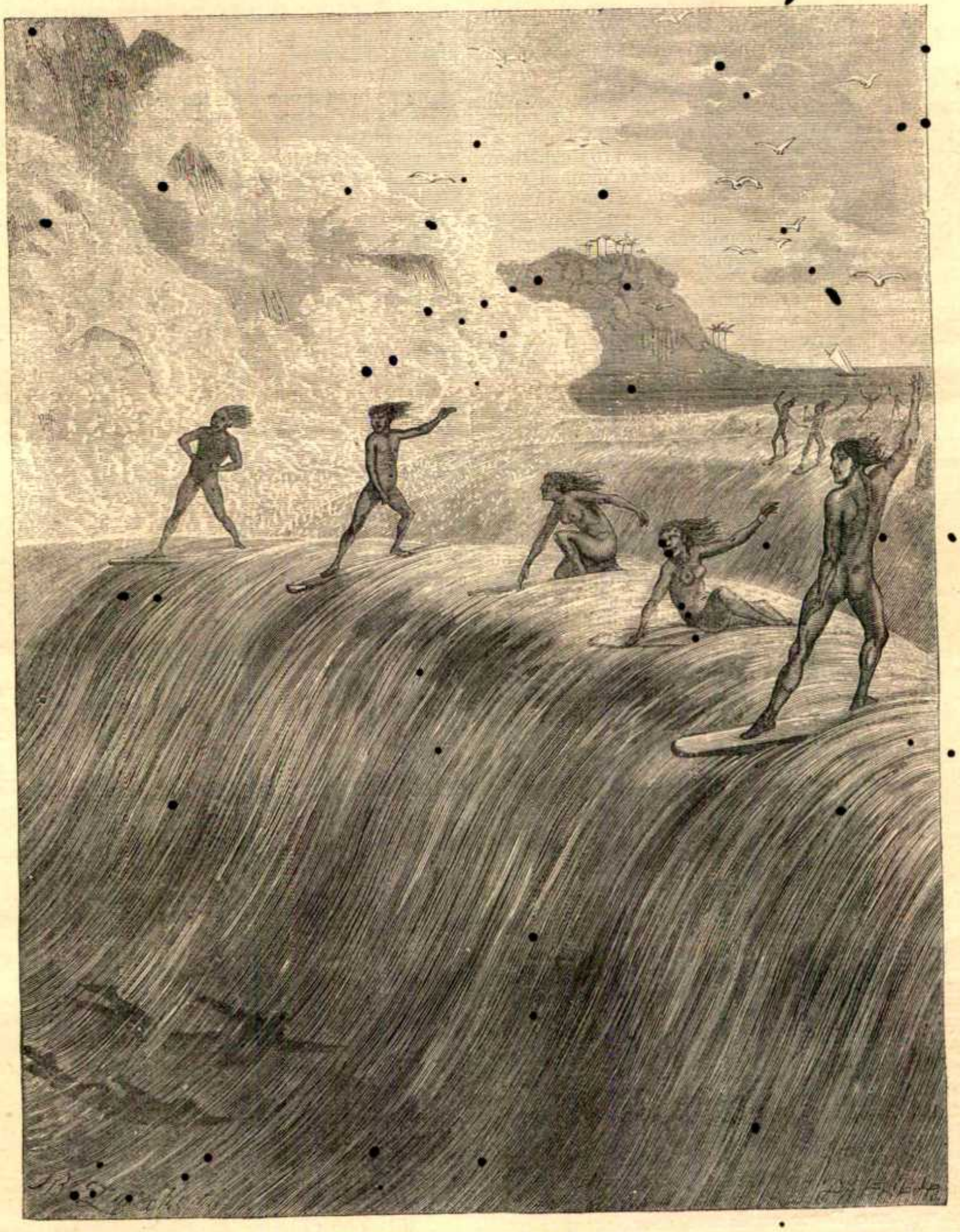
* "The Natural History of Man; being an Account of the Manners and Customs of the Uncivilised Races of Men." By the Rev. J. G. Wood, M.A., F.L.S., with new designs by Zwecker, Angas, Danby, Handley, &c. Engraved by the Brothers Dalziel. 2 vols. 1868-70. (London: George Routledge and Sons.)

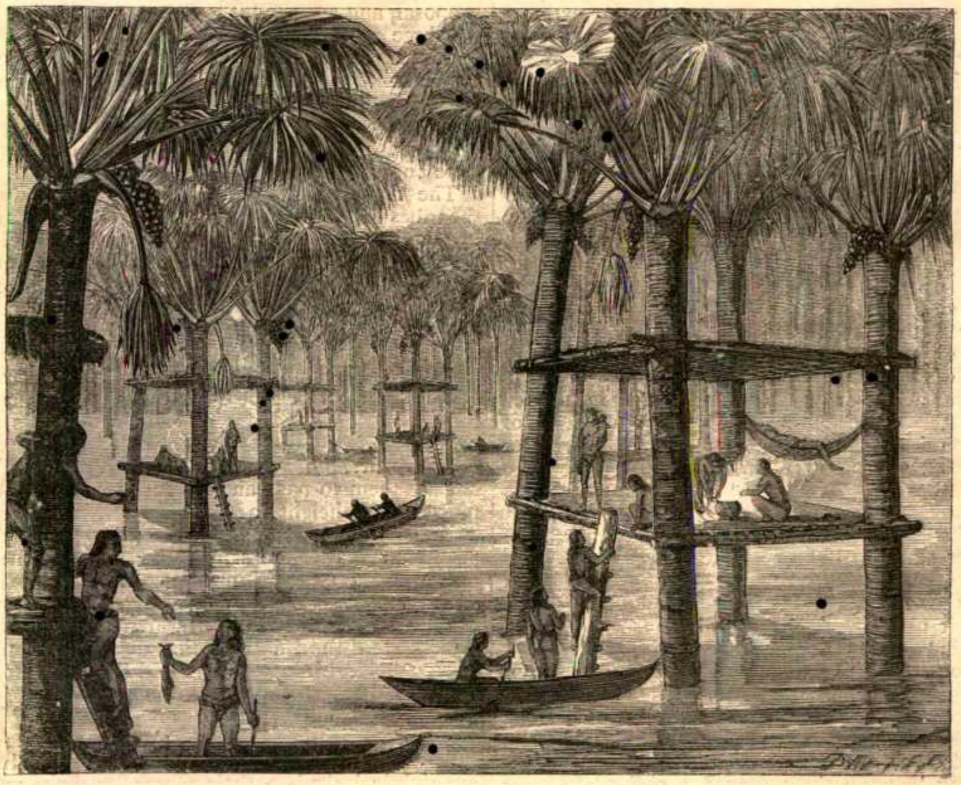
The general plan pursued by Mr. Wood in his account of different nations is necessarily very similar. The obtainment of food and the manufacture of the means of getting it—the bow and arrow, blow-tube and poisoned shaft, the canoe, the javelin, the club, the boomerang, or lasso; war and the requisite weapons or means of defence; dress, simple and slight as it often is; and religious observances of one kind or another, constitute, with the initiatory ceremonies attendant upon entrance into manhood, marriage and death, the principal occupations and events of the life of the savage, and these, of course, form the staple of Mr. Wood's work. To do this well, however, is no slight task, considerable reading and comparison of the accounts of travellers is required, and the whole has to be worked up into the form of a continuous narrative. Mr. Wood appears to have carefully selected his authorities, and has taken only what he considers trustworthy and reliable. To give some idea of the method adopted, we may refer to his account of the Zulu Kaffirs, who he considers to have descended



SURF-SWIMMING IN THE SANDWICH ISLANDS

from the northern regions of the Continent to their present abode, and who, as is well known, are a dark-skinned but highly intelligent race. While possessing some of the characters of the negro, as the crisp, woolly hair, large wide lips, and dilated nostrils, they differ radically from him in the possession of a lofty and intellectual forehead, a more prominent nose, high cheek bones, and a nameless but decided cast of countenance. As a people, they are devoid of care, requiring no clothes, building huts of the slightest construction, and obtaining food with the greatest facility. Their reasoning powers are highly developed, and they delight in controversy. Mr. Wood then proceeds to describe the life of a Kaffir from infancy to old age, including an account of his dress, ornaments, and ceremonial observances.

To the account of the Kaffir there succeeds an equally interesting and trustworthy description of the Hottentot and of the Bosjesman or Bushman. Then follow accounts of the Korannas, the Namaquas, Bechuanas, Ovambos, and the numerous tribes of Southern and Central Africa. The facts recorded appear to have been drawn from many different sources, as Baines, Chapman, Moffat, Lichtenstein, Anderson, Burchell, Petherick, and, of course, largely from the narratives of Livingstone, Speke and Grant, Sir Samuel Baker, Du Chaillu, and Burton. The incidents selected to illustrate the character and habits of each race are, in general, very pertinent and striking, and render the whole work as amusing as it is instructive. Thus the love of finery innate in the African is well illustrated in the following story :—"An English vessel arrived at an African port, a large part of her cargo consisting of stout iron wire ; nearly the whole of this was bought by the natives, and straightway vanished, no one knowing what had become of it. The mystery was soon solved. Suddenly the Kaffir belles appeared in new and fashionable costume. Some of them had been to towns inhabited by Europeans, and had seen certain 'cages' hung outside the drapers' shops. They inquired the use of these singular objects, and were told they were the fashionable attire of European ladies. They straightway burned to possess similar costumes, and when the vessel arrived with its cargo of wire, they bought it up, and took it home for the purpose of imitating the white ladies. Of course they had not the least idea that any other article of apparel was necessary, and so they wore none, but walked about the streets quite proud of their fashionable appearance."



THE LAKE-DWELLINGS OF THE ORINOCO

The extraordinarily despotic power possessed by the chiefs of many of these tribes over the property and lives of their subjects constitutes a very remarkable chapter of their history, and is illustrated by Captain Speke's account of M'tesa, the king of the Waganda, to whom a rifle having been presented, he loaded it, and handed it to one of his pages, telling him at the same time to go and shoot somebody in the outer court. The page, a mere boy, took the rifle, went into the court, and in a moment, to Captain Speke's horror, the report of the rifle showed that the king's order had been obeyed. This barbarian was in the habit not only of flogging his wives fearfully with whips made of hippopotamus hide, but of killing them without the slightest remorse. Speke states that scarcely a day passed without some woman being led forth to execution.

In the account of the Andaman Islanders, their consummate skill in the use of the bow is described ; their harpoon arrows, with which the Mincopies catch the larger fish, and which are very similar to those of Vancouver's Island, their beautiful canoes and extraordinary rowing, or rather paddling powers, beating our best crews with facility ; and their family affection. To this succeeds an account of the scarcely more civilised natives of New Guinea, with their tufted hair, active climbing habits, and curious weapons. Then follows a description of the natives of the Polynesian Islands, the Fiji with their wonderful coiffures, their ingenious manufacture of veils, fans, baskets, and canoes, their warfare and cannibalism; the Solomon Islanders and natives of New Hebrides; and after these the natives of Borneo and Sumatra, and the various American tribes.

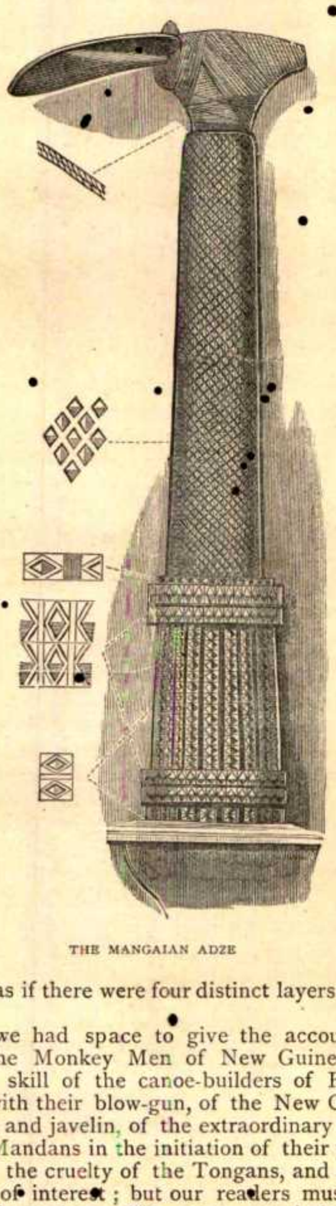
We append an account of the surf-swimming of the Sandwich Islanders, with an illustration, as copied by Mr. Wood from the now, we fear, seldom-read "Captain Cook's Voyages," who gives the following spirited account, which will not improbably be new to many of our younger readers:—"The surf, which breaks on the coast round the bay, extends to the distance of about 150 yards from the shore, within which space the surges of the sea, accumulating from the shallowness of the water, are dashed against the beach with prodigious violence. Whenever, from stormy weather, or any extraordinary swell at sea, the impetuosity of the surf is increased to its utmost height, they choose that time for this amusement, which is performed in the following manner. Twenty or thirty of the natives, taking each a long, narrow board rounded at the ends, set out together from the shore. The first wave they meet they plunge under, and suffering it to roll over them rise again beyond it, and make the best of their way by swimming out into the sea. The second wave is encountered in the same manner as the first, the great difficulty consisting in seizing the proper moment of diving under it, which, if missed, the person is caught by the surf, and driven back again with great violence, and all his dexterity is then required to prevent himself from being dashed against the rocks. As soon as they have gained by their repeated efforts the smooth water beyond the surf, they lay themselves at length on their board, and prepare for return. As the surf consists of a number of waves of which every third is remarked to be always much larger than the others, and to flow higher on the shore, the rest breaking in the intermediate space, their first object is to place themselves on the summit of the largest surge, by which they are driven along with amazing rapidity towards the shore. If by mistake they should place themselves on one of the smaller waves which break up before they reach the land, or should not be able to keep their plank in a proper direction on the top of the swell, they are left exposed to the fury of the next, and to avoid it are obliged again to dive and regain the place from which they set out. Those who succeed in their object of reaching the shore have still the greatest danger to encounter. The coast being guarded by a chain of rocks, with here and there a small opening between them, they are obliged to steer their board through one of these, or in case of failure to quit it before they reach the rocks, and, plunging under the wave, make the best of their way back again. This is reckoned very disgraceful, and is also attended with the loss of the board, which I have often seen with great terror dashed to pieces at the very moment the islander quitted it. The boldness and address with which we saw them perform their difficult and dangerous manœuvres was altogether astonishing, and is scarcely to be credited." These swimmers used often to pass nearly a mile seaward in order to enjoy the rapid motion of their return as long as possible. Both sexes and all ranks unite in it, and even the very chiefs themselves, who have attained to the corpulency which they so much admire, join in the game of surf-swimming with the meanest of their subjects. Some of the performers acquire a wonderful amount of skill, and, not content with lying on the board, sit, kneel, and even stand upon it as they are hurled shorewards by the giant waves. The boards are of various sizes, according to the age and station of the owner. For adults they are about six feet in length. They are slightly convex on both sides and are kept very smooth, all surf-swimmers cherishing a pride in the condition of their boards, and taking care to keep them well polished and continually rubbed with cocoa-nut oil.

As an example of the wonderful strength exhibited by savages, the case of the Dyaks of Borneo may be cited, one of whom, while on the march with some English soldiers, exhibited it in a very unexpected manner. "The path was a terrible one, up and down steep and slippery hills, so that the Chinese coolies, who accompanied the party, first threw away their rice, and lastly sat down and wept like children. The English sergeant, a veteran accustomed to hard marching both in China and India, broke down at the first hill, and declared his inability to move another step under the load which he carried. Mr. Brooke, who was in command of the party, asked one of the Dyaks to carry the sergeant's burden, and promised him an additional piece of tobacco. The man was delighted with the proposal, and accepted it. He was already carrying food for three weeks, his whole store of clothes, one twelve-pound shot, two twelve-pound cartridges, a double barrelled gun, a hundred rounds of ball cartridge, and his own heavy sword and spear. So little, however, was he incommoded with this, that he stuffed the whole of the sergeant's kit on his back, and walked off as easily as if the whole load were but a feather weight."

The drawing on page 11 shows the lake dwellings of the tribes inhabiting the Delta of the Orinoco, as described by Humboldt in his "Personal Narrative." "The large tract of land that forms the Delta of the Orinoco (we quote from Mr. Wood) possesses some very remarkable characteristics. It is always wet, but during several months in the year it is completely inundated, the river rising to an astonishing height, and covering with water a tract nearly half as large as England. This seems to be as unpropitious a spot as could be adopted for human habitation, and yet the Waraus (or Guarápos, as Humboldt spells the word) have established themselves there, and prefer it to any other locality, probably because their strange mode of life enables them to pass an existence of freedom. Varying much in the height to which it rises, in some places exceeding fifty feet, the Orinoco has the quality of rising year after year to the same height in the same place, so that when a mark is made to designate the height to which the water rose in one year, the same mark will answer year after year with scarcely the slightest deviation." Here the Itá palm thrives, which supplies to the Warau food, drink, clothing, and residence; for, selecting four that grow near each other in the form of a square, and cutting away any intervening trees, he makes deep notches in the trunks some three feet above high-water mark. In these notches are laid beams that are tightly lashed in their places by ropes made of Itá fibres. On these leaves are laid a number of cross pieces, usually composed of the gigantic stems of the leaves, then a layer of the beams themselves, and finally a smooth coating of mud, which soon dries under the tropical sun, forming a smooth, hard, and firm flooring, that will bear a fire without risk of damage to the wooden structure below. Ten or twelve feet above the floor the Warau constructs a roof of palm-leaves, the corners of which are supported by the same trees which uphold the house.

The extreme mechanical ingenuity of some uncivilised tribes, working with very imperfect implements, is perhaps nowhere better shown than in the drawing on the opposite page of an adze made by the inhabitants of Hervey Islands, and which also gives a very good idea of the excellence of the illustrations in Mr. Wood's work, and of which the following description is given:—

"The lower part of the handle is completely hollow, the native manufacturer having contrived to cut away the wood through the intervals of the upright pillars. As these intervals are not quite the third of an inch in width the labour of removing the interior part of the handle must have been very great, and the work exceedingly tedious. Even with European tools it would have been a difficult piece of workmanship, and the difficulty is greatly enhanced by the fact that the native who carved it had nothing but a sharp stone or a shark's tooth lashed to a handle by way of a knife. The head of the adze is made of stone, and is lashed to the handle in a way exactly like that which is employed by the New Zealanders, except that it is far more elaborate. As if desirous of giving himself as much trouble as possible, the maker has employed the finest possible sinnet, not wider than packthread, and quite flat. It is laid on as regularly as if wound by machinery, and the native artist has contrived to produce the most extraordinary effects with it, throwing the various portions into a simulated perspective, and making the lashing look as if there were four distinct layers one above the other."



THE MANGAIAN ADZE

We wish we had space to give the account of the activity of the Monkey Men of New Guinea; of the manipulative skill of the canoe-builders of Fiji, of the Zarabatana with their blow-gun, of the New Caledonian with his sling and javelin, of the extraordinary and cruel rites of the Mandans in the initiation of their youth into manhood, of the cruelty of the Tongans, and a hundred other details of interest; but our readers must refer to the work for themselves. We cannot conclude without a word of praise for the illustrations, which are extremely numerous, whilst many are original, and drawn from implements in Mr. Wood's own collection. We miss an index.

H. POWER

NOTES

IN stating that we believe that the English Eclipse Expedition is now finally arranged, it is due to the Government to add, and we do so with the greatest pleasure, that it is now quite clear, that only a small part of the blame, which certainly attaches to some one, can be laid at their doors. In fact, explanations certainly are due from those who have had the management of the now famous Joint Committee. It appears that a deputation was named, and accepted the trust of representing the requirements of Science to the First Lord of the Treasury, which trust they neither fulfilled nor handed back to the Committee in order that another deputation might be appointed. We next hear of a letter written to the wrong Government department; and last of all, we are informed that the letters of the Government department—the last, we believe, written not later than the beginning of September—stating, among other things, that an application should be made in October, when the possibility of granting ships could be better discussed—have not yet been brought before the Joint Committee, which has just been summoned by the Secretary for the 4th of November, that is, to-morrow. It is not for us to censure such conduct as this, but it is our clear duty to point it out, and we hope the matter will be taken up. In spite of this mismanagement, however, we hear that the Government are prepared to aid both by money and ships when an application shall be made, and we cannot doubt that application will be made. There is still ample time to organise an expedition which shall do much good work, though perhaps it is too late to send out and erect the largest class of instruments. Large instruments, however, will be in the hands of the members of the American Government Expedition, so that this is the less to be regretted.

WE have great pleasure in announcing that Prof. Wyville Thomson, F.R.S., has been appointed by the Crown successor to Prof. Allman in the chair of Natural History in the University of Edinburgh. A vacancy is thus caused in the chair of Natural History at Queen's College, Belfast, for which we understand there are already many candidates.

THE difficulty of providing funds for the establishment of a Professorship of Physical Science in the University of Cambridge has been overcome by the colleges, at a meeting of their heads, taking upon themselves a quota of the rates for improvements and other purposes in the town of Cambridge, which was formerly charged upon the University funds. This sum amounts roughly to more than twelve hundred pounds per annum; so that the University will speedily be able to avail itself of the munificent offer of the Duke of Devonshire, and will doubtless proceed at once to establish a Professorship of Physical Science, and obtain the other aids in the way of laboratory, apparatus, and assistants, that the Professor may require.

THE following notices of lectures this term in Cambridge show that there is great increase of activity in teaching the various branches of Natural Science in that university. Professor Liveing gives a course of lectures on the "Experimental Laws of Heat," and also gives instruction in practical chemistry in the University Laboratory three days in the week. Professor Humphry gives a course on "Practical Anatomy," also a course on "Anatomy and Physiology," and connects with these a "Microscopical Demonstration" once a fortnight, and instruction in "Practical Histology" once a week. Professor Newton gives a course on "Zoology and Comparative Anatomy." Professor Willis gives a course on "Mechanics and Mechanism," and their application to "Manufacturing Processes" and the "Steam-engine." Professor Miller gives a course on "Elementary Crystallography and Weighing." Professor Sedgwick gives a course on "Geology." In Downing College, Dr. Bradbury lectures on "Comparative Anatomy," and Mr. Danby on "Geology." In Trinity, Mr.

Trotter lectures on "Electricity," Dr. Michael Foster on "Physiology," and Mr. Trotter on "Elementary Botany." In St. John's, Mr. Main lectures on "Chemistry," and gives practical instruction in the College Laboratory; and Mr. Bonney lectures on "Geology." Most of these College lectures are open to all the students of the University.

The Natural Science Demyship of 75*l.* per annum for five years, at Magdalen College, Oxford, which was not awarded at the last examination, is announced for open competition in March next. Further particulars will be sent on application to the College.

The splendid Physical Laboratory lately built at Oxford is opened this term for practical instruction in Physics, under the superintendence of Professor R. B. Clifton, F.R.S., assisted by two demonstrators.

A chemical society has been established at Zurich under the presidency of Dr. Wislicenus.

We regret to have to announce the death, on October 26, of Dr. Thomas Anderson, Superintendent of the Calcutta Botanic Gardens. Dr. Anderson had greatly assisted in the establishment of the Cinchona plantations in British India, was the author of a large number of papers on botanical subjects, and at the time of his death (on sick leave in this country) was engaged, in conjunction with Dr. Hooker and Dr. Thomson, on the new Flora of India.

The death is announced of the Rev. F. Bancks Falkner, late head-master of Appleby Grammar School, a gentleman well known from his numerous and elaborate meteorological reports and letters, which mostly appeared in the pages of the *Standard* and *Scientific Opinion*.

At the first of the winter *soirées* to be held on Monday evening, November 14, at 8 o'clock, at 27, Harley Street, Mr. A. R. Wallace will read "An Answer to Dr. Hume, Lecky, and others, against Miracles." A discussion will be invited.

The following officers are proposed for election by the Council of the London Mathematical Society at the general meeting to be held on Tuesday evening next:—President, W. Spottiswoode; Vice-presidents, Prof. Cayley, Prof. Henrici, Prof. H. J. S. Smith; Treasurer, Prof. Hirst; Secretaries, H. Jenkins, R. Tucker.

The fourth opening *conversazione* of the Hackney Scientific Association took place on the 25th October, at the Meeting Rooms, New Tabernacle, Old Street Road, E.C. The objects exhibited were lent entirely by the members and some friends, and comprised an unusually fine display of microscopes of the most powerful description. Geology was well represented by a very rare collection of fossil mammalian teeth from the Freshwater Post-tertiary beds in the Lea Valley, exhibited by Mr. R. E. Olliver, also a fine series of Coal-measure fossils by Mr. W. Appleford. Astronomy was unusually well represented by numerous objects, amongst which were two refractor telescopes, exhibiting convenient forms of mounting, also numerous sketches by Mr. W. R. Birt, exhibiting the most recent progress in selenography. We must not omit to notice a numerous collection of astronomical drawings, by Mr. A. P. Holden, amongst which were four very fine sketches of the recent great sun-spot.

A Natural History Society was established a year ago in King Edward VI. Grammar School, Birmingham, and is now in active operation. A school collection has been commenced, with a view to a future museum.

The Special Commissioners appointed for the purposes of the Public Schools Act, 1868, in virtue of the powers conferred upon them by that Act, have made five statutes for determining and establishing the constitution of the New Governing Bodies of the Schools of Winchester, Harrow, Rugby, Shrewsbury, and Charterhouse. These statutes have been laid before Her Majesty in Council, and are published in the *London Gazette* of Tuesday, Oct. 25. Notice is given that it is lawful for the bodies or persons authorised so to do in that Act, within two months from the date of the publication of this notification, to petition Her Majesty in Council to withhold her approval from the whole or any part of such statutes.

As the subject of hereditary deformities is attracting some attention in our columns, it may be worth while to call attention to Brown-Séquard's experiments on epileptic guinea-pigs detailed at the recent meeting of the British Association. Dr. Brown-Séquard produced epileptic fits in the guinea-pigs either by the section of one-half of the spinal cord, or by the division of the sciatic nerve on one or both sides. During the fits it sometimes happens that the hind foot gets between the teeth, and is bitten. The animal, on recovery from the fit, tastes the blood, and if it be one in which the sciatic nerve has been divided, proceeds to nibble off the two outer toes, which have entirely lost their sensibility from the operation on the nerve. In breeding from pairs of this kind, the offspring is without the two toes of which the parents have deprived themselves; and in these cases all the offspring become, as they grow up, perfectly epileptic; while in ordinary cases epilepsy is only rarely transmitted hereditarily. Other peculiarities existing in these epileptic guinea-pigs were also found to be transmitted to their offspring; and in dissection of the hereditarily malformed animals, a node was found on the sciatic nerve corresponding to that formed after section of the nerve in the parent.

It may interest some readers to know that the Board of Trinity College, Dublin, have expended about 2,000*l.* in draining the College park. Situate as the park is, in the very centre of the city, its drainage will confer a considerable benefit on the city. Dublin will also be much improved by the space of ground which the College authorities are giving to the city in College Green, as well as ornamented by the new cut stone wall and handsome iron railings which take the place of the old irregular quadrangle in front of the College. The College share of the expenses amounts to 2,000*l.*

We have received the half-yearly Report of the Marlborough College Natural History Society, from which it appears that the Society has entered on the seventh year of its existence. During these years it has undoubtedly greatly strengthened the love of Natural Science among the *alumni* of the College, an evidence of which is the publication during the last half-year of "The Birds of Marlborough" by an old member of the Society, E. F. im Thurn, a little volume for which we would wish a greatly increased circulation. The officers regret that the number of members of the Society is not yet commensurate with the number in the College who take an interest in Natural History. In no more practical way can the love of a study of nature be fostered than by the encouragement of societies similar to this.

It is stated that the Botanical Gardens at Strasburg were used during the siege as a burying-ground.

The twentieth part of the late Prof. Schnizlein's "Iconographia familiarum naturalium regni vegetabilis," is published under the superintendence of Dr. Eichler, of Munich. This magnificent work, commenced seven and twenty years since, is now completed in twenty parts.

As Colombia in the last year exported about 3,500,000lbs. of cinchona bark, valued at 87,000*l.*, the Government there naturally looks with some interest on the trade, and is desirous to improve it. It is curious to see in the official report of the Secretary of the Treasury a detailed account of the successful measures of our Government for the cultivation of cinchona in India.

AN earthquake was felt generally in the Natal colony and the Orange River Free State, about 3 45 P.M. on the 3rd August. It did no material damage. It was recorded at Bloem Fontein, in the Free State, and at Pietermaritzburg, Durban, New Guelderland, Ladismith, Noodsberg. The course appears to have been from N.W. to S.E.

THE three most recent parts of the *Bulletin* of the Imperial Academy of Sciences of St. Petersburg contain the following articles:—Quelques propriétés du fer déposé par la voie galvanique, R. Lenz; Rapport sur un voyage entrepris dans l'intérêt de la linguistique, F. J. Wiedemann; Remarques sur les Echinoderes, El. Metschnikoff; Sur le poil du *Rhinoceros tichorinus*, J. F. Brandt; Sur less congruences binômes exponentielles à base 3 et sur plusieurs nouveaux théorèmes relatifs aux résidus et aux racines primitives, V. Bouniakowsky; Note relative à une demonstration, donnée par Cauchy, des équations générales de l'équilibre, J. Somoff; Sur les sentences de Publilius Syrus, A. Nauck; La Métrique pâlie Vuttodaya, J. Minayeff; Sur l'histoire composée en arménien par Thoma Ardzrouni, X^e s., M. Brosset; Sur un théorème relatif à la théorie des résidus et de son application à la démonstration de la loi de reciprocité de deux nombres premiers, V. Bouniakowsky; Remarques et rectifications concernant l'histoire naturelle des Alcides, J. F. Brandt; Sur le symbole de Legendre $\left(\frac{a}{p}\right)$, V. Bouniakowsky; Embryologie du *Phthirius pubis.* (avec une Planche.), Os. Grimm; De l'influence de la chaleur sur l'élasticité du caoutchouk, J. Schmulewitsch; Notice sur Ak-tau et Kara-tau, montagnes dans la presqu'ile de Mangyschlak, côte orientale de la mer Caspienne, G. V. Helmersen; Sur les dérivés de la série isocaprine, A. Borodin; Détermination du coefficient constant de la précession au moyen d'étoiles de faible éclat, M. M. Nyrén.

A RIVAL to the far-famed Mont Cenis tunnel is announced from America. The tunnel through the Hoosac Mountain, on the Troy and Greenfield Railway, is steadily progressing, and has now overcome the great difficulties with which it started. It is 4¾ miles in length (more than half that of Mont Cenis), and of this distance about one-third is already penetrated. The work is actively proceeding night and day from both ends, and it is expected it will be finished before the expiration of the contract in 1874.

FURTHER favourable reports of ipecacuanha cultivation in India have been received. The Conservator of Forests states that the plants in the gardens at Nelamboor are doing well, and that some of the fleshy leaves were four inches long.

A UNIVERSITY FOR TEXAS

THE Rev. W. H. Sent has been for some time past in Europe, with letters from the late President of the United States, the Governor of Texas, and other distinguished Americans, as the agent of the Soule University and the Chappell Hill Female College, two literary institutions located at Chappell Hill, Washington county, in the State of Texas. His aim is so to enlarge and furnish them, especially the University, which has a medical department in Galveston, as to develop a great and permanent centre of learning and science. Considering the vastness and the resources of Texas, its position as bordering on semi-civilised regions beyond, the rapid increase of its population, which includes a large European element, and its growing commercial relations with Europe, especially with this country, such an enterprise as this must commend itself as one of great importance, and of general interest.

The degree of success that has attended the agency is, indeed, extraordinary. Mr. Sent received contributions of books, specimens, &c., from various departments of the United States Government, from Prof. Agassiz, Yale College, the Smithsonian Institution, and other eminent sources in America; and in Europe from the Emperor, the French Government, the Jardin des Plantes, the great French authors, &c., and the same line of success was continued in Belgium, Holland, Denmark, the various German States, and elsewhere. We are glad to know that he is meeting with encouragement in London from the Admiralty, the Geological Survey, the University of London, from many of our publishing houses and other sources. The agent is applying to many of our Learned and Scientific Societies for their transactions, which will, we doubt not, be cheerfully contributed. The agency involves a patient, persevering effort to accomplish a very important work. We most heartily commend the enterprise to the friends of education in this country.

Any contributions of books, or of botanical, geological, or mineralogical specimens may be sent (with a statement of the sources whence they come) to care of Messrs. Trubner and Co., 60, Paternoster Row, London, or to Messrs. Caleb Grimshaw and Co., Liverpool.

EARTH CURRENTS

IT has been my pleasure at different times to call attention to the connection that exists between the Auroræ Boreales and that great apparent rush of electricity through the crust of the earth which eagerly seizes upon the easy paths, offered to its passage by the wires of the telegraph, and by filling them with electricity, produces what are called "earth currents," or "deflections." The aurora is always accompanied by such displays, but it is rarely in England that they are of such strength as absolutely to break down telegraph communication. The earth currents of Oct. 24 and 25 have only been equalled by those which occurred in 1859.

The following extracts from the diary of one of the large telegraph stations in the South of England will be found interesting:—

Oct. 24,	5.0 P.M.	Slight deflections on all long circuits.
"	5.30 "	Gradually increasing.
"	6.0 "	Very strong; circuits suspended for ten minutes.
"	7.0 "	Gradually decreasing.
"	8.15 "	All circuits right.
Oct. 25,	3.0 "	Deflections, which have been intermittent all day, or very strong.
"	3.30 "	Circuits nearly all stopped.
"	4.0 "	Working through on some circuits, but slow.
"	5.0 "	Deflections decreasing.
"	5.45 "	On again; all circuits suspended.
"	6.15 "	Deflections decreasing again.
"	7.0 "	Circuits clear.

This is only a sample of what occurred simultaneously all over England, and probably the globe. The currents were very irregular in their direction and very variable in their strength. Circuits running S.W. to N.E. are usually most powerfully affected, but on this occasion all directions seemed equally affected.

Where two or more wires run between two stations, the effect of these currents upon the working is easily remedied by substituting the second wire for the earth to complete the circuit. This practice was largely adopted on Monday and Tuesday last.

The most striking fact observed was that on each occasion the currents *ceased* when the auroral display *commenced*. I have not noticed this before, probably because the cessation of the one phenomenon and the first appearance of the other have scarcely ever before been so strongly indicated.

W. H. PREECE

DR. C. W. GÜMBEL ON DEEP-SEA MUD

DR. C. W. GUMBEL has recently published an important paper, containing an account of some highly interesting investigations on Deep-sea Mud. Sir R. Murchison and Professor Huxley provided him with a large quantity of mud, taken up from the Atlantic at lat. 29° 36′ 54″ N., and long. 18° 19′ 48″ W., at a depth of about 2,350 fathoms. This he first cleared, by long-continued washing, from all sea-salts soluble in water; then he divided it, by filtering, into three parts. In the first Foraminifera and larger organisms predominated; the second consisted of a sediment easily distinguished from the first, fine but heavy; the third was fine and flaky, remaining lightly suspended in water, and consisting almost exclusively of *Bathybius*, Coccoliths, Coccospheres, together with other organisms of the smallest kind (Diatoms, Radiolaria, Sponge-spicules, and a very few of the smallest Foraminifera). "Dried to about 100° C." says Dr. Gumbel, "10 per cent. of the mud consisted of large Foraminifera; 1·3 per cent. of fine, heavy mud, and 88·7 per cent. of finest *Bathybius* mud. The 10 per cent. part consisted mostly of *Globigerina*, which occurred in an astonishing variety of forms, from the smallest shapes to figures of a considerable size, and could easily be distinguished as *Gl. bulloides* and *Gl. inflata*. Next to these in number were *Orbulina universa*, *Cristellaria crepidula*, *Truncatulina lobatula*, *Discorbina rosacea*, *Rotalia soldanii*, *R. orbicularis*, *Pulvinulina elegans*; *P. micheliana*, *Nonionina umbilicata*, *Polystomella crispa*, *Lituola globigeriniformis*, with many other (but more dismembered) species. Along with these there were individual specimens of large *Radiolaria*, Siliceous Sponge-spicules, Diatoms, shells of *Ostracoda*, torn pieces of sponge and (very rarely) of *Echinodermata*, and fragments of wood, which were very decidedly distinguishable. It is a question whether the latter was a part of the apparatus used in raising the mud. It is in the highest degree remarkable that all traces of Bryozoa, corals, and firm pieces of more highly-organised animals, were wanting, or at least were very rare.

"The fine heavier mud which composes the sediment contains, for the most part, inorganic elements, with fragments which consist essentially of carbonate of lime, and which, on being dissolved in acids, leave behind cuticular membranes and flakes, which partly gave the reaction of conchiolin. It appears to follow from this, that these pieces of calcareous matter, although I could discover by the microscope no structure in them, are essentially derived from pounded molluscous shells. The remaining portion, which was insoluble in diluted acids, was composed of irregular, for the most part lump-shaped, granules of quartz, of clearly recognisable scales of mica, of dust, and of magnetic iron, which could be drawn out by the magnetic needle; of single red, blue, and dark green transparent pieces of mineral; and of grains of crystal, of a peculiarly dark irridescent brilliance, which I can refer only to Labradorite. The polarisation and stauroscope apparatus was used for the purpose of determining these inorganic ingredients.

"These inorganic elements of the Deep-sea Mud, found at such a distance from land, appeared to me worthy of the greatest consideration. Their origin can scarcely be ascribed to the loosening of the perhaps rocky bottom of the sea, at the point where it was sounded. They rather prove that inorganic substances, which are derived from the rocky masses of the land by mechanical destruction, are conveyed by ocean currents to parts of the sea the farthest removed from land. This would render easily explicable the admixture of inorganic elements in many ocean sediments of ancient times. The explanation of clayey or marly interpositions would be made much less difficult. If heavy masses of mineral are transported so far, how much more easy would be the transportation of clayey mud which remains so lightly suspended in the water! It is almost self-evident how quantities of clay or marl may be brought to a stand at certain parts of the high sea, marked out beforehand by the direction of the ocean currents and the configuration of the bottom of the sea, and when the direction of the currents changes, may come to form even alternate strata of chalk and marl. We thus obtain a mode of explaining the formation of many marl deposits, which is at once natural and simple.

"The third portion of the Deep-sea Mud is worthy in a high degree of the interest both of the zoologist and the geologist, whilst it gives scope for many far-reaching theories. If we first analyse it microscopically, the substance, which resembles a white clay mud, resolves itself, apart from the intermingled minutest *Globigerinæ* and some few other Foraminifera, into a heap of little granules, the so-called Coccoliths (Discoliths and Cyatholiths), and of granulous flaky little lumps, the so-called *Bathybius*, compared with which all other ingredients,—the siliceous-shelled Diatoms, and *Radiolaria*, and also perhaps the so-called Coccospheres and other small organic bodies excepted,—are of very secondary importance.

"The part of the Deep-sea Mud which is made up of Diatoms and *Radiolaria*, together with Sponge spicules, is of especial importance, because it consists to no inconsiderable extent of silica, and appears to be the source from which the siliceous concretions in many chalk formations have drawn their materials. That these form no inconsiderable part of the composition of Deep-sea Mud may be clearly seen by removing the chalk by means of acids, and the organic matter by heat or by sulphuric acid. There then become visible the most beautiful forms of Diatoms, with especial frequency, *Gallionellæ*, *Coscinodisci*, and *Naviculæ*, more rarely *Actinocycli*, *Pleurosigma*, *Rhabdonema*, *Grammatophora*, and others, of which many, concealed in the network of granulous *Bathybius* masses, were formerly scarcely visible. Many forms of extremely beautiful *Radiolaria* were also seen, together with simple Sponge-spicules. Lastly, we remark some slight fragments of plants, which may belong to the species of *Saprolegnia* and *Protococcus*."

Speaking of the Coccoliths and the *Bathybius*, Dr. Gumbel says he is in a position to confirm the conclusions of Profs. Huxley, Carpenter, and Haeckel with respect to their organic nature. In a note he adds, "I have already stated my opinion on this subject (NATURE, April 1870) but must here rectify a mistake in that communication, namely, that the organic matter of the Coccoliths yields with iodine, blue, therefore cellulose, reaction. This colouring, I am now convinced, is not the consequence of chemical action, but a phenomenon of refracted light, such as occurs with small thin leaves or membranes when greatly magnified."

After detailing some observations, microscopic and chemical, on *Bathybius* and Coccoliths, Dr. Gumbel proceeds to speak of the further distribution of the latter. "First," he says, "on looking through the Algæ, Hydrozoa, Polyps, Corals, &c., which occur on shallow sea-coasts, such as may easily be met with in every botanical and zoological collection, I succeeded in numerous instances in finding Coccoliths in the places where they had grown, and not seldom, *Buthybius* at the same time. These investigations were extended to points on the coasts of almost all seas; and now, instead of the statement lately made that the organisms in question thrive only at a depth of 5,000 feet, I am in a position to assert on a proved fact, that Coccoliths (*Bathybius*) occur *in all seas and at all depths*. This deprives these minute bodies of a certain air of wonder with which they were surrounded, as the offspring of the profoundest and most secret depths of the ocean; but by their astonishingly wide distribution and their vast numbers, which stamp them as one of the most essential members of rock-forming substances, they gain infinitely in scientific interest."

Dr. Gumbel maintains that the distribution of Coccoliths in time is not less remarkable than the present distribution in space. There is proof, he says, that they are to be found in "almost all sedimentary formations" Referring to their distribution in various formations, he says:—"But besides the Coccoliths another ingredient demands attention. In the case of the chalk of Meudon, rich in Coccoliths, if the carbonate of lime be removed by means of diluted acids, there remains a flaky and cuticular residue, in which are found thin, transparent flakes, full of the smallest granules, and resembling *Bathybius* in a high degree. . . . . This places their organic nature beyond question, and firmly establishes their relationship with the *Bathybius*. The imperishableness of this substance is indeed very remarkable." After stating that the Coccoliths occur in all the soft marls and limestones of the Jurassic and Liassic formations—"The Muschelkalk," continues Dr. Gumbel, "appeared for a long time to be proof against every experiment. Every specimen of marl which I examined was apparently free from Coccoliths. At last I had the good fortune to discover traces of them in a somewhat impure piece of rock-salt from Wilhelmsgluck. Even here they show themselves extremely sparingly, but in the company of flakes, which are not unlike *Bathybius*. To the present time I have in vain examined the similar rock-salts of Berchtesgaden and Stassfurt; and as yet indications of Coccoliths in the Permian formation and the Coal-measures are wanting. On the other hand, the soft marls of the mountain limestone of Regnitzlosau, the soft marls of the Conodont strata of the Baltic provinces, the Trenton marl of New York, and even the siliceous limestone of the Potsdam sandstone, contain some traces, although to an extremely small extent.

"These facts all point to the conclusion that in the majority of calcareous marine deposits, the Coccoliths originally formed a more or less essential part of the calcareous masses, and that in thick or granulous, and particularly ancient limestone rocks, they can no longer be perceived, either on account of the opaque character of the rocks, or because they have been made by some change wholly or in part unrecognisable, or have been altogether destroyed. I have been able by some experiments to throw further light upon this subject. That these smallest organic bodies can be recognised in hard limestones only in the rarest cases, even when it contains them in great numbers, I convinced myself by means of thin slices, which I made from Deep-sea Mud, thoroughly dried and rendered hard by repeated soaking in diluted Canada balsam and by heating, and also from writing chalk, made hard in the same way, and rich in Coccoliths. The infinite numbers of finest granules and rings are so massed together, one over the other, that it must be regarded as an extremely rare case when a Coccolith is clearly seen here and there at the very thinnest edges."

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

Section A.—Mathematical and Physical Science

On a new Electro-Magnetic Anemometer and the Mode of Using it in Registering the Velocity and Pressure of the Wind.—Mr. J. J. Hall. The anemometer consists of two parts, viz.—velocity apparatus and registering apparatus. The first consists of a set of Robinson's hemispherical cups, which communicate their motion downwards into a brass box, where it is reduced in angular velocity, and causes a contact disc or *commutator* (in which two platinum contact pins are fixed equidistant from one another) to revolve in $\frac{1}{10}$th mile. An insulated metallic lever, having a platinum working face, stands on either side of the disc, so that upon the completion of every $\frac{1}{500}$th mile one or other of the contact pins comes in contact with the two levers, thus uniting them and completing the circuit. The levers are raised a few degrees and then fall back to their normal position ready to be taken up by the next pin, and so on. The recording apparatus consists of a train of wheels and pinions working in a frame or between two brass plates, the arbors of which project through a dial-plate whereon the circles and figures are engraved and carry the hands. These wheels are driven by a weight attached to a line wound round a barrel, and a locking-pin disc (the pinion of which works in the first wheel) is released at every contact of the cup-apparatus by an electro-magnet which unlocks the pin-disc and allows the first hand to advance $\frac{1}{500}$th mile on the graduated dial by a jump similar to the minute hands in remontoire clocks. By turning on a "strike-silent" stop a hammer lever is brought into connection with the escapement and strikes a bell at every contact. By this arrangement the observer has nothing to do but to notice the seconds-hand of his watch or chronometer while he counts the number of times that the bell is struck, each of which corresponds to the five-hundredth part of a mile, and by a formula arranged (and exhibited) by Mr. Hall (who has also arranged a comprehensive series of tables for use with this instrument) the hourly velocity may be readily deduced. In noting velocities extending over long periods of time, the instrument is read in the same manner as the ordinary cup and dial anemometer, or as a gas meter. By means of the formula before mentioned (although the unit of measurement in this instrument is five-hundredths) the observer may arrive at results as near the truth as if the instrument were capable of registering the one-thousandth part of a mile, while the great advantage lies in the fact that the battery power is less called into action, from which we may infer its elemental duration will be considerably longer.

A Magnetic Paradox.—Mr. S. Alfred Varley, Assoc. Inst. C.E. The author stated he had termed the instrument a Magnetic Paradox because the phenomenon exhibited by it was the apparent repulsion of soft iron by a magnet. The apparatus consisted of a compound magnet in a box, and when pieces of soft iron were placed on the box over the poles they became magnetic by induction and were attracted by the magnet; but if a soft iron bar not by itself magnetic was approached near to the pieces of iron, they leapt away from the magnet in the box and became strongly attached to the soft iron bar, the pieces of iron appearing to be repelled by the magnet and attracted by the iron bar. The author stated the explanation demonstrated the duality of the magnetic force, and it would also prove, did we not already know it, that magnetic force was transmitted only by induction. He stated that if a piece of soft iron were placed over the poles of a magnet, the magnet developes the magnetic forces resident in the iron by separating them, and the iron is attracted only by virtue of the forces existing in the iron itself, and to the extent to which the forces are separated. If the magnet be bent, bringing the lower pole round and over the piece of soft iron, the magnetic forces resident in the soft iron will be more developed; but if the piece of soft iron be midway, it will not be attracted, as the forces on either side are equal and balance; another attraction will, however, be manifested if one pole be nearer to the piece of iron than the other. If, instead of bending the magnet as just described, the piece of soft iron placed over the magnet be approached by a soft iron bar, the magnetic forces separated and rendered active in the piece of iron will develop the magnetic forces resident in the iron bar, and if the bar opposed no resistance to the assumption of the magnetic condition, it would exert an attractive force for the piece of soft iron equal to that exerted by the magnet, provided always that the bar was at the same distance. It was stated that as the mass of iron in the iron bar was much greater than that of the piece of soft iron, the resistance opposed by the bar to polarisation was comparatively small, and might be disregarded, and consequently it followed that as the dual forces resident in iron are equal, and the one force cannot be developed without equally developing the other; when the iron bar was approached nearer to the piece of soft iron it became attracted, leaping away from the magnet and attaching itself to the iron bar, and this notwithstanding that the attractive force exhibited by the iron bar has been called into being by the magnet in the box, which is nearer to the piece of soft iron than it is to the iron bar. The iron bar also collected the magnetic rays of force issuing from the magnets, and consequently it exerted a greater attraction for the piece of soft iron than any individual magnet forming part of the compound magnet. This was shown by placing a piece of soft-iron on the pole of one of the magnets and removing it from the pole by the superior attractive force of the iron bar. It was also shown that if only the thickness of a piece of writing-paper were placed between the magnets and the piece of soft iron, the appearance of repulsion could be prevented.

Section B.—Chemical Science

On the Separation from Iron Furnace Cinder of Phosphoric Acid for Manurial Purposes.—Mr. James Hargreaves. While the author was engaged in an attempt to produce a good serviceable steel direct from phosphoric pig-iron, by the use of nitrate of soda, the fact forced itself upon his attention that phosphorus had previously been too much looked upon as something to be *got rid of*, and not sufficiently as something to be *got hold of*; and that to effect the latter would be the best means of effecting the former. When phosphoric pig-iron is converted into malleable iron, the phosphorus is, in great part, transferred to the refinery and puddling furnace cinder in the form of phosphate of iron, the amount varying with the composition of the pig-iron which yields it. The refining and puddling furnace cinder from Cleveland pig-iron generally contains from 3 to 7 per cent. of phosphoric acid, which is from one-fourth to one-half the amount contained in good commercial soluble phosphate of lime. This cinder is sometimes again used for the manufacture of pig-iron, but the product is of small commercial value on account of the accumulation of phosphorus in it. The concentration of the phosphorus from the pig-iron into the cinder in the form of phosphate of iron renders it more easy and practicable to separate, when the preparation of compounds of phosphoric acid is the object in view, as there is a smaller bulk of material to be treated to obtain a given amount of this product. The phosphoric acid may be separated either in the form of soluble superphosphates of lime and magnesia, or of the alkaline tribasic phosphates.

On the Retention of Organic Nitrogen by Charcoal.—Mr. Edward C. C. Stanford, F.C.S. In this paper the author submitted some incomplete researches, as a first instalment of what promises to be a wide field of inquiry; viz., the action of charcoal on organic nitrogenous matter. He was desirous of knowing whether or not a loss of nitrogen occurs when that form of matter remains in contact with charcoal, and if so, what becomes of it. If any loss occurs, it would invalidate the process recommended by the author at the Exeter meeting of the Association, viz., that of using charcoal as a means of securing the whole value of town excreta. He said that he had shown last year that, with either fluid or solid excreta, there was no loss, as far as his experiments had then extended; and he had pointed out, also, that he expected no loss from oxidation, as both must already be regarded as oxidised compounds. His experiments since had extended over a long period, and he had included meat as one of the nitrogenous matters used; in all, however, he had found no loss of nitrogen, no oxidation, and no formation of nitrates.

Section C.—Geology

On a Census of the Marine Invertebrated Fauna of the Lias.—Mr R. Tate. The author gave an analysis of the fossils, but desiderated more precise data before exact results could be obtained.

On the Formation of Boulder Clays and Alternations of Level of Land and Water.—Rev. J Gunn. The author illustrated his own opinions, which were completely at variance with the generally accepted interpretation of the origin of these beds.

On Some Cases of the Recent Conversion of Glacial Drifts into what Appears to be Middle Drift.—Mr. G. J. Stoney.

On the Occurrence of Pebbles and Boulders of Granite in Schistose Rocks in Islay, Scotland.—Mr J. Thomson. The author described the different rocks exhibited in a section across Islay from west to east, and the position of the metamorphic rock in which the boulders occurred which underlies a bed of quartzite seventy feet thick. Specimens of some of the smaller boulders with their interesting matrix still attached to them were exhibited. The bed probably indicated one of those recurring glacial epochs which had formed the subject of Mr. Wallace's communication to the section.

Diamonds of South Africa.—Professor Tennant.

Changes of Climate.—Mr R. A. Peacock. These were due, according to the author, to rain and rivers, to denudations, to risings and sinkings of land, and to the great range of temperature in interplanetary space and on the various parts of the earth's surface. The warm, genial climate of the Carboniferous period he ascribed to the absence of high hills at that time on the globe.

Sur le terrain Silurien du centre de la Belgique.—Professor Malaise. The author described the series of beds with their fossil remains, and considered that they represented a portion of the Middle Silurians, more extensively developed in Belgium than in Britain.

On the Remains of an Insect discovered in the Carboniferous beds at Huyton.—Mr Clementshaw exhibited the specimen of the insect, and pointed out the characters upon which he ventured to refer it to the *Fulgoridæ.*

Notes on a Merionethshire Gold Quartz Crystal, and some Gold found recently in the River Mawddach.—Mr. T. A. Readwin.

Section D.—Biology

Department of Anatomy and Physiology

On the Connection of the Hyoid Arch with the Cranium.—Prof. W. H. Flower, F.R.S. In the sheep, as is well known, the anterior arch or cornu of the hyoidean apparatus is described as consisting of three bones, of which the uppermost is by far the largest and most important, and has received the name of stylo-hyal. This bone is long, compressed, and at the proximal end enlarges and divides into two short branches, by the anterior of which it is continued as a cartilaginous band to the cranium. The upper end of this band is again ossified in the form of a curved cylindrical plug of bone, with a truncated lower extremity, lying in a groove on the side of the tympanic bone, the edges of which groove meet around it in adult animals, and often become ankylosed with it; but this is only a secondary connection. The primary connection is with the periotic or petro-mastoid bone, immediately in front and to the inner side of the stylo-mastoid foramen. In embryonic specimens it can be traced as a distinct band of cartilage lying to the anterior and inner side of the lower end of the Fallopian aqueduct, and passing to the upper and back part of the tympanic cavity, near to the spot from which the stapedius muscle takes origin. This is then the true proximal extremity of the anterior arch of the hyoidian apparatus, if we leave out of consideration the stapedius and incus which there is reason to believe are developed from the same rod of cartilage—a question which is not discussed in the present communication. Whatever may be the case with regard to the origin of the last named parts, it is a subject of easy demonstration that in the sheep there is an ossified portion of the upper end of the hyoid arch, above and distinct from the stylo-hyal, which becomes firmly united with the periotic, and which may ossify either from a separate centre or by extension of bone from the periotic. Whether it should be considered as a process of the periotic or as a separate element may still be a matter of opinion; but the existence of such a part as a distinct portion of the hyoid arch requires recognition. It may be conveniently distinguished by the name of *tympano-hyal*, as it is always in relation with the tympanic bone, and continues the hyoid arch up to the wall of the cavity of the tympanum.

This portion of the skull can be distinctly recognised at the spot indicated (*i.e.* to the anterior and inner side of the stylo-mastoid foramen) in almost all mammals, though in very different degrees of development, usually in accordance with the size and amount of ossification of the remainder of the anterior arch. Thus, in those of the Ungulata, as the ruminants, and especially the horse and rhinoceros, in which the stylo-hyal is very largely developed, the tympano-hyal is most conspicuous, but where, as in the pig, the anterior arch is little ossified, the tympano-hyal is comparatively rudimentary. In the cetacea it is quite distinct, though small, and a fine band of cartilage can often be traced from the upper end of the stylo-hyal into it, embedded in the great ligamentous mass which attaches that bone to the exoccipital and surrounding parts of the cranium, and which of course is only a secondary connection

In man, this bone or process is also quite distinct, although it seems to have been generally confounded with the stylo-hyal The so-called styloid process of the temporal bone has long been known to have a separate centre of ossification, and is also generally recognised as the homologue of the stylo-hyal of other mammals, one of the main points of difference being, that whereas in all others it is an independent bone not connected directly with the cranium, in man it is always ankylosed to the "temporal," or forms a process of the skull.

If a human skull at the period of birth is examined, a very small round piece of bone surrounded by a deep groove can be seen exactly where the tympano-hyal is found in the sheep, just behind the posterior limb of the inverted arch formed by the tympanic bone, and in front and to the inner side of the stylo-mastoid foramen. This increases somewhat in size as age advances, forming a distinct process, supported, and partly embraced in front by the vaginal process of the tympanic. The true styloid or stylo-hyal at birth is a slender rod of cartilage, often partially ossified in the centre, and invested by a strong fibrous sheath, from which the stylo-hyoid, stylo-glossus, and stylo-pharyngeus muscles take origin. Though it occasionally becomes ankylosed in the adult with the tympano-hyal, as is the case with those skulls which have very long styloid processes, this does not occur so frequently as is described in most works on anatomy. In the large majority of skulls, before middle age, the stylo-hyal is free, and is commonly lost in maceration. The short process which is always present, and which is commonly considered as a rudimentary styloid process, is really a distinct portion of the hyoid arch, corresponding with the tympano hyal of the sheep.

The communication was illustrated by specimens and diagrams.

On the Correspondence between the Anterior and Posterior Extremity, and the Modifications of the Position of the Limbs in the higher Vertebrata—Professor W. H. Flower, F.R.S. This communication was chiefly devoted to an exposition, by means of specimens and diagrams, of the views held by most English anatomists of the serial homologies of the different bones of the extremities, founded upon comparison of the anterior, cephalic, or preaxial border of the one, in the primitive position, with the same border of the other, which leads to results opposed to the views of Wyman and other American anatomists, founded upon the principle of antero-posterior symmetry.

On Lefthandedness.—Dr. Pye-Smith. The author referred to the prevalence of this condition as an occasional variety as far back as tradition goes, and in various parts of the world. Like righthandedness, it should be regarded as a functional specialisation, not a structural transposition. That it does not depend on transposition of the viscera is proved by several cases; and also that it does not result from the abnormal origin of the subclavian artery, as referred to in a previous number of NATURE. Righthandedness is probably the immediate result of some structural difference between the two cerebral hemispheres. Gratiolet's statement, that the left hemisphere is earlier developed, is contradicted by Ecker, Vogt, and Callender; but Broca's, that it is normally heavier than the right, is confirmed by Dr. Boyd. The author then spoke of the possible truth of Brown-Séquard's theory of the left hemisphere presiding specially over animal, the right over organic functions. Normal aphasia with right hemiplegia was contrasted with cases cited from Dr. Ogle and Dr. Hughlings Jackson, of left-handed persons with left hemiplegia and aphasia. The primitive condition was probably one of perfect bilateral structural symmetry and ambidextrous function. The normal condition at present is the result of hereditarily transmitted specialisation of structure and functions, both the result of some advantage resulting to individuals using the right hand, eye, or foot, for the performance of more specialised functions than those of swimming, climbing, &c., such for instance as carrying weapons or nursing children Lefthandedness would then be explained as a more or less complete reversion to an ancestral condition. Right and Lefthandedness should, therefore, be compared with such deviation in function and structure as is observed for instance in the chelæ of the higher Crustacea, while transposition of viscera is to be classed with the reversed twist occasionally seen in the skull of *Pleuronectidæ* and the shells and entire bodies of Gasteropoda.

Professors Burdon Sanderson and S. Stricker read a paper on *A New Method of Studying the Capillary Circulation in Mammalia.* The circulation was studied in the omentum of a guinea-pig immersed in a solution of salt and water of a certain strength and temperature, the animal being thoroughly chloralised.

Contributions to the Migration Theory.—Richard Caton, M.D. This paper contained an account of a number of experiments on he capillary circulation of the frog, fish, and tadpole in reference o the interesting phenomenon of the migration of blood-coruscles out of the vessels. The opinion was expressed that this ccurrence was chiefly due to congestion, and also that there re grounds for considerable doubt as to whether it had any onnection with the suppurative process, as hitherto supposed to e the case This paper was read immediately after those of Dr. urdon Sanderson and Prof. Stricker, and the three were disussed together.

On the Antiseptic Treatment of Contagia as Illustrative of he Germ Theory of Disease—Mr Hope. The author gave ome valuable details as to his treatment of the rinderpest which oke out upon his experimental farm in Essex in 1867. The ajority of between 260 and 270 cows were attacked by that isease. He injected carbolic acid through either the mouth or ctum, and 111 of those cows so treated recovered. The reainder not so dealt with died or had to be slaughtered. He so argued that the chemical instead of the medicinal treatment contagion was much better both in respect to men and the wer animals. He also gave illustrative cases of scarlet fever, th the view of showing that the sipping of a very weak solution carbolic acid, sprinkling body, clothes, carpets, &c., was ghly beneficial in its effects.—Dr. Baylis, medical officer of alth, Birkenhead, agreed with the reader in his views regarding nderpest, but not entirely with his treatment of fever. He r. Baylis), speaking of the unsatisfactory manner in which that ject was treated by the British Association, expressed a hope t before next year's meeting they would institute some expeients as to the action of disinfectants.

Department of Ethnology and Anthropology

Dr. King read a paper *On Blight in Man, and the Animal d the Vegetable World.* Having defined the terms blight, tagion, and infection, the author proceeded to describe the s by which their presence could be traced, and enumerated various diseases which were supposed to be contagious or inious, referring incidentally to small-pox, which could not, in opinion, be averted by vaccination. He thought that disease the result of a local impurity of the atmosphere, and that reas a healthy person might be affected if he went to the lly impure spot, the party suffering could not convey it to her upon his removal to a different locality.

r. Hitchman read a paper on the *Anatomy of Intellect,* iling numerous physiological experiments in regard to the re of life and mind in man and animals. Mental phenomena, aintained, did not always imply the existence of brain, or alic ganglia, or of nerves conveying impressions to cerebral nisation at all. Mind is not invariably dependent upon a cular condition of brain—this organ being often sound in acute and chronic cases of insanity, the seat of disease being found, according to statistical observation, at home and abroad, in the alimentary canal, liver, uterus, spleen, heart, and lungs, in at least a moiety of all cases. This is true, even in the most severe mental affection known to the physician, paralytic dementia. The mental principle is not confined to brain molecules, but is equally contained in parts far distant from them, and is separable from the body, as mind, in a latent state, as well as an immaterial new individual. The whole mental organisation is specially operant independently of all molecular changes in ganglious and nervous cords, though the psychical mode of action is largely determined in the genus *hōmo* and higher forms of animal nature, by the modification of structure and physiological condition of each anatomical organ; both healthy and morbid changes show there is a certain point in the physical history of instinct and intelligence, at once and for ever fatal to the doctrine of Professor Tyndall, and other physicists, viz., that thought, sense, emotion, nay, every fact of consciousness, are due exclusively to molecular motions of brain.

On the Relation of the ancient Moabites to neighbouring Nations, as disclosed in the newly discovered Moabite Stone—Rev Dr. Ginsburgh. This stone was found as recently as the year 1868, during researches in Palestine; the inscription occupied 34 lines, and was written in a language which traced its origin to a date long prior to the Christian era. The translation looked like a chapter of the Bible; and when it was borne in mind that of 15 cities mentioned in the Old Testament, 11 were referred to on the stone, no one could doubt that the Moabites were in a far greater state of civilisation than was generally supposed. The inscription dated back as far as 900 years before Christ, and was, therefore, older than two-thirds of the Old Testament. As the result of careful study, he came to the conclusion that an organised Temple service was observed amongst the Jews out of Palestine, and that that service must have been very much akin to the service of the Moabites; that at a period 900 years before Christ, the word "Jehovah"—although subsequently avoided with so much persistency—was so often upon the lips of the Hebrew race, that it passed over to a neighbouring nation; that the simplicity of the language was a striking evidence of the advanced stage of civilisation of the Moabites, and that in prowess they were superior to the Jews.

The following papers were read relating to the Australians, their language, and mental characteristics The first paper was by Mr. C. S. Wake, and was entitled, *The Physical Characteristics of the Australian Aborigines;* the second was sent by Dr. Bleek, and was on *The Position of Australian Languages.* The author traced certain analogies between the several Australian languages, placing them all in Max Muller's great nomadic or Turanian class; and although the Australians have, with few exceptions, no grammatical distinctions of gender, the author does not think that this necessarily excludes them from the sex-denoting family. The use of suffixes in the Australian languages led him to infer that they have been derived from the more temperate zones Indeed, the nations using suffix-pronominal languages are found on the outskirts of the tropics, and in temperate and cold latitudes, while those speaking prefix-pronominal tongues are restricted to the tropics; and again, the suffix-pronominal class are addicted to sidereal worship, and the prefix-pronominal to ancestor worship. The author, however, carefully showed that the physical descent of a race by no means necessarily coincides with the descent of its language; and, in conclusion, the learned doctor expressed his belief, based on a study of the mythology and the present customs of the Australians, that these have degenerated from a higher state of civilisation. The third and concluding paper in this series was by Mr. C. S. Wake, and was on *The Mental Characteristics of the Australian Aborigines.*

SECTION G.—MECHANICAL SCIENCE

Rolling or Shaping Axles.—Mr. Alfred Bowater. This paper embraced a description of a new machine, existing in model, for the shaping of railway axles by rolling pressure Whereas by the method of using the steam hammer an axle required half an hour in shaping, this rolling process would effect it in a superior manner in two minutes. The rolled axle was not only superior in quality, but was more uniform in size, and could be produced much more cheaply. The machine consisted of three rollers, which were regulated so that they might gradually press closer together, thus reducing the diameter of the bar and extending its length until shaped to the size required. Axles of any length could be rolled by the machine, with collars at any part of the tyre. The rollers were geared to revolve all in the same direction, and their friction imparted motion to the axle. The rolling process would obviate those flaws in axles which occasionally caused appalling accidents on railways.

On a New Safety Lamp.—Mr. W. E. Teale. After detailing a number of the objectionable features of the various safety lamps now in use, the author proceeded to say that, with a view to remedy so far as possible the dangers arising from the insecurity of the present oil lamps, the Protector Colliery Lamp has been carefully and thoughtfully designed to combine safety and brilliancy of light with cleanliness and economy. It is made on the principle of the ordinary sponge or portable gas lamp, in which is used a liquid specially prepared by the inventors. The reservoir, or gas-holder, is then screwed to the top of an ordinary Stephenson or Clanny lamp, within which are fixed a pair of horizontal hinges, moving upwards only. On the wick tube of the lamp, and sliding over it, is an outer tube, having round its centre a circular horizontal flange. When the reservoir is screwed upwards into the top, this flange comes into contact with the hinges, raises them in passing, and allows them to fall beneath it when screwed home, so that by reversing the screw, and withdrawing the reservoir gradually from the top, the said hinges prevent the return of the said sliding tube, thereby forcing it over the wick-tube, and so diminishing, and ultimately extinguishing the light. It is therefore impossible for a naked light to become exposed after the lamp has once been adjusted. To render security doubly sure, a lock and stop are so placed that after the light is put out by the action of the screw, it is still impossible for the collier to withdraw the reservoir from the top, so as to re-light his lamp. The safety of the mine is further insured by the fact that the gauze is kept perfectly clean, and therefore no coal-dust can adhere to it, as in the old oil lamps. In regulating and reducing the light when testing for gas, which can be done with the greatest ease and certainty, no pricker is used or required, and another source of danger is avoided. This lamp burns freely with less ventilation than any now in use, and is much more sensitive to the presence or action of gas, while it is impossible for the miner to light his pipe from, or tamper with, the light in any manner. It gives much more light than that produced by the very finest oil; and as neither smoke nor soot is made by combustion, the glass and gauze are as clean and the light as good at the end of the day as when the miner goes down the pit, and this without the trouble and great loss of time necessary to keep an oil lamp properly trimmed. As compared with oil, the cost of burning the Protector Colliery Lamp is very small, six days of ten hours each, or sixty hours, being obtained at a cost of threepence, or less than one half the price of ordinary miners' candles, and one-third that of the usual oil.

On Ocean Telegraphy.—Captain Rowett. The object aimed at in his paper by Captain Rowett was to show the superiority of hemp over metallic cables. The author contended that hemp cables were much lighter and extremely enduring when submerged, and iron cables were quickly corroded by the action of the sea water. Various specimens of submerged cable were exhibited by the author in support of his views.

SCIENTIFIC SERIALS

Journal of the Chemical Society, September, 1870.—This number only contains two papers; the first, on Vapour Densities, by Mr. J. T. Brown, contains a short description of the methods that have been proposed for their determination, and the formulæ employed for calculating the results from the data obtained. This serves as an introduction to a series of elaborate tables intended to facilitate these complicated calculations. The tables are a sequel to some previously published by Mr. Brown (*Journ. Chem. Soc.* N.S. iv. 72), and it might be acceptable to many chemists if the author would collect these and other tables and publish them in a separate pamphlet. The other paper is an abstract of a memoir in the Philosophical Transactions for 1869, entitled "Researches on Vanadium," by Professor Roscoe. The author has obtained three vanadium chlorides, a tetrachloride $V\,Cl_4$, a trichloride $V\,Cl_3$, and a dichloride $V\,Cl_2$. The tetrachloride may be prepared by passing dry chlorine over the mononitride heated to redness, or by transmitting a mixture of chlorine and the vapour of vanadyl trichloride $V\,O\,Cl_3$ over red-hot sugar-charcoal. Its vapour density corresponds to the formula $V\,Cl_4$. The trichloride is a crystalline peach-blossom coloured compound, resembling chromium sesquichloride. It is not volatile in hydrogen, but when strongly heated in this gas it loses chlorine, the dichloride and finally the metal being obtained. It is produced by heating the tetrachloride, or by its slow decomposition at the ordinary temperature, or by passing its vapour with hydrogen through a red-hot tube. Vanadium dichloride is an apple-green crystalline body, prepared by transmitting the vapour of the tetrachloride with hydrogen through a tube heated to dull redness. The dichloride, when heated in hydrogen in a platinum boat, yields the metal in bright, greyish-white lustrous grains. The processes hitherto described for the preparation of the metal have been tried by the author without success. The chloride or nitride is placed in a platinum boat and heated in a porcelain tube, through which a current of pure hydrogen passes. The metal does not tarnish in the air at common temperatures, but burns with brilliant scintillations when thrown into a flame. When heated in air it oxidises, producing all the oxides $V_2\,O$, $V_2\,O_2$, $V_2\,O_3$, $V_2\,O_4$, and $V_2\,O_5$. It is not attacked by hydrochloric acid or dilute sulphuric acid. Hot strong sulphuric acid slowly dissolves it. It is violently oxidised by nitric acid, and slowly dissolved by hydrofluoric. The metal burns in chlorine, and when heated in nitrogen forms the mononitride.

DIARY

THURSDAY, NOVEMBER 3.

LINNEAN SOCIETY, at 8.—On the Fertilisation of Orchids and Asclepiads: Dr Mansel Weale.—On a Solitary Bee from South Africa: Dr. Mansel Weale.

CHEMICAL SOCIETY, at 8.—On the Analysis of Cast-iron. Mr A. H. Elliott.

MONDAY, NOVEMBER 7.

ROYAL INSTITUTION, at 2.—General Monthly Meeting.

LONDON INSTITUTION, at 4.—Chemistry: Prof. Odling.

TUESDAY, NOVEMBER 8.

ETHNOLOGICAL SOCIETY, at 8.—On the Kimmerian and Atlantean Races: Mr Hector McLean.—Note on the name "Aymara": Mr. C. R. Markham.—Reply to Mr. Markham's Note by Mr David Forbes.

WEDNESDAY, NOVEMBER 9

ROYAL MICROSCOPICAL SOCIETY, at 8.—Notes on the Minute Structure of certain Insect Scales. Mr. S. J. M'Intire

THURSDAY, NOVEMBER 10.

LONDON MATHEMATICAL SOCIETY, at 8.—General Meeting. Retiring President's Address. Sketch of recent researches upon quartic and quintic surfaces.

BOOKS RECEIVED

ENGLISH.—The Elements of Mechanism: T. M. Goodeve (Longmans).—Papers on the Great Pyramid. J. V. Day (Edmonston and Douglas)

FOREIGN.—(Through Williams and Norgate)—Jahrbuch der Empfindungen: Hirzel und Gretschel.—Geometrische See-proben. Dr Boetticher.—Ueber die Entwicklung und Verwendung der Warme. P. Tunner.—Archi für Ophthalmologie: Arlt, Donders, und von Graeffe.—Jahresbericht ube die Fortschritte der Chemie

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BEET-ROOT SUGAR
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  Dr Balfour Stewart's Opening Lecture at Owens College, Manchester.—Lieut.-Col STRANGE, F.R.S.
  The Aurora Borealis.—A. S. HERSCHEL, W. G. GIBBS, T. G. ELGER; H. R. PROCTOR, Rev. R. MAIN; J. J. HALL, C. POCKLINGTON; A. M. SMITH
  The Aurora o Sept 24.—G. M. DAWSON
  Hereditary Deformities.—W. FIELD
  The Cefn Reptile and the *Times*.—W. BOYD DAWKINS, F.R.S.
MAN AND NATURAL SELECTION. By A. R. WALLACE
THE NATURAL HISTORY OF MAN. By H. POWER, M.B. (*With Illustrations*)
NOTES
A UNIVERSITY FOR TEXAS
EARTH CURRENTS. By W. H. PREECE
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ERRATA.—Vol. ii., page 399, second column, line 18 from bottom, "Electrometer" read "Anemometer." Page 512, second column, lin from bottom, for "requirements" read "acquirements."

NATURE

THURSDAY, NOVEMBER 10, 1870

SCIENCE AND THE WORKING CLASSES

THE spread of scientific instruction among the labouring population is a subject of greater importance than a superficial consideration might allow to it. Our scientific work is at present done almost entirely by our middle class, and mainly by those who have had such an education as is afforded by our universities or technical schools, or by foreign colleges. Not only do we look in this direction almost exclusively for the scientific training of the next generation, but also for the greater part of the work actually done in the field. Were statistics obtainable, it would surprise outsiders to learn how large a proportion of the practical observations in Astronomy, Geology, or Natural History is accomplished by men, the greater number of whose working hours are spent in towns or in some totally uncongenial occupation, and who can only devote a few precious hours stolen from their rest, or their brief summer holiday, to those pursuits which they have done so much to encourage.

This ought not so to be. Every one of these urban lovers of Nature must have returned from his annual retreat in the country with the thought how much more he could do for Science, how much greater scope he would find for the exercise of a keen eye or a cunning hand, if only fortune had so far favoured him that the prime of his life could be spent far from the tumult and dust of cities. And yet we find that, as a rule, those who do live in the country are very slow in making use of their glorious opportunities. The class who spend their whole time in the midst of every varying phase of the phenomena of Nature, have, taken as a whole, contributed little to the advancement of Science. How many a rare fossil, whose determination would have thrown light on some of the occult problems of geology, has been smashed by the careless hammer of the quarryman! How many a phenomenon of animal or vegetable life, the recording of which might have forestalled the discoveries of a Darwin or a Wallace by half a century, has remained unnoticed by the field-labourer before whose vacant gaze alone it passed! How many a strange monster or exquisite unknown form of life, for the possession of which all the museums of Europe would have eagerly competed, has been passed with merely an exclamation of awe or wonder by the untutored fisherman or sailor! The wealth of scientific knowledge which has in this manner been lost is incalculable. Now and then a Hugh Miller rises from the ranks to the command of a brigade in Science; now and then a George Stephenson, invincible from the feeling of conscious power, fights against difficulties of which those in the middle class can have but a faint conception, and comes to the fore by the force of inherent genius; but these are but a few solitary landmarks in the midst of the dead level of the intellects of our rural population.

The cause of this failure is not far to seek. It is simply want of education,—education, in the first place, of the powers of observation, and in the second place of the reasoning faculties, by the use of which alone the observations can be utilised and made to subserve the ends of Science. Everyone who has had the opportunity must have been struck with the dormant condition of the habit of observation of our agricultural population. An adaptation of purpose to end, or a deviation from the ordinary course of the laws of Nature, which would at once strike the educated eye, does not for a moment arrest their attention. Even of occurrences which pass daily before their eyes they are profoundly ignorant. We were once asked by a very intelligent labourer, whose occupation took him abroad frequently by night, if we could explain how it was that the moon shifted her place among the stars so much in the day time, while she remained stationary all night! The first thing to do with the working man is to teach him how to make use of his eyes. And in this first elementary lesson we are afraid our teachers of Science have hitherto lamentably failed. The error of "popular" scientific lectures, of evenings with working men at mechanics' institutes, is that which is so commonly attributed to clergymen, that of speaking over the heads of their audience. It does not really profit your Hodge or Styles to be discoursed to for an hour about the wonders of astronomy, the uninterrupted chain of organic beings from the Amœba to the Elephant, or the grandeur of the uniformitarian theory of geology. Take him out on a starry night and let him look through a telescope, and see for himself that Saturn is a round orb suspended in the air. with its marvellous rings and its attendant satellites; take him where with his own hands he may exhume a shark's tooth from a chalk-pit, and show him how this absolutely proves that the spot on which he stands was once fathoms deep beneath the ocean; put a flower in his hand, and point out the structure and the function of every organ, and you will at least have made a beginning. You cannot be too simple or practical. Treated in this way, we believe that every science-teacher who has tried the experiment will testify to the eagerness of the working man to increase his store of knowledge. In the case of those classes of the labouring population who live in towns, the only substitute is to take them to the Museums or Collections of Natural History, which are the best representatives of Nature herself. For our country gentlemen and tradesmen, the Naturalists' Field Clubs, which now flourish in so many counties, are doing a good work: something of a similar kind is wanted for the less educated class of the population.

But when the eye has been trained to observe, the whole work of education has by no means been accomplished. It is the portion of the work on which there is most need to insist at the present time, because it has hitherto been almost entirely neglected. The system pursued till recently in our Universities and public schools was based on a high cultivation of the reasoning faculties, to the almost entire exclusion of any recognition of the perceptive powers. There may be a danger now of running into the other extreme, and already we hear some zealous devotees of Natural Science exclaim against book knowledge, as if it were opposed to a practical scientific training. There cannot be a greater mistake. Unless a man thinks to live the life of a recluse, and to profit nothing by the labours of others in the same field, the greater part of his knowledge must always be derived from books. What we insist on is that the learner must be taught *first* to use his own eyes, before he has recourse to the experience of others. No man can be considered to be highly edu-

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cated who does not possess the power of bringing his perceptive faculties to play on the phenomena that surround him, and also of exercising his reasoning powers to systematise his observations, and to compare them with those of others who have preceded him. The surest way of cultivating the Perception is by the severe study of some branch of Natural Science; the Reason is to be trained in the lecture-room and the study. Nature does not proceed on the principle of setting one of her gifts at variance with another; and so far from one of these sets of faculties being opposed to the other, neither can be cultivated to the full extent of the mental powers without the assistance of the other. No nation has distinguished itself by producing a greater number of keen and accurate observers of Nature than the Scotch, and none has set a higher value on the education that is derived from books. In the scientific education of the agricultural population of England, it will be found that the long disuse for generations of the reasoning powers is the greatest difficulty to be overcome. Although we do meet here and there with those who are more or less accurate observers of Nature, it is extremely rare to find one who has any power of forming a connected train of reasoning as the result of his observations.

We need but look around us on the events passing before our eyes on the Continent of Europe, to recognise the manner in which Education is proclaiming herself victorious along the whole line. As a nation, we are slow to learn. But that nation must indeed be both deaf and blind, which does not at the present time see the necessity of straining every nerve to redeem itself from the disgrace of ignorance. With our working classes taught to exercise those faculties which they all possess, but which so few know how to use, and thus trained to form the strength of the nation in all fresh advancements in Science and the Arts, England would quickly distance all competitors, and assume that position which it would now seem younger rivals are likely to snatch from her grasp.

HUXLEY'S LAY SERMONS

Lay Sermons, Addresses, and Reviews. By Thomas Henry Huxley, LL.D., F.R.S. (Macmillan and Co., 1870.)

IN this volume Professor Huxley has presented to the public a miscellaneous collection of essays, some didactic, some controversial, some addressed to a general audience, some to a special one, and composed at various times during a number of years extending from 1854 to within the last few months.

The subjects of which the Professor treats are as various as the occasions for which his papers were written and the audiences which he addresses, and, as may be easily believed, his essays are of very unequal value. But one great element of value they all possess in common, and that is, the thorough-going boldness, and honesty, and out-spokenness with which they deal with all subjects alike. This is no small merit in any writer, and it is an especially great one in a man occupying the position which is held by Professor Huxley. It is a remarkable condition of English society at the present time that a man who combines real scientific eminence with great general ability and special oratorical power, as Professor Huxley does, is made, with or without any consent of his own, into a kind of popular oracle. Like an oracle he is expected to have a response ready for any imaginable query, and like an oracle too he must find himself not unfrequently under special and strong temptation to "prophesy smooth things." Yet Mr. Huxley does not prophesy smooth things; on the contrary, he does not hesitate to put the most unpopular propositions in the plainest possible language when he sees that it is right to do so; and to say that a man—and that man a public teacher—lies under special temptation, and that he resists that temptation, is to say at once that his teaching must be worth listening to, and that even where we cannot accept his doctrines, we may still listen to them with advantage and gain instruction from them.

With one exception, the papers in this volume may be classed under three heads, viz.: Educational Essays, either theoretical or practical, which include Nos. 1, 3, 4, 5, 6, and 9: Scientific Controversy, consisting of Nos. 12 and 13, on the Origin of Species; and also 7, 8, and 14, of which the first is the famous "Essay on the Physical Basis of Life," and the other two are replies to the attacks made upon the former: Finally, Presidential Addresses to the Geological Society, Nos. 10 and 11, of which the latter might fairly come under our second heading, consisting as it does of a very able reply to Sir W. Thomson's strictures upon modern geology. The essay which will not come into any of these divisions is the shortest in the book; viz. that on "Emancipation, Black and White." We must however devote some space to it, since it appears to us to be almost the best reasoned and most temperate view of what its author calls the "irrepressible woman question" which we have yet seen, although we are not prepared to accept the author's conclusions without reserve. In this essay Mr. Huxley's allegiance to the facts of science comes into uncomfortable collision with his allegiance to the traditions of party. He comes before us in the character of an advanced Liberal, but he cannot forget that he is, before all things, a biologist; and the consequence very naturally is, that although he is prepared to support a policy of emancipation—apparently upon the general principle that *all* government is a mistake—yet he is compelled to admit that the arguments of *extreme* emancipationists are "to a great extent nonsensical." We are confident that this question is one which must be ultimately settled mainly upon physiological grounds, and it is just because the conventions of society very rightly do not admit of the full and fair discussion of those grounds before mixed audiences, that the extreme emancipationists have been enabled to obtain for their theory the amount of currency which has lately fallen to its lot. In the present instance, Mr. Huxley appears to have followed out his physiological argument with characteristic fairness *to a certain point*, and consequently sees that after all due emancipation, "Nature's old salique law will not be repealed, no change of dynasty will be effected;" and again, that "so long as potential motherhood is her lot, woman will be found to be fearfully weighted in the race of life." But why should Mr. Huxley halt at this admission? Why does not his Darwinian logic carry him on to its legitimate and necessary consequence? According to the law of natural selection, when once fairly engaged in the struggle for existence, no less a penalty than ultimate extinction awaits the weaker race. If the parallelism between a race and a sex can be maintained at all—and the parallelism is Mr. Huxley's, not ours—it plainly implies that, put into competition with man, woman must sooner or later cease to exist *as a competitor*, just as certainly as the black rat has been driven out before the *Mus decumanus*, or as, to adopt a different class of example, the handloom weavers have been driven from the market by machinery and steam. But while we thus doubt the wisdom, or indeed the possibility, of placing women on a level with men, and in competition with them, we would by no means therefore be understood to argue against giving them a liberal education or improving the law in regard to their property.

Passing on to consider for a short time the Educational essays, we need say but a very few words of the single specimen given of the writer's method in practical education, viz. the Lecture on a Piece of Chalk. It is certain to be well known to most of our readers. Those who do know it, for the most part recognise it as a model both in matter and in manner of what a single lecture ought to be; those who do not had better read it at once, for till they have so done they will have but an imperfect idea of such a model. The other educational essays, viz. the first six in the book (with the exception of the second) contain an exposition of the author's views on many of those points in the theory of education which are most keenly disputed at the present time, such as the value of natural science as contrasted with mathematics or philology as an instrument of education; that of the natural history sciences as contrasted with other branches of natural science, the method by which they should be taught, &c. Now it is only fair to admit that in all these matters Professor Huxley's addresses must be looked upon as the speeches of an advocate, and moreover of an advocate who feels that he carries the feeling of the public with him for the most part, but is by no means equally sure that he can overcome the prejudices of the jury. Still, considered as the speeches of an advocate, they are admirable, and it must be remembered that an advocate may prove his case, and this, we think, Mr. Huxley has in several instances done. In regard to the most important of the questions discussed, we are disposed to believe that any one of these three instruments of education may turn out a highly cultivated and thoroughly well educated man, provided the teacher knows how to teach and the learner presents good raw material upon which he may exercise his skill; but this by no means proves that they are all of equal value. One thing we can certainly say in regard to the classical education of our own day, that is to say, of the day of those who are the acting men of the present generation, viz., that, however well it may have served the turn of that small minority who were sure to make the best of any kind of education, and for whom, therefore, it is the least necessary to make provision, it has done nothing at all for the great majority of those who have been submitted to it. It is not too much to say, that out of the men who have gone from public schools to Oxford, and who have spent their whole lives between the ages of eight and twenty-two in learning Latin and Greek, not one in three could at the latter age read a Latin or Greek author with ease and intelligence. This may not in itself prove the case of science as an instrument of education, or even prove the inefficiency of classics; but at least it shows that classics have failed as a fact, and reduces us to this dilemma, that we must admit either that they are but a very imperfect means of education, or that the general standard of educability among young Englishmen is unaccountably low. One other alternative indeed remains, viz. the supposition that classics have been generally very badly taught, but this seems to us hardly tenable. It is difficult to believe that so much labour has been bestowed by so many good scholars as may be found amongst the schoolmasters of the last fifty years, upon the art of teaching classics, without the elements even of the art being discovered. At any rate reformers, or even revolutionists, in education may fairly argue, that what has not been done in so many years by a method which has had the whole field of the higher education to itself, is hardly likely to be effected by a persistent continuance in the same path. We are reminded of the physician of Laputa, of whom, when he had already almost killed his victim by his discipline, Gulliver says, "We left the doctor endeavouring to recover his patient *by the same operation*."

We have left ourselves no space in which to notice the remaining and more directly scientific portion of Professor Huxley's work. The book is not to be discussed fairly in the space at our disposal: it is, however, full of interest throughout, and we need perhaps the less regret that we are unable to direct our readers' attention to the remaining essays, inasmuch as they constitute that part of the work which deals with the scientific controversies of the day some of which have already been discussed in NATURE.

G. W. C.

FERNET'S ELEMENTARY PHYSICS

Traité de Physique Elémentaire. Par Ch. Drion et E. Fernet. Troisième Edition. (Paris: V. Masson et Fils. 1869.)

THE third edition of this well-known handbook of French physics deserves more than a casual notice. We are told in the preface that it has been entirely recast by the second of the two original authors, M. Fernet, a pupil of the lamented Verdet, who has caught something of the spirit of his master. There has been no teacher of physics in our time whose work has been, on the whole, comparable to that of Verdet. He has all the clearness of Tyndall; and, as almost all of his published lectures were delivered to audiences more strictly scientific than those to whom the famous books on Sound and Heat were originally presented, he is never diffuse. His arrangement of the essential points of his subject, and his grouping of the illustrative details and of the exceptions to the general principles which govern it, have scarcely been equalled even in France, which is the special country of precise and exhaustive exposition. It is high praise, therefore, to say of M. Fernet, that in parts his book recalls his master's method and style.

The treatment of mechanics which is common in this country places statics before dynamics. There is only one thing to be said in favour of this arrangement—that the idea which lies at the root of dynamics, that of change of rate of motion, is a little difficult for a beginner to catch, and it is very hard to get him to see how to express it in any but the simplest case. On the other hand, we have no clear notion of Force at all until we master it, and statics without that idea is a series of barren propositions, which stand to real life much in the same relation as a "uniform, weightless, perfectly rigid, straight rod" does to a real bar. M. Fernet accordingly places this conception at the commencement of his book, and we find the laws of uniformly accelerated motion given before the discussion of levers and centres of gravity.

It is extremely easy, and perhaps a little ungracious, to select for notice points the omission of which causes surprise even in an elementary work of 800 pages. The need of compression, which constrains an author who has to treat in one volume mechanics, hydrostatics, pneumatics, light, heat, sound, electricity, magnetism, and meteorology, is so great, that it is impossible to question too loudly the prudence of obvious omissions. But we should have expected to find a little discussion of the adhesion of liquid plates to the solids which they wet, and to each other; and one would willingly excuse the absence of the regulation picture of the balloon and its car, with the accompanying history of the brothers Montgolfier, for an account of the mercurial air-pump of Sprengel or Jolly. M. Fernet is perhaps a little open to the charge which is constantly brought against the scientific men of his country, that if they read anything but the science of the *Comptes Rendus* and the *Annales de Chimie et de Physique*, they never indicate the fact by a line or an allusion. M. Verdet alone, of well-known French scientific writers of our time, knew German and English as well as French science, and showed that he knew it. We should scarcely have found any book of his on the subject of heat, without any reference—as far, at least, as I can find—to Joule's principle of the equivalence of mechanical effect and heat, of the "mechanical equivalent" by which the one may be converted into the other, or of the fraction of the heat which can be converted into work by a perfect heat engine.

So far as it goes, in fact, the book is extremely clear and satisfactory; but it gives one less impression of a complete working up of the subject to the latest date than we had expected to find in it. Take for instance the well-known series of experiments by which Kundt established the velocity of sound in tubes of different materials. They were explained by Tyndall in this country a couple of years since, and it is impossible to conceive any which make more visible to the student those vibrations of bodies which he is constantly required to admit in the course of his reading on acoustics. The omission of all mention of the famous "singing flames" is more defensible from a purely scientific point of view, as the experiments scarcely admit precise measurements, and can only be relied on to convince any one who needs convincing how many astonishing things there are within the range of the science.

M. Fernet has given a considerable number of notes, in which there is a precise mathematical treatment of the statements in the text. We should be glad to see the plan followed more frequently in elementary treatises. In a book, the introduction of simple mathematics in a note does not distract the attention of the reader who is frightened even by a simple equation, and it gives precision to statements which can scarcely be fixed without them, or without a long and extremely tedious paraphrase. We are more doubtful of the advantage of a practice common in France, and which has crept into some English books; the introduction into the text of *notes non exigées*. These are parts which the student who is preparing for a special examination may omit if he chooses. The text of a book of this kind ought, it seems to us, to be either one thing or another—to be composed with a perfectly definite object, and to be one and indivisible. As it is, it produces something of the impression that is given occasionally when a single picture is used to illustrate two different propositions. The lines which belong to the first get so mixed with those of the second, that the student can follow neither. A book too full of *notes non exigées* is apt to be too little systematic and scientific for the more advanced student for whose benefit these notes are inserted, and to be made too difficult for the simpler readers whose wants are mainly kept in view.

WILLIAM JACK

OUR BOOKSHELF

Cryptogames Vasculaires du Brésil. Par Prof. A. L. Fée avec le concours de Monsieur le Dr. Glaziou. Pp. 268, 4to., 78 Plates. (Paris: Baillière.)

PROFESSOR FEE is by many years the oldest amongst living fern-authors. He has held for more than a quarter of a century the chair of botany at Strasburg, and has concentrated his attention principally upon ferns and the other allied higher orders of cryptogamic plants. He published a general treatise upon the classification of the order as long ago as 1844, and since then many consecutive years have never passed without producing some memoir upon the subject from his fertile pen. Having recently received a fine collection of ferns from Dr. Glaziou, the superintendent of the Botanic Gardens at Rio Janeiro, he has been stimulated to add one more memoir to the series, and that is the work now before us. All the series of his monographs, several of which are in folio, are illustrated beautifully and copiously, not only with full-sized figures of the plants, but also with careful magnified analytical details; and together they form by far the most extensive and excellent series of fern-plates which anyone upon the Continent has published. The present memoir is quite upon a par with its predecessors in this respect. It is in quarto, and contains seventy-eight quarto plates and a list of all the ferns and fern-allies known to the author as inhabiting Brazil, with a list of special stations, but with descriptions of novelties only. But there is one drawback to the value of Fée's works, and that is a very great one. Living at a distance from the great metropolitan herbaria, our author has apparently worked almost entirely upon his own private collections, and has continually failed to recognise well-known plants, and has made new species in great numbers out of the specimens which his correspondents have sent him, which no one else has been able to understand as such. In none of his works—we have no alternative but to say—has this tendency been carried to a greater excess than in the present one. For Brazil alone he describes and figures in the present memoir upwards of 180 new species, so called. These are not from tracts of country which the collectors whose gatherings have been already reported upon have not visited, or have left unexplored, but nearly all from the vicinity of the capital, and from the gatherings of Glaziou. Now the neighbourhood of Rio Janeiro is exceedingly rich in ferns; but there is, perhaps, no other part of Tropical America from which herbaria, both in England and on the Continent, have been more bountifully supplied. The consequence is, that out of this 180 we do not think that more than from twelve to twenty species are really new, in any sense in which we understand in this country what is meant by a species. For instance, we have some seven or eight species elaborately characterised and figured from what cannot be called anything else than so many individual fronds of that most cosmopolitan of ferns, our common English *Aspidium* or *Polystichum aculeatum*. Or, to take one of the exclusively Brazilian species, *Cyathea Gardneri*, a very distinct tree-fern, is included in the list under five different names—*Gardneri* (Dr. Gardner's number on which Hooker described the species quoted), *incurvata* (a name of Kunze's published in the Linnæa from Regnell's specimens), *mamillata*, *taunaysiana* and *attenuata*, the last three new species here named and figured for the first time; but the figures, beautiful as they are, might, any of them, have been drawn from Gardner's specimens. The author does not seem to have any knowledge of numerous English and German books and papers in which Tropical American ferns are described, as for instance, Grisebach's excellent Flora of the British West Indies; and this leads to further name-crossing. In short, although one cannot but admire the excellence and the copiousness of the illustrations in these memoirs, and ought not to leave out of sight the example of devotedness to science which they show, expenditure of time devoted to one object through a long course of years, and of money, only a very small proportion of which their sale can possibly repay, yet still the predominant feeling on the mind must needs be that to deal with plants in this way has a direct tendency to bring species-botany at a very rapid rate into a state of utter confusion.

J. G. BAKER

The Laboratory Guide. A Manual of Practical Chemistry for Colleges and Schools, especially arranged for Agricultural Students. By A. H. Church, M.A., Professor of Chemistry in the Royal Agricultural College, Cirencester. Second edition, enlarged and revised, pp. 170. (London: Van Voorst, 1870.)

THIS little book, as its title indicates, is intended mainly for the use of students of agricultural chemistry, and we fear it might cause disappointment to anyone who wished to employ it as a guide to general analysis. The science of chemistry is so rapidly increasing, that it would seem almost hopeless, at the present time, to give students a complete knowledge of chemistry and leave them to apply their information to the special subject they intend to follow. Professor Church's book is intended to obviate this difficulty, and after a few introductory lessons of universal application, the student commences experiments on materials with which he is certain to come in contact in agriculture, such as superphosphate, milk, soils, &c. Part I. treats of chemical manipulation, and consists of a number of lessons intended to accompany the course of lectures, and from which the student will learn the mode of performing some simple operations, as solution, filtration, crystallisation, specific gravity, and will become acquainted with the modes of preparation and properties of the principal elements and compounds. Each lesson commences with a list of the apparatus required, the ordinary reagents, and the special materials and tests necessary for the performance of the experiments, which are detailed with great clearness. This arrangement is calculated to cause the student to be careful to have everything ready before commencing work, and will thus save him much time and inconvenience, for few things are more likely to endanger the success of an experiment than leaving it at a critical moment in order to obtain some piece of apparatus or reagent which should have been previously prepared. Part II. treats of qualitative analysis, of which Chapter I. deals with the elements, reagents and tests, and reactions; and here we find the terms univinculant, bivinculant, trivinculant, &c., as equivalent to monad, dyad, triad, &c. The principal distinguishing characteristics of the different groups of elements are here given. The section on reagents and tests will be found useful, for it contains the modes of testing for impurities, and indicates the strength of the different solutions employed, two things to which attention should always be paid. The second chapter of this part describes the methods of qualitative analysis, all rare elements and those with which the agricultural student is not likely to meet being omitted. The third part is devoted to the general processes of quantitative analysis, and the fourth to the examination of manures, soils, water, and food. This book will doubtless be invaluable to agricultural students, besides being useful to those requiring special information on the subjects of which it treats. The appearance of such a work is a satisfactory indication of the extension of the application of scientific chemistry to the useful arts.

The Book of the Roach. By Greville Fennell (of the *Field*). 16mo. pp. 118. (London: Longmans and Co. 1870.)

WHILST Mr. Pennell has instructed us in catching *lege artis* all the various fish in British rivers and lakes, Mr. Fennell has been content to devote a little volume to the natural history and fishing of the Roach. Let no one smile at the man in the punt with his humble notions of enjoyment. Maybe he has been toiling hard the whole week in the noisy, murky town; the quiet sport of the Saturday afternoon suits his purse exactly, and there will be real enjoyment over the dish of fried roach "caught by father." Nay, if we could measure the amount of pleasure, healthy recreation, and renewal of vigour obtained by the multitude in the unpretentious sport of roach-fishing, and compare it with that sought for by the select few who have the privilege of finding their amusement on a salmon river, we should probably find the balance very much on the side of the former. No apology, therefore, was needed from Mr. Fennell for the publication of his little book on the Roach. He has divided it into eight chapters, of which the first two are devoted to the natural history of this fish, and the five following to a description of the tackle and various kinds of baits, and to the methods of roach-fishing generally as well as at certain localities. In the last chapter hints are given on the roach as an article of food, on the method of cooking, &c.

A. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Hypothesis regarding the Corona

HAVING read in Nos. 34 and 35 of the valuable periodical NATURE (of June 23 and 30) the two articles about the Corona, I beg leave to direct your attention to an hypothesis concerning its nature, and especially the origin of the *beams*, which I sent to the Physical section of the Amsterdam Academy of Sciences, of which I have the honour to be a member.

I have just received No 1776 of the *Astronomische Nachrichten*, for October 15, where the American astronomer, Dr. Gould, in a notice regarding the total eclipse of the sun of August 7 (1869) says:—

"Of the Corona I made some hasty measurements both with the telescope and without it. Its form varied continually, and I obtained drawings for three epochs at intervals of a minute. It was very irregular in form, and in no apparent relation with the protuberances of the sun, or the position of the moon. Indeed, there were many phenomena which would almost lead to the belief that it was an atmospheric rather than a cosmical phenomenon. One of the beams was at least 30′ long."

This passage induced me to submit to the trial and judgment of my fellow-members of the Physical section of the Royal Academy a very simple hypothesis regarding the nature of the Corona, which entered my mind shortly after the observation of the total eclipse of August 18, 1868.*

I think—and, if I am not mistaken, all astronomers agree with me—that a part of the luminous phenomenon which we call the Corona belongs to an atmosphere of the sun,† having a feeble reflecting power, or being itself luminous. But the beams in the Corona, whose variability is now confirmed anew, are necessarily an optical phenomenon. They originate, I believe, in the inequalities of the moon's surface. If the sunlight slants somewhere along the moon's limb through a *valley*, we observe from our point of view a *beam*, provided there exist between the moon and us particles able to reflect the sunlight, or to transmit it like semi-transparent bodies. There is no need to have recourse to diffraction.

To look for these particles in the atmosphere of the earth, as Mr. Gould does, is, in my opinion, not tenable, as the Corona and the beams have also been observed in eclipses, where the cone of the shadow even had‡ a breadth of thirty-six German leagues. The molecules of the atmosphere, which we see around the eclipsed sun, are wholly within the cone of the shadow.

These reflecting particles are undoubtedly to be looked for beyond our atmosphere, between the moon and the earth, and I believe that they may be regarded as identical with those particles which float in the ether, and under other circumstances cause the zodiacal light.

That the zodiacal light, or rather the particles which cause it, reach the earth's orbit, is, as regards nearly its whole circumference, not subject to any doubt, as the apex of the zodiacal light is "mostly farther off the sun than 90°. Only in the months of March and April is it not possible to follow the zodiacal light so far" (Schmidt, "Das Zodiacallicht," Braunschweig, 1856). But then, at those particles which are situated on the apex of the zodiacal light, the sun's ray makes a right angle with the line which joins the earth, and the circumstances are not favourable for small and widely-dispersed particles to reflect much sunlight in the direction of the earth. It is, therefore, probable that in March and April these particles would show themselves farther from the sun if the illumination were stronger. At a total eclipse of the sun that angle is $179\frac{1}{4}°$ for a particle visible at 30′ distance from the moon's limb, and $179\frac{3}{4}°$ for particles in the immediate neighbourhood of the moon's limb; and it is an acknowledged fact that, under these circumstances, there is much more light reflected than if reflected at a right angle. Moreover, the intensity of the transmitted light increases equally with this angle.

Schmidt mentions, in his above-quoted work, that he and other observers, during the total eclipse of July 28, 1857, looked out for the zodiacal light, but in vain. He thought the sky not dark enough, and expresses himself as follows:—"Das Ansehen der den schwarzen Mond umgebenden vielstrahligen Corona war im ganzen betrachtet nicht gerade geeignet, sie *sogleich* in Beziehung zum Zodiacallichte zu denken."

Thus it seems that the beams made Schmidt conclude, "This is no zodiacal light." If my explanation is the true one, and if the sun's atmosphere is accepted as unlimited, and gradually passing over into the ether, then the light of the Corona, which equally surrounds the moon, falls altogether into the same category as the beams, only that it originates in the reflecting particles beyond the moon. Accordingly, my hypothesis is expressed thus: *Both the Corona and its beams have the same origin as the zodiacal light.*

I remark, finally, that the strangely curved form of some beams, as well as their variability, may be very well accounted for on this hypothesis—the curved form by the irregularity of the moon's surface. For instance: I take a particle of light of a beam of the Corona; I imagine myself in the molecule which, according to my hypothesis, corresponds with that particle of light, and which is situated between the moon and my eye. Looking, then, from that stand-point to the moon, I must see the light of the sun slanting over a valley; but if I move myself laterally—*i.e.* in the direction parallel to the moon's radius which corresponds with that valley—then it is not certain that I shall see on the same point an extraordinary amount of light, for it is possible that at the same place a ridge has intervened. It is, however, very possible that, if I move in a direction perpendicular to the former (and also perpendicular to the direction towards the earth), I come into a region where that ridge does not intervene, but where the continuation of the valley is again visible, through which the sunlight slants. In this case, the beam of the Corona—*i.e.* the effect produced by those particles which receive more light than others at equal distances from the surface of the cone, having its apex in the eye, and which surrounds the moon—has a curved form.

The variability is, I think, satisfactorily explained by the motion of the moon passing by the sun.

This hypothesis forced itself upon me when at Toli-holi* I saw the moon rising behind a hill. Before she made her appearance her light shone over the trees, and produced, in a hazy air on my side of the hill, beams which very much resembled those of the Corona. I do not know whether the zodiacal light has already been analysed by the spectroscope; if so, the comparison of the spectra of zodiacal light and the Corona will serve to test my hypothesis.

A second test is this: total eclipses observed in the months of December and January should show less brilliant beams than total eclipses observed in other months, since in December and January the earth is near the direction of the perihelion of the circumference of the zodiacal light. The next total eclipse of December 12, 1871, will perhaps give some information on this point.† J. A. C. OUDEMANS

Batavia, Sept. 2

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* At the Island of Mantawalu-béké (near Celebes), where I had the pleasure of meeting the Commander (Captain Bullock) and the Etat-Major of H.M.S *Serpent*, and the Spanish astronomers of the municipal Athenæum at Manilla, the fathers Faura, Nonnell, and Ricardo

† This is probably the Chromosphere, as seen in the Eclipse.—Ed.

‡ (*Subintellige*): at the surface of the earth.

* *Vulgo*, erroneously, Tontoli, north coast of Celebes.

† We hope also something from next month's eclipse.—Ed.

The Fuel of the Sun

In your impression of October 6 Mr. Murphy adds another to the frequent attempts that are still made to galvanise the expiring hypothesis that attributes the solar heat and light to a meteoric bombardment. Many very strong and sufficient objections have been already brought against it, but as Mr. Murphy states that he is "not mathematician enough to form any opinion on the merits of the controversy," I will add two arguments which to my mind are quite sufficient to annihilate this explanation—both of which may be sufficiently understood without mathematics, and neither of which have I ever seen fairly stated.

1st. The advocates of the meteoric bombardment usually start from the fact that great meteoric showers fall upon the earth. Thus, Dr Tyndall, in his lectures on "Heat considered as a Mode of Motion," introduces Mayer's hypotheses, with an account of the number of meteors counted during the August and November showers; and these observed meteors and a few comets are the only actual observed material upon which this bombardment theory rests—all beyond them are mere figments of mathematical imagination, and any supplies derived from the zodiacal light, or otherwise exclusively from the space within the earth's orbit, must have been exhausted within the period of human existence.

Now, it is quite obvious, without any detailed calculation, that if these meteoric bodies, coming from anywhere you please outside of the earth's orbit in sufficient quantities to maintain the heat and light of the sun, had fallen, as they must have done, upon the earth in a proportion due to its magnitude and position, they must in the course of a few millions of years—say from the era of the Laurentian rocks to that of the London sewage deposits—have covered the earth with a very important superficial stratum, instead of merely supplying a few rare specimens for our museums. Every slowly deposited sedimentary rock upon the face of the earth should be thickly peppered and conglomerated with meteoric dust and nodules. With these considerations and the well-known geological facts before us, I need scarcely state the obvious conclusion, viz. that the evidences in support of the theoretical terrestrial requirements of this bombardment hypothesis are contemptibly insufficient

My second objection attacks the fundamental basis of this hypothesis, and I think destroys it altogether. I maintain that any explanation of the sources of solar light and heat which does not equally and necessarily account for the radiations of all the other self-luminous orbs that people the whole immeasurable depths of space, is philosophically worthless. It is thus worthless if it does not also account for the perpetual renewal, the constancy, the eternal permanence, of all these radiations. The pale nebulæ, as well as the brighter suns, should be equally included in its grasp.

Tested thus broadly and philosophically, the meteoric bombardment hypothesis appears in its true colours as a monstrous physical absurdity. It assumes a perpetual flow of solid masses *converging* continuously from everywhere towards everywhere; or otherwise a state of things which could only endure through the time which these meteoric masses would occupy in travelling the semi-distance between the neighbouring suns. These little journeys ended, the interstellar space must, according to this hypothesis, become a sterile vacuum, all the lights of heaven must go out, eternal darkness must rest upon the face of the deep, and everlasting death pervade the universe.

W. Mattieu Williams

The Cockroach

I have only to-day noticed the Rev. C. J. Robinson's letter on this subject in your issue of the 29th Sept. A friend of mine, whom I have known all my life, who occupied an important trust as Bank Manager in India last year, and who is at present home on sick leave, assures me that Dr. Norman Macleod is wrong when he denies the nail-nibbling propensities of the cockroach. My friend had been in Kurachee for some time, and on his journey from that town to Bombay by sea he was annoyed one night in his berth by some insect crawling over his face; half asleep and half awake he put up his hand to his face and sent the insect to the foot of his berth. Shortly after he was awoke by a pain at his great toe, and on looking at it he discovered that a cockroach had nibbled off all the nail down to the quick.

James Durie

Aberystwith, Oct. 8

Were Cockroaches known to the Ancient Greeks and Romans?

Your correspondent, Rev. C. J. Robinson, drew attention in your columns (Nature, Sept. 29) to the question whether these troublesome insects were known to the Ancient Greeks and Romans; he says, "there is a good deal to lead one to suppose that the *μυλακρίs* mentioned by Aristotle, and the *Blatta pistrinorum* of Latin writers was the same as our loathsome pest." I think Mr Robinson is mistaken in supposing that the *μυλακρίs* is mentioned by Aristotle, at least I can find no mention made of this insect in the writings of the Stagirite. The word *μυλακρίs*, meaning some kind of insect, occurs in the fragments of Aristophanes preserved by Pollux, who amongst other meanings of the term gives the following one:— *ζῶόν η ἐν τῷ μύλωνι γινόμενον*, and then quotes this couplet from Aristophanes,

Ἵνα ξυνῶσιν ᾧπερ ἥδεσθον βίῳ,
Σκώληκας ἐσθίοντες, καὶ μυλακρίδας.

"Where they may partake of the food of which they are fond, eating worms and *mylocrides*." It would not be possible to say what the *μυλακρίs* here denotes, but from the creature being often produced in mills, it may possibly mean a "Cockroach," though a "meal-worm" (*i.e.*, the larva of the beetle, *Tenebrio molitor*) would suit equally well The Greeks, however, had a word which may well represent the Cockroach, though it is even here impossible to speak with certainty. The word, *σίλφη*, it is probable denotes this insect. Aristotle (Hist Anim. viii. 19. § 4) uses the word once; he enumerates the *silphe* amongst insects which cast their skins. The Scholiast in the "Peace" of Aristophanes says the *silphe* is an ill-smelling insect (δυσώδμος). Aetius (8. 33.) speaks of "the fat of the stinking *silphe* which inhabits houses." The epigrammatist Evenus (Analect, i. p. 167) speaks of the *silphe* of the booksellers' shops, and applies to it the epithets, page eating (σελιδηφάγος), destructive (λωβήτειρα), black-bodied (μελαινόχρως)." Lucian speaks of the mere book collector as providing pastime for mice and habitations for *silphe*, and cuffs his slaves for not keeping the mice and *silphe* away. (Advers. Indoct, iii. 114, Ed Hemsterhus). The Scholiast here gives a description of the *silphe* which Schneider with some reason refers to some kind of *Lepisma*. Ælian (H. A. i 37) says that the *silphe* infest swallows' nests; these cannot be cockroaches. Galen and Paulus Aegineta apply the epithet, *βδέουσαι*, to the *silphai*. Dioscorides (ii 38) says that the inside of the *silphe* found in bake-houses when pounded with oil is good for pains in the ear This leads me to the *Blatta* of the Romans. "On pulling off," says Pliny, "the head of a *blatta*, it gives forth a greasy substance, which, beaten up with oil of roses, is said to be wonderfully good for affections of the ears." He speaks of the disgusting nature of this insect, one kind of which is known by the name of *Myloecon*, and found in mills (Nat. Hist. xxix. 39). In another place (xi 34) Pliny says, "It is the nature of the *blatta* to seek dark corners and to avoid the light; they are very often found in baths." According to Virgil, "the light-avoiding *blattæ*" find their way into bee-hives (Geor. iv. 243). Horace (Sat. ii. 3, 119) ridicules an old miser for sleeping on straw and leaving his bed clothes in his chest, the food of *blattæ* and *tineæ*, "Blattarum ac tinearum Epulæ." Martial (Lib. iv. Ep. 37.) says unless his books are well put together they become the prey of *tineæ* and *blattæ*.

Constrictos nisi das mihi libellos
Admittam tineas trucesque blattas.

From the above passages it will be seen that the *blatta* was a destructive insect to clothes, books, &c., that it avoided the light, and was fond of warm places, that it frequented mills and exuded a greasy substance from its head, that it was a disgusting creature (probably in allusion to the smell) all of which particulars are true of cockroaches, and as there are many species of the family, and are widely distributed over all parts of the globe and must have been known to the ancients, I think there is good reason for concluding that the cockroach was known to the Greeks by the name of *σίλφη*, and to the Romans by that of *blatat*.

W. Houghton

The Aurora Borealis

I shall be obliged if you will put on record a few scattered notes which I took of the splendid Aurora Borealis of October 25, seen from Arthingworth, Northamptonshire. When I first observed it at half-past five P.M., a crimson glow extended in an irregular band from N.N.E. to W, most prominent at about 20° to 30° above the horizon. This increased in height and breadth until it nearly reached a point S.W. of the zenith, and about 15° W.N.W. of the star Vega. At this time the northern part of the sky was perfectly free from aurora; gradually that part and the whole dome of the heavens, with the exception of a section from W. to nearly S., became filled with luminous streamers. These, for about 20° on each side of N., were white, the others crimson striped with white or rather greenish light, but the green I believe to be an effect of contrast, as where similar streamers were distant from the red light they were white.

The white or green steamers appeared to eclipse the red light, they changed their size, shape, and position, while the red continued comparatively unchanged. There were also dark streamers which, at first, I believed to be mere spaces without light, and to be caused by the darkness beyond, but I became ultimately convinced (as far as one could be convinced by appearances so subject to illusion) that they formed a part of the phenomenon itself. These streamers or long brushes could be seen beyond and clear of the luminous portion of the aurora, leaving the normal light of the sky between them and it, and hanging like long horse-tails, or like the fringes of rain seen on the edges of a distant rain cloud; changing their shape and position just as the luminous streamers are seen to do.

The most remarkable part of the phenomenon, however, was the circle of sky, or what may be called the pole of the aurora, to which the streamers converged. It appeared to embrace about from 7° to 10° of space. To an ordinary observer it might have appeared occasionally to shift its position to some extent, but, as far as I could judge during an hour's observations, this was not really the case, flickerings at times covered portions of it, and at other times the whole became faintly luminous; but by marking its position with reference to some small stars, this seemed to me to be unaltered. Most singular were the terminations of the streamers they culminated at this circle, not being undefined or gradually evanescent, but having angular tips far brighter than the portions immediately beneath, the nearest illustration to which I can give is an inverted fish-tail or bats-wing gas burner, except that this gives a feeble light at the point, while the aurora tips were whitest and brightest there, the streamers now fading off, and now becoming brighter and tinged with red as they got to 40° or 50° from the horizon; the tips varied constantly, but preserved the mean distance from the pole or focus of the aurora. The position of this was, as far as I could ascertain without star maps or instruments for observation, about 15° W.N.W. of Vega. The convergence of the beams was not in appearance conical, but dome or cupola shaped; this was, however, in all probability an optical illusion. Whether there was really a convergence or whether the beams were parallel, and the convergence an effect of perspective, can only be decided if some approximative measures of the distance of the streamers be ascertained. It appears to have been at a greater distance from the earth than is usually attributed to aurora borealis, having been seen in different parts of Europe and I believe in America. Doubtless the comparison of these observations will give some parallax or approximation to measurement of the distance. I remember about seven or eight years ago seeing an aurora at Chester, where the flashes appeared close to the observer, so that gleams of light continuous with the streamers could be seen between the houses of the town and myself, like the portions of a rainbow intervening between terrestrial objects and the observer. I tried then to ascertain if there was any reflection or other cause of optical illusion, but could not see it as other than a real effect; I seemed, so to speak, to be in the aurora. The effect on the 25th was very different, and gave me the idea of great distance.

The light was sufficient to enable me to tell the time by my watch easily, but not to read newspaper print.

Between half-past six and seven o'clock it faded away, and at from half-past seven to ten had become an ordinary white aurora, confined to the northern portion of the heavens.

115, Harley Street, Nov. 2 W. R. GROVE

ON the evening of the 24th ult. the aurora was most beautifully seen here, and if you have space for it, I will add a further spectroscopic observation to those you have already recorded. I found no continuous spectrum, but two of the lines described by your other correspondent.

1. A line in the light green, much reminding one of the line from the larger nebulæ, but more brilliant and with a peculiar flickering in it. This line was well seen in all parts of the sky, but was specially bright in the auroral patches of silver light.

2. A line in the red, very much like the lithium line, but rather more dusky. This line was only well seen in the rosy patches of the aurora, but could be faintly traced wherever the rose tint at all extended.

When the display of rose-coloured light was at its height, the spectrum from the most vividly coloured portion gave the red line very distinct, while the green line still remained bright by its side. I am quite inclined to agree with your correspondent, T.F., in the conjecture that both these lines are due to hydrogen, though (probably through difference in temperature or pressure) they do not quite agree with the lines of that gas as taken from the discharge in a vacuum tube.

The spectroscope was one of Mr. Browning's small direct-vision 5-prism instruments adapted for star purposes.

It may be worthy of note that the belts of Jupiter are highly coloured at the present time. The equatorial zone is of a distinct dark ochre colour, deepening to red brown as it approaches the lower edge (in an inverting telescope); two thin belts above are slate purple, and a darker belt below is of a deep purple, with a faint trace of rose colour.

The planet was thus seen on Nov. 2, at 9 p.m., not far above the horizon, and in bright moonlight, in a 8½ Browning's silvered spectrum with achromatic eye-pieces—144, 305, and 450; best I think with 144.

Guildford, Nov. 5 J. R. CAPRON

Clouds

I DO not think Prof. Poey's "New Classification of Clouds," published in NATURE of Sept. 8th, does much to advance science. I see no use in any classification of clouds, unless it is based on their mode of formation, and, so far as I see, there are but three ways in which it is possible for clouds to be formed. These are:—

1. The cooling of a mass of air *in situ* by radiation. This forms stratus.

2. The cooling of a mass of air by diminished pressure when it flows in an ascending column. This forms cumulus. A modification of this process is when (according to Espy) sudden expansion takes place above, so as to diminish the pressure through the entire height of a column of air, and, in consequence of the cold due to the diminution of pressure, to produce condensation of vapour throughout the column. This is Espy's explanation of waterspouts.

3. The cooling of a mass of air by coming into contact with a cooler mass of air than itself. This forms cirrus.

Of course these three modes of formation may be modified and combined in endless ways. To mention one of the simplest: A cloud which has begun to form as a cirrus or cumulus, may become a centre from which heat is radiated, and thus go on forming as a stratus.

It is in the highest degree unphilosophical to reject stratus as a species of cloud on the ground that it is "not a cloud properly so called, but a mist or hoar frost." A cloud and a mist do not differ fundamentally.

Prof. Poey is, however, right in saying that cumulus is not a distinct species of cloud. It is only a cloud which (in consequence, I believe, of the loss of electrical tension) has begun to run together into raindrops. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim

Extreme Seasons

A GREAT deal of speculation has been indulged in to account for the extreme seasons that have prevailed over so large a part of the northern hemisphere during the last few months. In this country, as we are subject to extreme seasons, more particularly as regards the rainfall, the subject is one of peculiar interest. In a paper read before the California Academy of Sciences in February on the subject of our extreme seasons, I brought forward a number of observations to show that these were due to broad polar and equatorial currents occupying large portions of the earth's surface continuously, and without much perpendicular or horizontal disturbance, except at the borders where the currents meet. The facts I then brought forward showed that from October to the middle of February a northerly current prevails over this portion of the American continent, extending from one to two hundred miles to the westward of San Francisco to the eastern edge of the Mississippi valley, whilst a southerly current prevails over the eastern side of the continent as far as the Atlantic. The southerly current to the westward extends uninterruptedly across the whole breadth of the Pacific to the coast of Japan. This same distribution of air currents without much perpendicular or horizontal mixing has apparently continued during the summer, and acounts, I think, satisfactorily for the extreme heat that has marked the continental climates over so large a part of the northern hemisphere. Nor is it surprising that the summer temperature on the continents should be so universally hot, as a horizontal wind, either from the north or from the south, blowing over the land in summer must necessarily be a hot wind. That there is no cosmical cause for this elevated temperature is proved by the extremely low summer temperature prevailing over the Pacific between this place and Japan. The mean temperature, as ascertained by observations made on board the mail steamships between here and Japan was, for Nov. 1869, 70°·2, for January, 62°·9, for May, 1870, 61·9, for July, 65·7, giving a mean of 2°·7 less for May and July than for January and February. The difference in favour of the winter temperature would be still more marked were the coast temperatures eliminated, as they perhaps should be; as these were much above the mean in summer and below the mean in winter. As to the causes that lead to the peculiar distribution of the air currents in certain seasons, I have not the slightest idea, but I think that, admitting the fact, it affords a satisfactory explanation of anomalous temperatures both in winter and summer.

San Franciso, California, Sept. 4 JAMES BLAKE

Cyclones

CYCLONES are commonly regarded as exceptional phenomena of the atmospheric circulation; and we see in text-books statements as to the seasons of the year at which they are most apt to occur, descriptions of the premonitory signs which herald their approach, and directions to aid ships in avoiding the most dangerous portions of the storm-field. In short, each cyclone is regarded as an exceptional fact, an isolated burst of fury from the old storm-god Hurakan.

The writer has lived all his life on the great highway of cyclones, at Charleston, South Carolina; and from the observations of many years, has been led to conclude that this commonly received view embraces only those cyclones which, on account of their rotatory violence, really do threaten destruction on land and sea; and that consequently it overlooks a most important series of phenomena, which, though they do not so forcibly arrest attention, are even perhaps more significant in a scientific point of view. Though *destructive cyclones* or hurricanes are fortunately rare, cyclones or grand rotatory movements of the atmosphere are, at least on certain portions of the earth's surface, of *every-day occurrence*. In Charleston, Savannah, and along the coast of South Carolina generally, the writer knows from experience that very few, if any, changes of wind are to be observed, but such as are due to the cyclone which happens just then to be passing on its northward journey; and even the apparent exceptions are probably not difficult of explanation.

There is in short an atmospheric "Gulf Stream," whose course, beginning somewhere eastward of the Caribbean Sea, is nearly the same as that of the oceanic "Gulf Stream," and this atmospheric stream is composed of an endless succession of cyclones chasing each other ceaselessly up towards the polar regions, along the track recognised as that of great hurricanes. These cyclones vary within very wide limits both as to velocity of rotation and velocity of translation, as well as in diameter, and all the characters usually ascribed to such atmospheric movements. Many of them exhibit no wind stronger than a pleasant breeze in any part of their field; and a few have so gentle a motion, at least in some parts of their circuit, as will not agitate an ordinary vane; a few are almost wholly without clouds, and very many wholly without rain or lightning. Their effect upon the barometer, when appreciable, must generally be very slight; but in temperature they are usually divided into a warm and a cool semicircle by a line which, in Charleston, lies about S.W. and N.E.

Observation of the winds, during a voyage in a sailing vessel from Charleston to Liverpool, along the course of the Gulf Stream, has satisfied the writer that this *stream* continues unbroken between these two points, and this conclusion was strengthened by repeating these observations between Liverpool and New York. In the former voyage, hardly one of the cyclones which passed over gave more than a stiff breeze, while in the latter, from Cape Clear to Sandy Hook, every cyclone was a storm, and one of them was reported by the captain, on his arrival, as a "hurricane."

The causes of this aerial current, and its connection with the circulation of the whole terrestrial atmosphere, it is not the writer's purpose at present to discuss, though he considers the discussion one of almost *cosmical* importance. But the existence of such a stream is a fact of practical commercial value, in fixing the natural highways for sailing vessels between Liverpool and the Atlantic and Gulf ports of the Southern States. Obviously the short route from Northern Europe to those ports will be that southward along the coast of Europe until reaching the trade winds, then westward to strike the cyclonic current in the neighbourhood of the West Indies, and then, if bound to Atlantic ports, northwestward with that current. When bound, on the contrary, from the Southern ports to Northern Europe, the short route is obviously that along the Gulf Stream, which is also that with the current of the atmospheric stream. To reverse this practice, either way, is deliberately to sail "against wind and tide," if such a stream exist.

The flow of atmospheric *waves* which, in a recent work, has been described as setting from the coast of America towards Europe, though the writer has not seen that work, he believes cannot be other than the flow of cyclones in that portion of the atmospheric stream lying between the vicinity of New York and the English Channel. The cyclonic character is not always distinct, and sometimes is completely masked by the great distance of the observer from the centre, and the consequent apparently rectilinear course of the wind; and the chances of mistake are still further increased when the observer is moving in a course parallel to the path of the centre of the cyclone.

These observations have already been brought to the notice of the Smithsonian Institution, and the writer hopes that something will be done in America towards the comprehensive, precise, and detailed inquiry which the subject demands. But unless attention of the same kind be given in Great Britain, and in the voyages of the Atlantic steamships, the resulting information will remain incomplete.

JOHN M CRADY

Curator of the Museum of the College of Charleston

Charleston, U.S., Sept. 13

Singing of Swans

IN times ancient and modern "singing of swans" has been reckoned by naturalists among "vulgar errors" and groundless superstitions. It may therefore be interesting to your readers to hear that swans actually do sing, which I can testify by my own personal experience.

From my ninth to my eighteenth year I lived at a place in the west of Iceland, called Gufudalur. It is situated at the end of a small firth, called Gufufjördur, which is so shallow that by low water it is almost dry: the bottom of the firth is covered with sea-grass (marhálmur). In this firth hundreds of swans gather together all the year round, except during the winter months, when the firth is covered with ice; and in the month of August, which is their moulting season, when all of them leave this firth and go to another not far off, called Gilsfjordur. There is no apparent reason for this migration, as Gufufjordur seems in every way as safe and convenient for them during this season as Gilsfjordur. Tradition therefore accounts for this migration in the following manner:—Once upon a time two widows lived one on each side of Gufufjordur. At that time the swans did not go away during the moulting season, and the widows used to gather great quantities of swans' feathers, which are sold in Iceland at the present day at a halfpenny a piece. Thus the swans' feathers formed a considerable item in the income of the two widows. Once, however, one of the widows gathered feathers on a piece of land belonging to the other. A quarrel arose, and one of the widows uttered a spell to the effect that henceforth all the swans should leave Gufufjordur during the moulting season. I will not vouch for the correctness of this tradition, but the fact remains that this migration takes place annually during the above-mentioned season.

During nine years I have heard the singing of the hundreds of swans which gather together in Gufufjordur. In the morning and evening their singing is so loud that it can be heard miles away, and the mountains on both sides ring with the echo of it, for at that time every individual swan seems to join in the chorus. This is, indeed, a wonderful conceit. The singing of the swan has not the least resemblance to the cackling of geese or the quacking of ducks. In fact, its voice is unlike the voice of any other bird that I have heard; it seems so clear and full, and has, as it were, a metallic ring in it. When it is calm and clear in the morning or the evening, the swans fly along the valley towards the mountains in parties of seven or nine, sometimes only three; as far as I can remember they are always in odd numbers. During their flight, they either keep in a straight line, one after another, or they form a triangle, leaving an open space in the middle: the foremost swan sometimes emitting single sounds at short intervals. The tradition of the singing of the swan being sweetest just before its death is well known in Iceland; but I am unable either to deny or to confirm this tradition, because I have never been present at the death of a swan.

The swans of Gufufjördur do not lay eggs there, and I am inclined to think that the most of them do not lay eggs at all, for their number in this firth does not seem to be less from the middle of May to the end of July, which is the season during which swans in Iceland lay eggs and bring up their young ones. On the mountains round Gufufjordur there are many small lakes or tarns, and on the banks of those lakes I have seen swans build nests and lay eggs; as a rule there is only one pair on each lake, and, strange to say, these swans sing but very seldom.

JÓN A. HJALTALIN (Icelander).

152, St. Paul's Road, Camden Square, N.W.

State Aid to Science

I REGRET that I should have worded my lecture on Cosmical Physics in such a way as to leave it doubtful how the central establishment I spoke of was to be supported.

Unable myself to conceive the possibility of such an institution being properly supported otherwise than by State aid, I fear I did not sufficiently realise that others might not be of the same opinion. At the time of the establishment of the present Meteorological Office, it was acknowledged that private scientific enterprise cannot be expected to furnish the money requisite to carry on an extensive system of meteorological observations, and the same conclusion equally applies to the other branches of cosmical inquiry.

The most convincing proof of the justice of this conclusion lies in the fact that the British Association, who have hitherto contributed a large portion of their income to advance terrestrial magnetism, find that they cannot do so much longer without detriment to other subjects which have an equal claim upon their liberality. They have therefore resolved to give up their connection with the Kew Observatory after the autumn of 1872. Further proof is surely needless.

B. STEWART

THE THEORY OF NATURAL SELECTION FROM A MATHEMATICAL POINT OF VIEW

THE fascinating hypothesis of Darwinism has, within the last few years, so completely taken hold of the scientific mind, both in this country and in Germany, that almost the whole of our rising men of science may be classed as belonging to this school of thought. Probably since the time of Newton no man has had so great an influence over the development of scientific thought as Mr. Darwin; and no one can over-estimate the debt which Science owes to his patient researches and his clear insight into some of the hidden ways of Nature. The advocates of Darwinism have, however, almost invariably failed to recognise that the theory consists of two essentially distinct portions, one of which may be admitted while the other is denied. The first portion is that with which the name of Darwin is popularly associated, although its origination is by no means due to him, namely, the probable ancestry of all forms of living organism from a single or a few original germs; the other portion, and that which we especially owe to his genius, is the theory that the infinite modifications of existing forms owe their origin to a process of Natural Selection from spontaneous variations. These two perfectly distinct hypotheses have generally been so confounded together that those who have attacked or defended the one have also attacked or defended the other. My object in the present paper is to show that, while the former hypothesis may be considered as established, as nearly as it is possible to establish a theory which requires thousands or millions of years for its complete development, the arguments in support of the second hypothesis are far less satisfactory.

The principle that new forms of organic life have been produced by modifications of older nearly-allied forms is by no means a new one; its inherent reasonableness and probability commended it to Lamarck and the author of the "Vestiges of Creation" long before it was elaborated in a more scientific form by Mr. Darwin and Mr. Wallace. It has been opposed, of course, by theologians; but, were it not that the theological mind is inherently averse to the reception of new ideas, it would have been seen that the supposition that the Creative Power works by continuous modification and adaptation of contrivance to end, by a constant exercise of His prerogative, is a far higher tribute to His exalted attributes, than the popular dogma that all living things were created as we now see them by one single gigantic effort, after which the power collapsed, and has never since been exercised. Why should organic life be the one thing in the world not subject to change? The *coup de grace* may be considered to have been given to the anciently received theory by the investigations so ably carried out by Mr. Darwin and Dr. Hooker on the characteristics of Insular Floras. The fact that no island which has been separated from the mainland during recent geological epochs has genera, and scarcely even species, of animals or plants peculiar to itself, while islands which have remained isolated during lengthened geological periods have faunæ and floræ almost entirely peculiar to themselves, is inexplicable on any other hypothesis than that of the gradual differentiation of species by long-continued separation. No more striking instance of this law has been given than that afforded by the East Indian Islands, as shown in Mr. Wallace's "Malay Archipelago." Two great types of animals and plants are found in different regions of the archipelago, the Indo-Malayan and the Australian; and these two types are separated, not by any diversity of climate and soil—not even by any of the wide but shallow channels which indicate recent separation, such as that between Borneo and Sumatra—but by the narrow but very deep channel separating Bali from Lombok, which indicates a lengthened geological separation of two continents at this point.

The hypothesis that the prime agent in all these infinite modifications is the principle of Natural Selection from spontaneous variations, has been recently further illustrated by Mr. Wallace's volume of Essays, "Contributions to the Theory of Natural Selection;" and it is mainly from the illustrations furnished in this work that I propose to derive my arguments as to its inadequacy. In the first place I wish to call attention to the fact which the Duke of Argyll has already acutely pointed out in his "Reign of Law," that the theory does not even attempt to explain the most inexplicable phenomenon in the development of these organic changes, namely, the first commencement of a tendency to variation. The title of Mr. Darwin's famous work, the text-book of the theory, seems to me, indeed, altogether a misnomer: "The *Origin of Species* by means of Natural Selection." Mr. Darwin admits the existence of what he terms a "spontaneous" tendency to variation among the offspring from a common ancestor; this "spontaneous" tendency is the only natural law which can correctly be termed that of the *origin* of species; all that Mr. Darwin and his disciples attempt to explain is the survival and propagation of certain among the diverse forms thus resulting in preference to others. Throughout the whole of Mr. Wallace's volume he appears to have no consciousness that his theory does not go to the root of the matter. When once the tendency to change has set in, there can be no doubt that "Natural Selection," "The Survival of the Fittest," whatever you like to term the principle, is one among many causes which tend to the perpetuation of certain forms. When, however, Mr. Darwin asserts, "I am convinced that Natural Selection has been the main, but not exclusive, means of modification,"* I am by no means prepared to go with him to that extent. Some of Mr. Darwin's disciples go even further, and seem to consider it, in fact, as almost the only means.

There is no phenomenon in Natural History which is more thoroughly relied on by the advocates of Natural Selection as furnishing a decisive argument in favour of their theory, than the one which forms the subject of the longest of the essays in Mr. Wallace's volume, that of Mimicry or Mimetism. I propose, therefore, to occupy the greater part of this paper with an inquiry how far the facts which have been adduced support the conclusions first brought prominently forward by Mr. Bates in his "Naturalist on the Amazons," and more fully elaborated and illustrated by Mr. Wallace. There can be no doubt about the frequent occurrence of "protective resemblances" in the animal kingdom. Certain classes of animals enjoy, from various causes, exceptional immunity from the attacks of their natural enemies. In order to share in these immunities, it is found that other animals, belonging to an entirely different class or order, whilst retaining all the structural characters of their own class, so closely resemble in external features of colour and form particular species of the favoured races as to be readily mistaken for them. How do the advocates of the theory of Natural Selection attempt to account for this superficial resemblance? By the continuous preservation, through countless generations, of those particular individuals which spontaneously approach most nearly to the ultimate forms.

Now, there are two principles admitted or insisted on by every advocate of Darwinism, which it is necessary to bear very clearly in mind in the following argument. The first is, that, in a state of nature those differences which ultimately become specific or generic are brought about by exceedingly slow gradations. And it is obvious that it must be so. For if by chance any strongly abnormal form is produced, even should it survive to generate offspring, which is in itself doubtful, it must necessarily cross with other less abnormal individuals, and its descendants would thus have a tendency to revert towards the parental form. On this point Mr. Darwin himself says: "It may be doubted whether sudden and great deviations of structure, such as we occasionally see in our domestic productions, are ever permanently propagated in a state of nature."† And again, "Natural Selection always acts with extreme slowness."‡ The other point which I wish to be borne in mind is, that no change can possibly take place by the process of Natural Selection which is not directly of advantage to the individual. On this point again all the supporters of the hypothesis are agreed. Mr. Darwin distinctly affirms that "only those variations which are in some way profitable, will be preserved or naturally selected;"§ and Mr. Wallace even more emphatically speaks of "the principle which Mr. Darwin so earnestly impresses upon us, and which is, indeed, a necessary deduction from the theory of Natural Selection, namely, that none of the definite facts of organic nature, no special organ, *no characteristic form or marking*, no peculiarities of instinct or of habit, no relations between species or between groups of species—can exist, but which must now be or once have been *useful* to the individuals or the races which possess them."‖

We have, therefore, established at the outset these two data: that the passage from the ordinary to the mimetic form is effected by a number of exceedingly small steps, and that every one of these changes must present some advantage to the species which undergoes it. Now let us apply these two principles to the recognised facts of Mimetism; and for this purpose we may take a single instance, one of the most remarkable and best authenticated, recorded by Mr. Bates in his "Naturalist on the Amazons," and more fully in his paper on the "Lepidoptera of the Amazon Valley," in the "Transactions of the Linnean Society." There is in South America a tribe of butterflies of very gaudy colour, the *Heliconidæ*, which appear to enjoy exceptional immunity from the attacks of birds, from the exudation, when attacked, of a nauseous fluid, and are consequently extremely abundant. Another South American genus of Lepidoptera, the *Leptalis*, belongs structurally to an entirely different class, the *Pieridæ*, and the majority of its species differ correspondingly from the *Heliconidæ*, in their size, shape, colour, and manner of flying, being nearly pure white, and of the same family as our common cabbage butterfly. There is, however, one particular species of *Leptalis*, which departs widely in external facies from all its allies, and so closely resembles a species of *Ithomia* belonging to the *Heliconidæ*, as apparently not only to deceive the most experienced entomologists, but even to take in its natural enemies also, and, although perfectly harmless, to share the immunity of the butterfly it simulates. Mr. Bates and Mr. Wallace have both attempted to show, with great ingenuity and plausibility, that this entire change from the normal form to that resembling the *Ithomia* has taken place through the agency of natural selection acting through a long series of generations. I believe, however, on careful examination, the line of argument will be found to break down, and that at its very outset, on the ground that the early stages of the transformation will be perfectly useless for the protection of the species.

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* Paper read before Section D of the British Association, at Liverpool, September 20th, 1870.

* "Origin of Species," 4th ed., p. 6. † Ibid, p. 47. ‡ Ibid, p. 121. § Ibid, p. 13[illegible]

‖ "Contributions to the Theory of Natural Selection," p. 47.

Applying the rigid test of mathematical calculation to the problem, I think it may safely be assumed that it would require, at the very lowest calculation, one thousand steps to enable the normal *Leptalis* to pass into its protective form. Mr. Bates indeed assumes that the change may have taken place much more rapidly, but this appears a very unsafe and unsupported deviation from the sounder principle laid down by Darwin and Wallace. It is indeed obvious that any marked variety resulting suddenly must inevitably revert, as already observed, more and more towards the parent type by crossing, unless, indeed, we are to suppose that a pair, male and female, are simultaneously produced with a deviation in exactly the same direction, and that their offspring keeps itself apart, interbreeding only with itself as a separate colony,—an assumption contrary to all experience. At all events, we may safely say that within the historic period no such change has been effected within a vastly larger number of generations, where human agency has not come into play. The next step in my argument is, that the smallest change in the direction of the *Ithomia*, which we can conceive on any hypothesis to be beneficial to the *Leptalis*, is at the very lowest one-fiftieth of the change required to produce perfect resemblance. I believe myself that a very much larger fraction, say one-fourth or one-third, would be practically useless; as I am told by practical entomologists that birds will distinguish with accuracy caterpillars suited for their food from other species scarcely distinguishable to our eyes, which are not so suitable. For the sake of argument, however, I will suppose that a change to the extent of one-fiftieth is beneficial to that small extent *after which* natural selection may begin to come into play. Mr. Wallace, indeed, argues that an infinitesimal and inappreciable distinction may make the difference of a slightly longer span of life being allowed to the butterfly, to lay its eggs in safety; but this is a deductive piece of reasoning derived from the theory, because necessary to it, and not inductive observation from nature; and I altogether decline to be carried further, for the sake of the theory, than the limit I have indicated. Suppose a parallel instance: that our common brown owl has a *penchant* for mice, while moles are abhorrent to its palate; is it conceivable that, supposing a mouse was born approaching a mole by the one-hundredth part in external appearance, say with feet a fraction of a line broader, or eyes slightly deeper set, the shortest-sighted of owls would for a moment mistake *Mus* for *Talpa*? Or, a still more parallel instance: suppose a blue-bottle fly were born blessed with a slightly narrower waist, or a faint band of yellow on its body, will any one maintain that it stands the least chance of escape from destruction by those birds which do not feed on wasps? And no one who has examined Mr. Bates's or Mr. Trimen's beautiful drawings, or, still better, the insects themselves, will say that I have exaggerated the extent of the passage from the normal to the imitative *Leptalis*.

If, therefore, this reasoning is sound, one thousand steps being necessary to effect this change in external appearance, and one-fiftieth of the whole change, or twenty steps, being the smallest amount that is really profitable to the animal, it follows that the first twenty steps of the transformation are not due to natural selection, but must have taken place by an accumulation of chances. Let us investigate the value of this chance. Suppose there are twenty different ways in which a *Leptalis* may vary, one only of these being in the direction ultimately required, the chance of any individual producing a descendant which will take its place in the succeeding generation varying in the required direction, is $\frac{1}{20}$; the chance of this operation being repeated in the same direction in the second generation is $\frac{1}{20^2}$ or $\frac{1}{400}$; the chance of this occurring for *ten* successive generations (instead of twenty, as I have assumed above) is $\frac{1}{20^{10}}$, or about one in ten billions. Now another factor comes into the calculation, and that is the number of individuals among which this chance is distributed. Mr. Bates and Mr. Wallace agree in stating that both in South America and in the Malay Archipelago the imitative species are always confined to a limited area, and are always very scarce compared with the imitated species. We will assume that the number of individuals of the imitative *Leptalis* existing at any one time is one million; the chance of there being among these million a single individual approaching the *Ithomia* to the extent of one-hundredth is $\frac{1000000}{10000000000000}$, or the chance against it is ten million to one.

It will be seen that in the above calculation I have endeavoured to throw every advantage into the scale of the natural selectionist. I believe myself, and I think most naturalists will agree with me, that vastly more than a thousand generations, each characterised by a small change, must be conceded; and that, on the other hand, a change to the extent of even greatly more than one-fiftieth would be absolutely useless. This idea receives great confirmation from observing the most wonderful identity of the marking in the mimicked and mimicker. If a rough imitation is so useful, it must be a mere freak of Nature to produce so absolute an identity, and we are landed in the dilemma that the *last* stages are comparatively useless. If, again, I had carried on the calculation to $\frac{1}{20^{20}}$ instead of $\frac{1}{20^{10}}$, it would have been difficult to have stated the result in figures; and if, on the other hand, it is objected that a million is too low an estimate of the number of individuals existing at one time, and a hundred million or a thousand million is substituted (an altogether inconceivable estimate for a rare conspicuous butterfly limited to a small area *), the result will not be materially affected. For, supposing the chance is reduced from one in ten million to one in ten thousand—and it is said that the world has existed quite long enough to give a fair chance of this having occurred once—it is not a solitary instance that we have. Mr. Bates states that, in a comparatively small area, several distinct instances of such perfect mimicry occur; Mr. Wallace has a store in the Malay Archipelago; Mr. Trimen records several of wonderful beauty and exactness in South Africa; and the more attention is turned to the subject, the more numerous do instances of mimicry become.

I have left out of account altogether those still more remarkable instances, which are even more difficult to explain on the theory of natural selection (as the number of steps must be infinitely greater), in which animals not only imitate others belonging to entirely different natural orders, as Diptera mimicking Hymenoptera, and caterpillars snakes, but where they resemble inanimate objects. The weird and uncanny resemblance of the *Phasmata* and *Mantides* to dry leaves and sticks has long been known: not only is the veining of the leaves accurately reproduced, but the attacks of parasitic fungi are simulated; and Mr. Wallace records instances of larvæ bearing the most minute resemblance to the droppings of birds, and spiders to the axillary buds of plants. Through what countless generations must these transformations have been effected! and by what mathematical formula could we express the chance against their occurrence, if natural selection only had been at work in their production? The difficulties in the way of the natural selection explanation are also materially increased when we find, as is often the case, that it is one sex only (the female) which undergoes these mimetic changes, and that the changes have to take place simultaneously in the direction of colour, size, form, and habit.

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* The latter number would give 150 individuals per acre over an area 100 miles square, or 50 per acre for an area as large as Ireland.

It may now fairly be asked, if the principle of natural selection is abandoned as the main cause of these wonderful modifications, what other theory can be substituted in its place? I do not know that the objector to a theory is always bound to provide another theory as a substitute. Mr. Darwin, in his "Variation of Animals and Plants under Domestication," quotes with well-deserved approval Whewell's aphorism, that "Hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of error." Mr. Darwin's and Mr. Wallace's hypothesis of natural selection has been of signal service to science; but if this hypothesis has been too rashly handled and too widely applied, it may be equally serviceable to point out its incompleteness or its error, as the first step to a still more scientific explanation. In the following remarks, I merely wish to call the attention of naturalists to one or two points which I think have almost been lost sight of in the discussion.

I have already adverted to the inaccuracy of the title of Mr. Darwin's great work, "The *Origin of Species* by Means of Natural Selection." The opponents of Darwinism, even so acute a reasoner as the Duke of Argyll, appear to see no alternative between the theory that species have arisen through the agency of external causes, and the theory that species have remained immutable since their creation. I can accept no such alternative. Indeed we may say that external influences *cannot* be the primary cause of the transmutation of species. The utmost claimed by the theory of natural selection is, that it selects the fittest from already existing so-called "spontaneous" varieties. Every page of Mr. Darwin's work teems with reference to this pre-existing tendency to variation, with respect to which he says: "Our ignorance of the laws of variation is profound." Mr. Bates, when speaking on the subject of mimicry, makes the following very remarkable admission:—"It would seem as though our *Leptalis naturally produced* simple varieties of a nature to resemble *Ithomiæ*."* By a careful study of the context, I can only conclude that Mr. Bates means the same thing by his "natural" varieties as Mr. Darwin does by his "spontaneous" variations, namely, an innate tendency to vary *not caused by natural selection*, but on which tendency natural selection operates, and without which it would be perfectly inoperative. The use of the term "spontaneous" is open to objection from a philosophical point of view. It either means that the phenomena in question are subject to no law, or that they are the result of some law with which we are unacquainted. The former hypothesis will probably be rejected by every scientific naturalist, and must be utterly abhorrent to the believer in a "Reign of Law." This tendency to variation in the offspring meets us on every side in our investigation of nature. Every gardener knows how uncertain is the produce of seeds compared with the produce of buds or offshoots from the same plant. The ordinary mode of obtaining new varieties of strawberries or other fruits is from seeds. An endless variety of the commonest florist's flowers is produced by sowing seeds from the same capsule. Of the laws of this variation we are, as Mr. Darwin says, "profoundly ignorant;" but it does not follow that a patient interrogation of nature pursued in the true Darwinian spirit, may not reveal to us something of these laws. Of one thing we are certain, that natural selection here plays no part. If then we must admit that the first beginning of change takes place without the operation of this principle, why should we claim for it the main, almost the exclusive agency, in the changes which follow? Some other principle, at present unknown to us, originates these variations; what right have we to say that this principle, whatever it may be, then ceases to act, instead of being the main agent in all the other subsequent changes?

But are we limited to negative evidence in tracing the transmutations of species mainly to some unknown internal law? A single sentence in Mr. Wallace's Chapter on Mimicry seems to me pregnant with results for the future inquirer. He incidentally remarks how frequently it is the case that, when mimicry has once set in by the action of natural selection, new habits and instincts come into play to assist in the mimicry. It does not, however, appear to occur to Mr. Wallace to trace any connection between the instinct and the mimicry. The connection will be found, I believe, to be very close. Passing by for the moment any definition of instinct, let us trace its range in the organised world. From the whole vegetable kingdom it is conspicuous by its absence. In the lowest classes of the animal kingdom, the Protozoa and Cœlenterata, it is found, if at all, in a very low form; and though there is a popular superstition that oysters may be crossed in love, yet we cannot attribute to the Mollusca as a class any strong development of the instinctive faculty. When, however, we come to the Articulata, and especially to the Insecta and closely allied Arachnida, we meet at once with developments of instinct rivalling, if not exceeding in perfectness, those found in the highest forms of animal life. In the lower orders of Vertebrata again, the Pisces and Reptilia, we apparently come to a retrogression in the instinctive faculty, which is once more strongly developed in the Aves and Mammalia. Now let us compare this with what is known of Mimicry. From the vegetable kingdom it is absent. There are, it is true, resemblances, and resemblances of the most wonderful and perfect kind, in the marking and venation of the leaves of plant belonging to entirely different natural orders, equal in extraordinary closeness to those of which I have spoken in the animal kingdom; but these are in no sense mimetic or protective. Mere protective resemblances of colour I consider of far less importance than of form or habit; since colour may unquestionably be affected directly by the external circumstances of light, &c., and varies "spontaneously" in both the animal and vegetable kingdom to a far greater extent than does form. In the lowest forms of animal life we have no well-authenticated instances of mimetism, the most striking among the Mollusca with which I am acquainted is one pointed out to me by Mr. G. S. Brady in the beautiful *Lima hians*.* But when we come to insects, we find protective resemblances of the most extraordinary kind, in marking, in form, in habit, presented to us on every side. Among fishes and reptiles the principle appears to be again comparatively in abeyance, and to be once more strongly developed in birds. The parallelism is indeed almost complete. In short, the power of mimetism, as far as is known at present, runs almost *pari passu* with the development of the nervous system.

But what is instinct? Modern naturalists are pretty well agreed in abandoning the old distinction in kind between reason and instinct, and in considering the nest-building instinct of birds and the cell-constructing instinct of bees, as but a lower form of the same faculty which we call reason in ourselves. It is admitted that this instinct teaches the bee which flowers to rifle for its honey, and even to modify its habits in accordance with the circumstances in which it is placed; but, according to the prevalent theory, it has no power to modify its proboscis so as to enable it to obtain the honey from the flower, or to modify its wings to suit to its new habit. In short its own body is almost the only thing over which the animal has no power. To me such a restriction appears to be unphilosophical. I cannot but believe in the existence of an unconscious Organising Intelligence, an idea which Mr. J. J. Murphy has ably and logically advocated in his "Habit and Intelligence." And if this inherent innate power of change is admitted, it at once harmonises the tendency to variation which exists in all created beings, with the perpetuation of those forms best adapted to resist the struggle of life, and lends to natural selection the assistance of a fellow-worker far more powerful and of more universal operation.

A powerful argument in favour of this view may be drawn from Mr. Wallace's volume. Every reader of that book must have been struck with the remarkable manner in which he completely abandons and casts aside his own theory when he comes to treat of man. Natural selection is amply sufficient to account for all the other transmutations in the animal kingdom; only give time enough, and it is competent to develop the elephant out of the *Amœba*—the one step in the animal creation which is beyond its power is that from the ape to man; all the infinite forms of the brute creation have resulted from this principle,—to produce the different races of mankind some-other power is needed. In a singularly able review of this work in the *Archives des Sciences Physiques et Naturelles*, M Claparède, of Geneva, points out with great acumen the singular inconsistency of this reasoning; and shows how great a want of faith in his own principle it betrays on the part of its author. Mr Wallace's line of argument is very interesting. We may take only a single instance. Man is the only terrestrial mammal with a bare hairless back. All savage nations feel the want of a covering to their back; in cold countries to protect them from the cold, in hot countries to protect them from the heat of the sun. It is impossible to conceive, therefore, that this absence of covering was ever directly beneficial to the race or the individual; and hence it cannot have been produced by the operation of natural selection; but must have been in some way connected with those reasoning powers which lead to the construction of clothing and dwellings on which his civilisation so largely depends. Mr. Wallace, however, appears to forget that he had previously stated his conclusion that "those great modifications of structure and of external form which resulted in the development of man out of some lower type of animal, must have occurred *before* his intellect had raised him above the condition of the brutes."* This principle, therefore, whatever it may be, other than natural selection, which produced man's bare back, must have been in operation before the intellect of man was developed. This strange inconsistency of Mr. Wallace's appears to result from the fact that he is unable to shut his eyes to the inevitable conclusion that the development of man from the ape, and the production of the different races of mankind, have not resulted from the operation of natural selection, pure and simple, but that this principle has been powerfully assisted by man's reasoning faculties. This reasoning seems to me perfectly sound and inevitable, admitting, for the sake of argument, Mr. Wallace's hypothesis, that man *is* descended from the apes; but, if we consistently believe in the action of general laws which govern the whole of animated nature, we must carry the argument back a step further. Reason is but a higher development of instinct. If man's reason has assisted him so to modify his body as to adapt himself to the circumstances with which he is surrounded, we are unable to bring forward any valid argument why the instinct of animals should not also assist them to modify their bodies, by slow and gradual degrees, so as to adapt them to the circumstances with which they are surrounded.

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* Transactions of the Linnean Society, vol. xxiii., p. 5[illegible]2.

* See NATURE, Vol. ii., p. 376.

In the essay alluded to above, M. Claparède, himself one of the few genuine Darwinians among French writers, points out the dangerous and unscientific manner in which the theory of natural selection is made, in the hands of its too zealous advocates, to explain phenomena which are probably due to other causes. The discovery of this law marked an era in the history of natural science, and gave a wonderful impulse to original research. The danger now is that the law will be pressed into services which have no claim upon it; and that, in the hands of injudicious partisans, it will become a hindrance rather than an aid to science, by closing the door against further investigations into other laws which lie behind it. To claim for Natural Selection the main agency in the creation of the countless forms of organic life with which we are surrounded, is straining it beyond its strength. An era of equal importance will be marked by the discovery of the law which regulates the tendency to variation which must necessarily underlie natural selection.

The argument of "design" was undoubtedly pushed by pre-Darwinian writers to too great an extent. The most recent phase of Darwinianism, however, is a complete denial of the existence of design in Nature. It is the carrying into Natural Science of the Hobbesian principle of Self-love. Every individual and every species exists for its own advantage only, and has no *raison d'être* except its own welfare. To my mind the beauties and wonders of Nature seem, on the other hand, to teach a different lesson, that,

> All are but parts of one stupendous whole,
> Whose body Nature is, and God the soul;

that there are laws, albeit almost unknown to us—not laws merely of external circumstance, but laws of internal growth and structure,—which actively modify each individual organism, not only for its own advantage in the struggle for life, but for the higher end of subordinating every individual existence to the good of the whole.

ALFRED W. BENNETT

THE PROFESSORSHIP OF NATURAL HISTORY, QUEEN'S COLLEGE, BELFAST

IN a late number we announced that Professor Wyville Thomson, of the Queen's College, Belfast, had been appointed by the Crown to the Professorship of Natural History in the University of Edinburgh. This will necessitate the resignation by Professor Thomson of his chair in the Queen's College, Belfast, a resignation which we may presume will be made before the commencement of the next term, and a resignation in which some of our readers and many of our men of science will take an interest, for the places of honour or emolument open to the student of Natural Science in this country are so very few, that there is naturally much excitement when one of the few is to be filled up. Already we hear of a whole host of young and meritorious workers setting their faces towards the city that boasts to be the Athens of the North of Ireland. The mere mention of the names of Dr. Cunningham, who in the Straits of Magellan earned his Natural Science spurs so well, of Mr. E. Ray Lankester, whose numerous papers show an intimate acquaintance with zoology, of Dr. Macalister, whose comparative anatomy memoirs are so well-known, or of Dr. Traquair, whose papers on fossil fish and on the skull of recent Pleuronectidæ, are of high merit, not to name others, will show that the post of Professor of Natural History in the Queen's College, Belfast, will be contested for by a little army of well-educated and accomplished gentlemen, the selection of any one of whom would reflect credit on the College.

But a rumour reaches us that there may be no election to the Professorship after all—that the spirit of economy is to annihilate the spirit of competition; that, in order that the Government of this great country may save certain paltry trifling possibilities of pension, it is their intention to translate to Belfast one of the four Professors of Natural Science in the Queen's Colleges of Cork and Galway. It is necessary to explain how this can be done. Each of the Queen's Colleges had originally a Professor of Geology and Mineralogy, and a Professor of Zoology and Botany. Their income was that of a junior assistant in the British Museum, and for common decency's sake, it was found necessary to raise it; this was done on the condition that each of the Professors undertook to lecture on the subjects at the time lectured on by his colleague, on the death or resignation of that colleague, without further increase of pay. So when Prof. Dickie, who was Professor of Botany and Zoology in the Queen's College, Belfast, resigned, on his removal to Aberdeen, Prof. Thomson had to lecture in zoology and botany, in addition to his own subjects of geology and mineralogy. Thus it happens that should the Government confer the vacant Belfast chair on one of the four existing Professors of Natural Science in the other two Queen's Colleges, his post in the college which he leaves will be filled up by his colleague, and the Crown will have to deal in the matter of pension, &c., with but four persons instead of with five, as they will have if they appoint a candidate who is not one of these four Professors to the vacant post. Nor can the Crown confer this Professorship on one of the present Professors, and then fill up the place thus left vacant by a new appointment, because, although the yearly salary of the colleague of the Professor thus elected will not be increased thereby, yet his fees, to a slight extent, will; and so, to break the bargain made, would be to the detriment of the individual—a thing, we believe, no Government would do. But why, we ask, should they, for a paltry saving, do detriment to the cause of Science in this country—courted when she is needed—kept at more than arm's length when it is imagined she may be done without? Science is but badly cared for in our country, and we here allude to the above facts for the purpose of urging those to whose care this appointment falls, to forget, for the once, all considerations except those for the good of the College, and to quicken the already expanding life of the Queen's University in Ireland by the infusion of fresh blood into this one of its Colleges.

It is in the interest of Science that we write, not in the interest of candidates, one of whose names we would not mention above another.

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* "Contributions to the Theory of Natural Selection," p. 319.

PITCHER PLANTS

DIFFERENCE of opinion has been expressed as to the nature and use of the liquid found in the so-called pitchers of various plants, such as Nepenthes, Rafflesia, and certain Orchidaceæ. The popular idea that these curious receptacles collect pure water for the refreshment of the thirsty in arid places, would seem to be set at rest, by a consideration of the fact that these plants grow in moist and marshy places. There would seem, moreover, to be some improbability that plants should secrete pure water.

In this country, where these plants are grown under exceptional conditions, there is some difficulty in settling these questions experimentally. In such cases, extraneous water often finds its way into the pitchers, so that several ounces may frequently be gathered from a single receptacle of Nepenthes, the greater part of which is accidental.

In August last I had an opportunity of collecting the liquid from two flowers of Coryanthes, one of the Orchidaceæ, which had just opened, in one of the well-known stove-houses of Mr. Wilson Saunders.

Though the quantity collected was small, amounting only to about three cubic centimetres, or 1·18 cubic inches, an examination showed the following properties:—

Clear and somewhat glutinous in consistence. Possessed of a high refractive power, and a specific gravity of 1·062.

Odour pleasant but faint, becoming more marked by a gentle heat. Neutral to test papers. Becoming milky, by concentration on the water-bath, it finally yielded a transparent gum, insoluble in alcohol.

Oxalates produced no precipitate of lime, but basic lead acetate gave a curdy reaction. Concentrated hot sulphuric acid blackened the liquid.

Although the taste was not acrid, the mawkish flavour would render it quite unpotable.

This examination therefore proved the liquid to be something else than pure water.

100 parts of liquid contained:—

Water and volatile oils . .	98·51
Non-volatile residue	1·49
	100·00

G. B. BUCKTON

SPECTROSCOPIC OBSERVATIONS OF THE SUN

PROF. C. A. YOUNG has obligingly sent me an account of his recent work, which is very rich in promise, as he tells me that he has now the dispersive power of 13 prisms of heavy flint, each with an angle of 55°. It is now some time ago since I announced to the Royal Society that over spots prominences, built up of different vapours, were sometimes observable by means of their lines, *bright and thin*, overlying the thick absorption lines in the spot spectra. This observation is, I hold, a clear proof of the truth of the theory put forward by Dr. Frankland and myself, namely, that changes in spectra, notably the thickening of the lines, are due to pressure, and not to temperature; for according to the theory of exchanges, the bright prominence must be hotter than the absorbing vapour which underlies it, and still the lines are thinner.

Dr. Young has now observed these phenomena with exactly the same result. He writes to Professor Morton:—

"I write to inform you that last Thursday, Sept. 22, about 11 A.M. Hanover mean time, I was so fortunate as to see the sodium lines D_1 and D_2, reversed in the spectrum of the umbra of a large spot near the eastern limb of the sun. At the same time the C and F lines were also reversed, but with the great dispersive power of my new spectroscope I see this so often in the solar spots, that it has ceased to be remarkable.

"The figure gives the appearance of the sodium lines. In the umbra of the spot the D_3 line was not visible, but in the penumbra was plainly seen, as a dark shade, represented in the figure.

"I am not aware that this reversal of the sodium lines in a spot spectrum has ever been observed before; its reversal in the spectra of prominences is not very unusual. A small prominence on the western limb of the sun, which was visible the same forenoon, presented all the following bright lines, viz.: C, D_1, D_2, D_3, 1474·, b_1, b_2, b_4, 1989·5, 2001·5, 2031·, F, 2581·5, 2796·, and h; 15 in all.

"In the spot spectrum the magnesium lines b_1, b_2, and b_4 were not reversed, but while the shade which accompanies the lines was perceptibly widened, the central black line itself was thinned and lightened."

Further, Prof. Young has succeeded in obtaining photographs of protuberances on the sun's limb, of which he has been good enough to forward me a specimen. They were obtained by attaching a small camera to the eye-piece of the telescope and opening the slit somewhat widely, using the hydrogen line near G. He adds:—"As a picture, the little thing amounts to nothing, because the unsteadiness of the air and the maladjustment of the polar axis of the equatorial caused the image to shift its place slightly during the long exposure of three-and-a-half minutes which was required, thus destroying all the details. Still, the double-headed form of the prominence is evident, and the possibility of taking such photographs is established."

In a letter to myself Prof. Young adds:—"I should not have published so imperfect a success were it not that my engagement as a member of Prof. Winlock's eclipse party prevents me from following up the matter at present. The experiments were tried on the 28th, and on the 30th the equatorial was taken down to be packed up and sent to the rendezvous, at Alvan Clark's factory, where all the instruments are collected and put in order previous to sailing."

J. NORMAN LOCKYER

NOTES

SINCE our last issue the Joint Committee of the Royal and Royal Astronomical Societies and the Council of the British Association have met to consider the question of the Eclipse Expedition, and in consequence of these meetings Mr. Gladstone has been asked to receive a joint deputation to urge upon the Government the importance of the proposed expedition. The Joint Committee have appointed the Presidents of the Royal and Royal Astronomical Societies, the Astronomer Royal, and Mr. Lockyer to plead its cause; while the Council of the British Association will be represented by the President and officers of the Association, Sir John Lubbock, M.P., and Dr. Lyon Playfair, M.P. Up to the time of our going to press, however, no time had been fixed for the deputation to wait upon the Prime Minister.

THE medals in the gift of the Royal Society have this year been awarded as follows:—The Copley medal to Dr. Joule; the Rumford medal to M. Descloiseaux; and the Royal medals to Prof. W. H. Miller and Mr. W. Davidson.

PROF. SIMON NEWCOMB has arrived in this country from the United States Naval Observatory. His mission among us is to examine and report on the great Newall telescope. He will then proceed to Gibraltar to observe the approaching eclipse.

ALL members of the British Association will be concerned to learn that Dr. Hirst feels called upon by the pressure of his new duties to resign the General Secretaryship of the Association, an honorary post which he has long filled with the greatest advantage to Science.

AMONG the mass of correspondence which has recently reached us are several interesting letters on early notices of the Aurora, for some of which we hope to find room next week.

WE rejoice to see several men of science, with Professor Huxley at their head, coming forward as candidates for the London School Board. We shall be glad to be informed if this example is followed elsewhere.

WE read in the *Echo* that the Electric Telegraph has been put to a new use in Canada. At Mimouski, when the late earthquake came upon them, they sent at once to Quebec, a distance of 200 miles, to ask, "How do you feel?" While the operator there was at his work the shock arrived. He at once sent to Montreal, about 200 miles further on, to ask if they had felt it. They had just time to say "No" before the earthquake came up.

THE new buildings of the Glasgow University were formally opened on Monday last. The proceedings were conducted within the Hunterian Museum. The Duke of Montrose, Chancellor of the University, presided, and congratulated the principal professors upon the success which had attended their efforts, and that they had lived to see the opening day of the new University. The merchants of Glasgow had made princely fortunes, which was creditable to their talents and their industry; but it was still more creditable to them that they had made such a use of their riches as to enable this noble building to be erected for the education of the rising generation. After Prof. Lushington had delivered an address, Mr. A. Orr Ewing, M.P., stated that from subscriptions and from Government 254,000*l*. had been obtained, and 117,000*l*. had been received for the ground upon which the old college stood. Everything in connection with the new building was paid. Of the 150,000*l*. in public subscriptions, Glasgow had given nearly all. The various classes met on Tuesday.

WE believe that Mr. W. Spottiswoode will succeed the late Dr. W. A. Miller as Treasurer of the Royal Society.

ON the 15th inst., Dr Grey will read a paper at the Statistical Society "On the Claims of Science to Public Recognition and Support." We are glad that the attention of such a powerful body as the Statistical Society is to be so authoritatively drawn to such an important subject.

THE Polytechnic School of Zurich has been suddenly deprived of its chemical teachers. In addition to the death of Prof. Bolley, which we have already recorded, Prof. Hadeler, giving way to the demands of failing health, has relinquished his chair of Pure Chemistry.

IT was recently determined to erect a statue to Prof. Morse, the eminent American electrician and inventor of the telegraph which bears his name, and for this purpose subscriptions were opened in the States. The full amount has been very speedily subscribed, and the erection of a marble statue is to be at once proceeded with. It is to stand in the Central Park, New York, a suitable position having been willingly granted by the Commissioners.

IT is a question of some interest and curiosity how to compare the forces of steam and gunpowder. The following calculation, each step of which can be easily followed, may therefore be acceptable to our readers. The force exerted by an exploding charge of powder in a gun requires for its calculation two considerations, *viz.* the amount of force given to the shot, and the time in which that force is imparted. Taking as our example the 300-pounder Woolwich gun, the first element of the calculation is as follows:—One steam horse lifts 33,000lb. one foot in one minute, or 550lb. in one second. The 300lb. shot leaves the muzzle of the gun at the rate of 1,300 feet per second. Let H represent the force in horse-power in the shot, then, by the well-known equations $W = \frac{1}{2} m v^3$, and $m = \frac{w}{g}$,

$$H \times 550 = \frac{300}{2g} \times (1{,}300)^2 = \frac{300}{64} \times 1{,}690{,}000, \; H = 14{,}403{\cdot}409$$

The force, *i.e* the work in the shot, is therefore measured by 14,403·409 horse power. That is, it would require that amount of horse power acting for the space of one second to give to a 300lb. shot the velocity with which it is driven from the gun by the explosion of the charge (43lb. of gunpowder). But this is done by the powder during that minute portion of a second in which the shot moves down the bore of the gun The results of Captain Noble's chronoscope make it appear that this time is somewhat less than one two-hundredth part of a second. The force exerted by the powder must, therefore, be 14,403·409 × 200: that is 2,880,681·8 horse power. Some comparatively small considerations, as the friction of the shot in the gun, are neglected in this calculation. It does not therefore, err in excess, and is sufficient to give some idea of the enormous force exerted. Nor is it uniformly exerted throughout the whole time of the shot's movement in the gun, nor does the calculation above made necessarily give the greatest intensity of action. This much we may state, that at some *instant* during the 200th part of a second in the case taken, the force of the expanding gas was to be measured by nearly three million horse power.

THE unpublished manuscripts left by the late Sir James Simpson on the important subject of Hospitalism have been confided to the care of Mr. Lawson Tait, of Birmingham, for completion and editing.

IN addition to the outbreak of the long-quiescent volcano of Tongariro in New Zealand, to which we lately called attention, we hear from American sources that "a volcano, near San Rafael Valley, Lower California, which has been in a dormant state for years, has commenced a violent eruption, emitting columns of smoke and scattering ashes and cinders for miles around its base St. Diego telegrams say it is plainly visible from there."

THE islands in the Sea of Okotsch, off the north-east coast of Asia, are being visited by ships in search of seals. A vessel recently arrived at San Francisco from Jones Island, between lat. 52° and 53° and long 145° and 146°, with 11,500 seal-skins on board, and another vessel with a still larger cargo is expected. This island is half a mile in circumference, and is uninhabited, and is remarkable for the great abundance of seals.

CONSIDERING the many uses to which india-rubber is now applied, one of the most important being its recognised superiority over gutta-percha for deep-sea telegraphs, and remembering the fears entertained some time back of the probability of a decrease in the supply, owing to the exhaustion of the forests consequent upon the immense demand, it is gratifying to learn that the quantity of rubber exported from Para during the past year exceeded that of the previous year by 22,731 arrobas (an arroba is equal to about 25½lb), and by 241,250*l*. in market value. It is true that the more accessible rubber districts are becoming exhausted, and give a smaller yield than in former years, but the rubber-bearing country is so extensive, and its rivers so incompletely explored, that the newly-discovered sources will, no doubt, more than make up any deficiency arising from the exhaustion of the old. It is difficult, however, to obtain accurate or reliable information from those engaged in the collecting of the rubber. The continued demand for rubber, which is collected with comparatively little labour, and requires but little skill and experience, absorbs all the attention of the natives over other products, and the constant rise in its value so stimulates its production, that it is more than probable there will be for some years to come an annual increase in the quantity imported of at least ten per cent.

IN the State of Santander, Colombia, one of the most important sources of state revenue is the manufacture of so-called straw hats. These hats are chiefly made in the Bucaramanga district, and are of a very fine and white material, but still not equal to the celebrated Panama hats, as they soon become dirty, owing to the plait not being drawn sufficiently tight. The weekly sale of hats in the above district averages from 600 to 800 dozen, the lowest quality of which fetch about 1*l*. 4*s*. the dozen, and the finest quality often realising as much as 1*l*. 10*s*. to 1*l*. 16*s*. each. These latter are principally made near Zapatoca, which also has a large trade, but not to the same extent as Bucaramanga. They are for the most part exported to Havana and the United States, where there used to be a great demand for them, but now the trade seems to have somewhat diminished. In the first half of last year 250 cargoes were exported, and as each cargo contains 1,200 hats, some idea may be formed of the large quantities manufactured. The above is gathered from a report on the industrial resources of the State of Santander. The hats referred to are described as being made of a white kind of "straw." We have not seen any of the actual material, but think, in all probability, the so-called straw may be the split leaves of some palm, perhaps *Thrinax argentea*, which was imported in considerable quantities some years ago, and manufactured at St. Alban's into "chip" hats.

THE astronomical and meteorological observations made at the United States' Naval Observatory during 1867 have just been published in a large quarto volume. There is an appendix of reports on the observations of the total eclipse of the sun of Aug. 7, 1869, the various phases of which are beautifully illustrated by chromo-lithographs, and the various instruments made use of are particularly described.

A PIECE of meteoric iron fell on the 18th of September last near Santa Clara, California, in the barn-yard of Michael Sanor, where it set the straw and *débris* on fire. When picked up it was exceedingly hot, and hissed when thrown into water. The meteorite which fell in October 1869 in Stewart County, Georgia, will be analysed, and a full description of its fall and accompanying phenomena given in the next number of *Silliman's Journal*, by Prof. Willet.

IN consequence of the depression caused by the war, the private enterprise for working coal near Manita in Asia Minor has been postponed, but the Government is taking measures at length for working the coal formations near Constantinople for the artillery factories, carried on under the direction of Messrs. Siemens. This coal was known and worked seventy years ago, as described in old books of travels, and then abandoned. The Heraklea coal mines are going on slowly.

THE Government of India is now cooling in its reduction fit, consequent on the supposed deficiency in the Budget, and is paying more attention to the discharge of its duties. The appointment of an assistant curator for the Geological Museum of India has been authorised, and an increase allowed for the literary purposes of the department.

IT is reported from that interesting region, the Argentine States, that two enormous fossil skeletons have lately been discovered at Fray Bentos, but they are so large as to be beyond the local means of transporting them with safety.

A GREAT earthquake has taken place in the town of Santo Tomas, the capital, and in the district, of Chumbivilcas, department of Cuzco, Peru. On the 10th of July, about 1.30 P.M., it was felt as the people were coming out from mass. The upper part of the two side towers of the church fell in, the vaulted roof was rent, as well as the walls, and much more damage done. Other damage also took place in the town and district. On that same day the River Santo Tomas, which runs about a league to the west of the town, suddenly rose and overflowed its banks, producing great destruction to the farms, horses, and cattle, but no lives were lost. It was afterwards ascertained that by force of the shock the waters of the Lake Quenacocha had broken out into the river. The lake is about 20 miles in circumference, and lies at the base of the western chain of the Andes. In the town of Ccolqquemarca the tower and body of the church were injured, and many houses overthrown. Up to the 12th the earthquakes were felt every few minutes, at least every quarter of an hour, and the river was still flooded. On the 13th August there was a slight earthquake in the evening at Lima. On the 27th August an earthquake was felt in Chile—we presume at Valparaiso.

EFFORTS are being made to obtain improved instruments and to extend the observations at the Meteorological Observatory at Durban, in Natal.

IN continuation of our comments on the caution necessary in dealing with scientific statements in the Indian press, we may advert to a case in which the Welsh fasting-girl is eclipsed. A correspondent of the *Indian Daily News* at Nuddea affirms that a Sudra woman, forty-five years old, has abstained from food twenty-five years. She bathes twice a day. Of course it is to be added that many natives have satisfied themselves of the correctness of this statement, as many Welshmen did with regard to their fasting-girl.

THE *Inverness Courier* of October 13 states that a number of hollow glass globes of a dark colour, and measuring about eighteen inches in circumference, have lately been found washed ashore at various parts of the coast on the west side of the Island of Lewis. They are hermetically sealed, and have certain characters, such as IV or VI impressed on the sealed part. Some of them are partially filled with a colourless liquid. The question is asked: "Have these been used for some experiment made for the purpose of ascertaining the course of some ocean current?" Can any of our readers throw light on this subject?

IN a recent number of the *Pharmaceutical Journal* a paper appears, by Mr. Cooke, on the Guarana, the seeds of a tree termed the *Paulina sorbilis*, belonging to the order *Sapindaceæ*, and abundant in the province of the Amazonas. The fruit is scarcely as large as a walnut, and contains five or six seeds, which are roasted, then mixed with water and moulded into a cylindrical form resembling a large sausage, and finally dried in an oven. Before being used it is grated into a powder, very like powdered cacao in appearance. Two spoonfuls of the powder are mixed in a tumbler of water, and this drink is regarded as a stimulant to the nerves, and like strong tea or coffee is said to take away the disposition to sleep. The active chemical principle is an alkaloid which Dr. Stenhouse has shown to be identical with theine. Guarana contains more than double as much of this alkaloid as good black tea, and five times as much as coffee, the proportion being 5·07 per cent in Guarana. It is rather a singular coincidence that the same alkaloid should prevail in all the principal substances employed in a similar manner as beverages in different parts of the world, in the tea of China and India, the coffee of Arabia, the cacao of Central America, the maté of South America, and the guarana of Brazil. Guarana is a nervous stimulative and restorative.

AN attempt is again being made, with hope of success, to work the quicksilver mine of Punitaqui, in Ovalle Department, Chile. It was worked for the crown in the Spanish times, but the war of Independence and the Indian incursions stopped it, as the latter did again in 1830.

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE

On the Temperature of the Air at four feet, twenty-two feet, and fifty feet high.—J. Glaisher, F.R.S. In his opening remarks, Mr. Glaisher spoke of the erroneous opinions which were entertained previous to his balloon ascents with regard to temperature at different heights; it was supposed that the temperature of the air always decreased from the earth upwards, and followed some constant law; this was, however, found not to be the case; and in the Report to the British Association at Nottingham, in 1866, the conclusions were, "that the law of decrease of temperature with increase of elevation was variable throughout the day, and also in different seasons of the year; that at about sunset the temperature was sensibly the same up to 2,000 feet; and that at night (conjectures from the results of the observations taken in the only two night ascents) the temperature of the air increased from the earth upwards." It was therefore evident that a very large number of ascents would have to be made to determine the real laws.

Fortunately, in the second year of the balloon experiments, he placed a dry and wet bulb thermometer at the height of 22 feet above the ground, readings of which have been taken four times daily, viz. at 9 P.M., noon, 3 A.M., and at 9 P.M. Although from these observations, and also from those made at the different ascents, it was known that sometimes readings at the higher point were above those at 4 feet from the ground, no particular attention was paid to the above readings until after the results of the observations made in M. Giffard's Captive balloon were known; these, however, were of such importance, proving that "the decrease of temperature with increase of elevation had a diurnal range, and was different at different hours of the day, the change being greatest at mid-day, and least at or about sunset (see Report to the B. A. for 1869 at Exeter), and that sensible changes occurred within 30 feet of the earth," that Mr Glaisher caused the observations taken at the height of 22 feet to be reduced; collecting the observations recorded during the period 1867-1870, the differences between the readings of the two thermometers were taken, and affixing the sign *plus* (+) to the difference when the temperature at the higher elevation exceeded that at the lower, and the sign *minus* (−) when *vice versâ*.

On taking the monthly means of these differences, it was proved that the *mean temperature* of the air at 22 ft. high differs from that at 4 feet by:—

	9 A.M. deg	Noon. deg	3 P.M deg.	9 P.M. deg.
In January	+ 0·5	+ 0·2	+ 0·4	+ 0·6
,, February	+ 0·2	0·0	+ 0·4	+ 0·5
,, March	— 0·3	— 0·2	0.0	+ 0·4
,, April	— 0·6	— 0·5	+ 0·2	+ 0·5
,, May	— 0·6	— 0·4	— 0·4	+ 0·5
,, June	— 0·8	— 0·9	— 0·6	+ 0·8
,, July	— 0·8	— 0·8	— 0·8	+ 0·7
,, August	— 1·0	— 0·5	— 0·1	+ 0·9
,, September	— 1·0	— 0·6	0·0	+ 0·7
,, October	— 2·2	— 0·1	+ 0·6	+ 1·0
,, November	+ 0·2	+ 0·1	+ 0·6	+ 0·8
,, December	+ 0·5	+ 0·3	+ 0·4	+ 0·4

Therefore the monthly mean temperature of the air at 22 feet was higher than at 4 feet, at all hours of the day and night, in January, February, November, and December; in the afternoon and during the night hours in the months of March, April, August, September, and October; in the evening hours and during the night, in the months of May, June, and July; and that the results in one year agreed very closely with those in the same months in other years.

By selecting the largest number with a + sign, and the largest with a — sign in each month, it was found that in the winter months the temperature at 22 feet high ranged from 2° to 4° above, and from 1° to 2° below that at 4 feet, and in the summer months from 4° to 5° above and from 4° to 5° below that at 4 feet high.

The ratio of — readings to + was, in January and February as 1 to 5 at all hours. In March, April, August, and September, during the day, one of equality. In May, June, and July, as 3 to 2 during the day hours. In October, as 1 to 4; in November, 1 to 7; and in December, as 1 to 10. At the hour of 9 P.M. throughout the year, it was as 1 to 7.

Thus the — sign preponderates, indicating lower temperature above, during the day hours, in the months of May, June, and July; the two signs are about equal in number in the months of March, April, August, and September, and the + sign preponderates, denoting higher temperature during the day and night, in January, February, October, November, and December, and during the night throughout the whole year.

A second thermometer, carefully protected from radiation, was placed in the middle of the year 1869 at the height of 50 feet, and since then its readings have been regularly taken.

The *mean monthly temperature* of the air at 50 feet high was found to differ from that on the ground as follows:—

1869	9 A.M. deg.	Noon deg	3 P.M deg	9 P.M. deg.
In October	+ 0·2	— 0·5	+ 0·7	+ 1·5
,, November	+ 0·6	+ 0·5	+ 0·8	+ 1·4
,, December	+ 0·9	+ 0·3	+ 0·5	+ 0·5
1870				
,, January	+ 1·1	+ 0·3	+ 0·7	+ 0·9
,, February	+ 0·1	— 0·3	+ 0·3	+ 0·5
,, March	— 0·3	— 1·8	— 0·7	+ 0·7
,, April	— 0·9	— 2·2	— 1·7	+ 1·4
,, May	— 2·4	— 3·6	— 2·8	+ 1·1
,, June	— 2·4	— 3·8	— 3·1	+ 1·1
,, July	— 1·8	— 2·9	— 2·8	+ 1·1
,, August	— 1·7	— 2·7	— 2·0	+ 1·7

Thus we have the unexpected results that the *mean monthly temperature* of the air at 22 feet and at 50 feet high is higher during the evening and night hours throughout the year than at the height of 4 feet, and also higher night and day during the winter months.

In conducting the above investigation, it was known that the clouds had great influence on the temperature; Mr. Glaisher therefore selected those days with a sky covered with dense clouds, and it was found that there was on such days no difference between the temperature at the heights of 4, 22, and 50 feet. At the height of 50 feet, in the summer months, the temperature during the day was frequently 6° or 7° lower, and at night 5° or 6° higher than at 4 feet.

SECTION B.—CHEMICAL SCIENCE

Experiments on the Preservation of Building Stones.—Prof. A. H. Church, M.A.

This paper gave a brief account of a process for preserving stone in which solution of monocalcic phosphate, barium hydrate, and dialysed silica are successively employed. Very numerous and extensive experiments have been made with this process upon public and private buildings. The New Midland Terminus, St. Pancras, has been treated with these solutions, and so have the Chapter House, Westminster, and portions of Canterbury Cathedral and the Houses of Parliament. The process is now the property of the Patent Concrete Stone Company.

Contributions to Mineralogical Chemistry.—By Prof. Church, M.A.

The author gives an account of his researches into the constitution of numerous mineral species. The paper gives a list of the nine species which he has discovered, including the rare cerium phosphate from Cornwall. The paper also contains the chief results which its author has obtained in the analyses performed by him during the last seven years with a view to the revision of the formulæ of the mineral phosphates and arseniates. References to the original memoirs are given under the description of each mineral. Prof. Church lays great stress, in the prefatory remarks to his paper, on the importance of ascertaining the hygroscopic water of minerals, and of obtaining the samples for analysis in a state of freedom from admixture by foreign and intruding bodies.

SECTION D.—BIOLOGY

Department of Zoology and Botany

Prof. Huxley, President of the Association, read a paper on the relations of *Penicillium*, *Torula*, and *Bacterium*, in which he showed the extreme probability, if not amounting to absolute demonstration, perhaps going as far towards it as the extreme difficulty of the investigation of the subject admitted, that these two latter forms were but stages of the first. Prof. Huxley gave an account of his experiments and researches, which were carried on with extreme care, and mentioned that he had become convinced that the movements of the minute germs so much alluded to by experimenters was to be accounted for by the explanation that it was the well-known Brownian movement. The bearing of this important paper seemed to be to account for the presence of Bacteria on more ordinary principles than those of spontaneous generation.

Mr. A. W. Bennett read a paper on the *Theory of Natural Selection, looked at from a Mathematical Point of View.**

Prof. Huxley paid a high compliment to the author of this paper, which he said was the first that he could recollect having heard read at Section D, which, taking up the side against Mr. Darwin, still did so in a proper and philosophic manner. He had often mentioned objections that had struck him to Mr. Darwin, who always, however, had ready a quiver full of facts which generally settled the question, and he thought it probable that when Mr. Darwin read Mr. Bennett's paper, he would have a few facts ready which might alter his view of the case.

Dr. Anton Döhrn read a paper on the *Foundation of Zoological Stations*. In this he insisted on the importance of there being zoological stations throughout the world, just as there now were meteorological and astronomical stations. The author mentioned that he had commenced at his own cost the establishment of one such station at Naples. In it he would have large and small aquaria, a constant flow of salt water, microscope rooms, and there would be a resident working zoologist, a library, and a series of bed-rooms for foreign naturalists. At this station, not only would collections of living marine animals be made for purposes of study and for supplying the aquaria of Florence, Berlin and Vienna, but a collection in spirits would also be kept to supply working zoologists at a distance with the means of research. The President of the Section, Professors Newton and E. Perceval Wright, spoke strongly in favour of the station to be established at Naples, and expressed the hope that perhaps at some future day others would be established at such outposts as Dingle in Ireland, Aden in the Red Sea, &c. &c.

Dr. J. Barker read a paper in which he described an interesting little Infusorium called *Pleuronema doliarium.*

Professor Dickson read notes *On the Embryo of the Date Palm.* The author criticised the descriptions given in the books where the slit of the cotyledon is represented sometimes as a transverse fissure near the upper part of the cotyledon (Schnizlein's Iconographia), or as a vertical one near the upper part (Le Maôut and Decaisne). Dr. Dickson described the slit as a vertical one, situated near the base of the cotyledon, and called attention to the fact that there was here no fixed relation between the medial plane of the cotyledon and that of the seed, an exception to the general rule for monocotyledons, as laid down by Hofmeister, to the effect that in vertical seeds (erect or pendulous) the medial plane of the cotyledon coincides with that of the seed, while in horizontal seeds the plane of the cotyledon is at right angles to it.

Mr. Tyerman exhibited drawings of a young healthy plant of the double cocoa-nut (*Lodoicea sechellarum*), which he had succeeded in growing at the Botanical Gardens, Liverpool. He mentioned that he had some difficulty in keeping the strangely elongated basal portion of the cotyledon from penetrating too deeply into the ground, but after it had grown to a distance of some two feet, the germ developed a single sheathing leaf, and then shortly afterwards two of the ordinary characteristic leaves of the plant made their appearance.

Profs. Balfour and Wright congratulated Liverpool on having such an interesting collection of plants as that they had seen at their Botanical Gardens, and on having so able and intelligent a curator as Mr. Tyerman, and hoped that when next they visited Liverpool they would find at the gardens a range of glass worthy of the gardens and of the most interesting collection of plants at present preserved in a few tumble-down houses.

On the Staperaythr Whale of the Icelanders.—Mr. Bird.

On the Affinities of the Sponges to the Corals.—Mr. W. S. Kent. The author criticised the views of Haeckel.

On the Effects of the Pollution of Rivers on the Supply of Fish.—Sir James Alexander. This paper gave rise to a lengthened discussion.

Mr. Moore, director of the Liverpool Museum, exhibited some of Captain Mortimer's Sea Aquaria, by means of which he had been enabled to transport from the tropics many delicate marine fishes, crustacea, and sea anemones. He also exhibited a young Lamatin and the jaw of a fish, the rami of the mandible of which, instead of being united by a ligamentous union and forming a bony symphysis, were most compactly hinged together, admitting of a considerable amount of lateral motion. Mr. Moore also exhibited a stuffed skin of that most wonderful shark from the Seychelles, called *Rhinodon typicus.* This was the only perfect specimen known in any museum. This shark, which grows to the length of 50 feet and upwards, was known to Mr. Swinburne Ward, late Civil Commissioner of the Seychelles, as common off those Islands. But it was not known to be described until identified by Prof. E. Perceval Wright as the *R. typicus* of Smith.

A Statement in Reply to two Objections of Prof. Huxley relative to certain Experiments.—Dr. Bastian.

Department of Anatomy and Physiology

On Some of the more Important Facts of Succession in Relation to any Theory of Continuity.—Dr. Cobbold. This paper—which by permission had been transferred from the Department of Zoology and Botany—Dr. Cobbold stated, was generally of an elementary character. He said that for several years past the Biological Section of the Association had permitted, if it had not actually encouraged, the reading and discussion of papers having for their object the popularising (or it might be the unpopularising) of the theory of natural selection. To many besides himself the separate papers and remarks which followed were eminently unsatisfactory, perhaps arising not so much from any want of ability on the part of the authors as from the unscientific method adopted by them. The discussion which followed the reading of the Rev. F. O. Morris's paper at Norwich, "On the Difficulties of Darwinism," was lamentable in the extreme; for, so far as could be gathered, not one of those who sympathised with the reverend gentleman's position had the courage to advance a single fact in favour of the view which his "difficulties" were intended to support. At Exeter Mr. Morris renewed his exposition, but a much more vigorous effort in the same direction was made by the Rev. A. Freeman. The severity of the criticism which followed these final literary efforts in aid of anti-evolutionism could only be understood by those who were present; but the general conclusion of scientific authorities was significantly expressed in the statement made more than once to the effect that "neither of the papers ought to have been read." For his part, he thought the utmost freedom should be allowed to all desirous of opposing this or that theory; yet it should be thoroughly well understood that the sectional committee deprecated the employment of quotations from the Scriptures calculated to excite religious prejudice. A purely scientific question could only be satisfactorily discussed on a scientific basis—unless, indeed, it was insisted that theological speculations were inseparable from the domain of science. He then went on to say that to the mass of so-called educated men the acceptance of the views set forth by Mr. Darwin in his work "On the Origin of Species" must naturally present a variety of difficulties, and it appeared to him (Dr. Cobbold) that the best and truest way of showing an intelligent sympathy with those who were so situated, was to select and present some natural group of observed facts in such a clear and, if possible, attractive light, that common sense alone might be trusted to recognise the reasonableness or otherwise of honestly asserted deductions. The facts he had selected for exposition were such as represented what might be termed the apparent chronology of the organic series, or, in other words, the ascertained times of the coming and flourishing of the larger animal groups. A true conception of what was or ought to be understood by the expression "equivalences"—botanical, zoological, or geological—lay at the very basis of a correct appreciation of the significance of the records of animal, vegetable, or sedimentary rock distribution throughout all time. Further, he ventured to assert that the grandeur of the formative scheme of nature, whether testifying to an evolutionary method of production or to a series of creative acts, few or many in number, could only be adequately realised by the naturalist whose powers of allocation and grouping enabled him to grasp the magnitude and infinite import of those relations. Dr. Cobbold said he had insisted upon equivalency for years past. He then proceeded to deal with the facts of succession, and said the earliest organism as regarded time which geology had revealed was the fossil called Eozoon, which belonged to the lowermost division of the animal series. Dr. Cobbold then described the succession of the various known groups, and, glancing at the times of origin and succession of the placental mammals, said the first thing that the record suggested was the rapidity with which the most divergent groups made their appearance. Of course, there was no real basis for an assumption of a coeval creation, so to speak. It might be fairly held, on zoological grounds, that we ought not to separate man and monkeys, but retain them as one of the twelve groups under the ordinary title of primates. He adopted the division of the placental into twelve groups, not from any rigid belief as to their separate equivalences, but because they were not only sufficiently distinctive for all practical purposes, and also formed on the whole perhaps the finest expression of grouping which science could at present afford. After dwelling at great length upon the succession of the various groups, he stated that as regarded the highest of all, the placental series, he would only say that, as he understood the doctrine, the strictest demand of the development theory did not require, as was too commonly supposed, a lineal descent as between bimana and quadrumana; but it was certainly held that either of these groups, as we now knew them, might have been separately evolved from more generalised primatal types, the intermediary terms being possibly connected by a long antecedent and far more generalised common progenitor. In that connection the most advanced evolutionist must candidly own that the assumedly missing tertiary primatals constituted a great and very natural bar to the complete and popular acceptance of the theory of descent by natural selection. On the other hand, the scientific naturalist, whilst admitting these serious deficiencies, threw into the opposite scale a multitude of considerations, the collective value of which seemed to him to outweigh all the data thrown into the anti-continuity side of the balance. For himself, in conclusion, he said that his necessarily limited application of those data was amply sufficient to enforce upon him the provisional acceptance of any theory of continuity. To his mind, its clear application irresistibly implied that nature, to use an old phrase, was but a series of harmonies—wheel within wheel, there being probably but one wheel differing only from all the wheels of whose limits it was not possible for them to conceive. However, in the contemplation of the phenomena presented to them within that wheel—or that realm of "orderly mystery," as the president had called it—there was ample room and verge for the display of the highest physiological attributes with which man was endowed.

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* This paper will be found in full at p 30

Department of Ethnology and Anthropology

The report *On the Heat Generated in the Blood in the Process of Arterialisation*, by Dr. Arthur Gamgee, was taken as read.

New Physiological Researches on the Effects of Carbonic Acid.—Dr. B. W. Richardson. The author explained that the observations he had made were new in that they related to the direct action of carbonic acid on animal and vegetable fluids, and they were interesting, equally to the zoologist and botanist as to the anatomist. The author first demonstrated the result of subjecting a vegetable alkaline infusion to the action of carbonic acid under pressure. The result was a thick fluid substance which resembled the fluid which exudes as gums from some trees. When the fluid was gently dried it became a semi-solid substance, which yielded elastic fibres. This observation had led the author to study the effect of carbonic acid on albumen, serum of blood, blood itself, bronchial secretion, and other organic fluids. When the serum of blood was thus treated with carbonic acid under pressure and general warmth, 96° F., the collodial part was separated; but when the blood, with the fibre removed from it, was treated, there was no direct separation, the blood corpuscles seeming for a time to engage the gas by condensation of it. But blood containing fibrine, and held fluid by tribasic phosphate of soda, was at once coagulated by the acid. The bronchial secretion is thickened by carbonic acid, and a tenacious fluid is obtained, resembling the secretion which occurs in asthma and bronchitis, while secretions on serous surfaces are thickened and rendered adhesive. After details of many other facts, Dr. Richardson concluded by showing what bearing this subject had of a practical kind. In the first place, the research had relation to the question of elasticity of organic substances; and, secondly, on the direct action of carbonic acid in the production of vegetable juices. But the greatest interest concentrated on the relation of the research to some of the diseases of the animal body. Thus in instances where the temperature of the body is raised and the production of carbonic acid is excessive, the blood on the right side of the heart has its fibrine often precipitated, and in many other cases fibrinous or albuminous exuded fluids are solidified, as is the case in croup. The author, in the course of his paper, explained how rapidly blood charged with carbonic acid absorbed oxygen when exposed to that gas, and he held that carbonic acid in the venous blood was as essential to the process of respiration as was the oxygen in the pulmonary organs.

SCIENTIFIC SERIALS

Journal of the Chemical Society, October, 1870.—The first paper in this number is by Dr. Divers, "On the Precipitation of Solutions of Ammonium Carbonate, Sodium Carbonate, and Ammonium Carbamate by Calcium Chloride." These results obtained by Dr. Divers are the following:—Calcic carbamate is soluble, and the presence of ammonia retards its transformation into carbonate. When carbonic anhydride is passed into an ammoniacal solution of calcic chloride, the carbamate is first formed, and is gradually precipitated as carbamate. This paper is followed by nearly two pages of Addenda et Corrigenda to the author's previous memoir.—"On the Manipulation of Gold and Silver Bullion," by Charles Tookey, Assayer in the Japanese Imperial Mint, formerly in the Royal Mint, Hong Kong. In this paper the author gives descriptions of two of the processes that he has adopted. Instead of boiling the cornets in separate parting flasks, he uses a series of perforated platinum tubes, supported in a porcelain plate. A number of cornets are, by this means, simultaneously submitted to the action of the nitric acid. Secondly, in order to clean the buttons, they are placed with the lower side uppermost on a platinum plate with depressed perforated cavities, which is plunged into hot dilute hydrochloric acid, afterwards into hot water acidulated with hydrochloric acid, and lastly into pure water. The plate is then drained by placing it on porous paper and dried over a gas flame.—"On some new Bromine Derivatives of Coumarin," by W. H. Perkin, F.R.S. On adding coumarin to bromine in the presence of carbonic disulphide, allowing the solution to evaporate, and crystallising the residue from alcohol, dibromide of coumarin $C_9H_6O_2Br_2$ is obtained. When coumarin and bromine in carbonic disulphide are digested at 140°, bromo-coumarine $C_9H_5BrO_2$ and dibromo-coumarin $C_9H_4BrO_2$ are produced, and are separated by crystallisation from alcohol, in which the latter is the less soluble. Dibromo-coumarin fuses at 174°, and distils nearly unchanged. It crystallises from alcohol in small needles. Bromo-coumarin fuses at 110°, and crystallises from alcohol in transparent prisms, often beautifully curved. When heated with solution of potassic hydrate both the bromo-compounds dissolve, producing crystalline salts, probably containing the bromo-coumaric acids.—"On Organic Matter in Water," by Mr. C. Heisch. The author has observed that certain waters which are known to be contaminated with sewage matters, give rise to the formation of a microscopical fungus when a small quantity of sugar has been added, and the mixture exposed to light for a few days at the temperature of 60°-70° F. Six drops of sewage from which the solid matter had settled, were mixed with 10,000 grains of West Middlesex and New River water; to 6 oz. of the mixture 10 grains of pure sugar were added, and 10 grains were also added to 6 oz of the water without sewage; these solutions, and some of the mixture of water and sewage, were placed at a window. The water containing the sewage and sugar became turbid in 24 hours, the other liquids remaining clear. On examining the turbid water with an $\frac{1}{8}$ inch object glass, it was found to be filled with small spherical cells, with, in most cases, a very bright nucleus, which group themselves in bunches like grapes; they then spread into strings, with walls surrounding and connecting the cells; the original cell walls afterwards break, leaving tubular threads branched together. After several days, an odour of butyric acid is perceived. One drop of fresh urine in 10,000 grains of water produced similar effects; though without the addition of the sugar, the water might be kept for weeks without becoming turbid. Filtration through Swedish paper, or boiling for half an hour, does not prevent the growth of the fungus. The water no longer exhibits this property, however, after passage through a good bed of animal charcoal, that is, if the charcoal is frequently exposed to the air. If the filtration is continuous, the filtrate soon becomes as bad as the original water.—"On the Methods for the Determination of Carbon in Steel," by Mr. W. D. Herman. The author has obtained very concordant results by burning the iron or steel in a current of oxygen, the iron is converted into ferric oxide and the carbonic anhydride collected in potash bulbs and weighed. Some results obtained by four different methods of estimating the carbon in iron and steel are given at the conclusion of the paper.—"On the Determination of Phosphoric Acid," by Mr. W. C. Williams. The author suggests a modification of the process for separating phosphoric acid from the alkaline earths originally proposed by Reigsig.

SOCIETIES AND ACADEMIES

BRISTOL

Observing Astronomical Society.—Report of observations made during the period from Aug. 7 to Oct. 6, 1870, inclusive.

Solar Phenomena.—Mr. Thomas G. E. Elger writes:—The magnificent display of solar spots observed in August was repeated, though in a rather less striking manner, during September. Between the 7th and 12th the spots were small, few in number, and mainly confined to the S. hemisphere; on the 11th only three moderately-sized groups were visible. The immense group observed last month, and which was near the centre of the disc on August 30, was due at the E. limb about the 17th, but owing to unfavourable weather and absence from home, I did not notice it till the 24th, when it measured 2′ 45″ × 1′ 50″ without including the outlying penumbra which followed it; its length on the 25th was 3′ 0″. The penumbra of this group presented some remarkable features It contained four large umbræ and many smaller ones; on the preceding side it was thickly studded with minute dots of every shade from black to light brown. When examined with a power of 180 at 3^h on the 25th, the entire group was evidently undergoing rapid and violent changes, the striation of the penumbra and the dark "spurs" and serrated edges of the umbra clearly indicating the cyclonic nature of the forces involved. The above group was preceded by a very long and narrow V-shaped spot, which occupied nearly the same position as a large spot observed in August. Several other groups were observed during the month, which presented interesting details, but they were generally smaller than the August groups. The appearances exhibited by the large group described above, and indeed by most spots of a similar class observed this year, seem altogether opposed to the "deep excavation" theory of sun spots. —Mr. T. W. Backhouse, of Sunderland, reports "a very fine group of spots passed the sun's centre in the northern zone on September 21; on Sept. 23, at $21^h\ 15^m$, it contained two very long penumbræ, which were not widely separated; the *f* one was 74,000 miles long, and the *p* one 92,000! On the 25th, at $19^h\ 45^m$, it was only 66,000 miles long, and the *f* penumbra was divided into two. Another very fine group, also in the N. zone, passed the sun's centre on the 24th. The dimensions in miles of its chief spot were as follows:—

Date.		Time.	Penumbra.		Umbra.	
			Length	Width.	Length	Width.
Sept.	21	$5^h\ 15^m$	——	47,000	26,000	11,000
,,		$21^h\ 25^m$	abt. 72,000	50,000	31,500	——
	22	$3^h\ 25^m$	50,000	——	29,500	——
	23	$21^h\ 25^m$	63,000	——	——	——
	25	$21^h\ 30^m$	abt. 70,000	——	——	——

On the 23rd, at $4^h\ 40^m$, I found it was divided into four, apparently by a violent current in the middle from *p* to *f*".—Mr. William T. Dunning, of Bristol, observed the large spot visible on September 21; with his 4-inch metallic reflector he could very distinctly see a black nucleus in the S part of the umbra. It did not appear to be actually enclosed within the umbra, but was situated on the margin of the penumbra.—Mr. E. B. Knobel, of Burton-on-Trent, says that on Sept. 25 the large group near the centre of the disc measured 2′ 54″ by 1′ 44″; on the 26th the two largest groups were equal in length to 2′ 44″ and 2′ 36″ respectively. They were distinctly visible to the naked eye. —The Rev. S. J. Johnson, of Crediton, writes that on September 21, at $4^h\ 30^m$, a power of 50 on a 2¼-inch *O.G.* showed penumbræ on, at least, 26 spots visible on the sun. On September 20 "seven spots were very large indeed, and arranged in five groups, each scattered over a large surface."—Mr. Albert P. Holden, of London, referring to the large spot, says that on September 20, "when entering on the solar disc it appeared as an elongated spot with a bright arm stretching over half the umbra till it joined a projection on the N. side. It was followed by a large broken group of broken masses of various dimensions. On the 23rd, at 8 A.M., the chief spot had enlarged considerably, while the broken ones following it had very much decreased. The great spot was very nearly divided into two by a very broad arm springing from the N.; the W. portion of the umbra being again subdivided by a similar arm on the S. side. This last was on one side broken up very peculiarly, so that it presented the appearance as if a handful of bright straw had been thrown carelessly upon it. The eastern portion of the umbra was crossed by a very bright curved streak, which was so bright and so clearly distinct right up to its edges as to appear more like a carved piece of silver. On the 24th, at 8 A.M., the broken mass before referred to as following the chief spot had almost disappeared, with the exception of one small spot and a small amount of penumbra. The great spot was also a little smaller and quite divided by the broad arm. A large crack appeared in this latter. Each of the two portions of the original spot were also divided by luminous bridges across them. On the 28th the appearance of the umbra was much the same, although the penumbra was entirely changed and a great narrow branch had projected S. to an immense distance. This great arm was dotted here and there with a few patches of umbra. The broken mass which had followed the great spot at its first appearance was now entirely dissipated. On the 29th the two portions of the original spot were widely separated and much contracted, and two spots to the S. which had hitherto been of very small size much increased in dimensions. The rotation of the solar orb then carried the spot out of sight. The principal fact impressed upon the mind by these observations is that a spot becomes dissipated in consequence of its continual division and subdivision by the projection of luminous bridges across its various portions.

DIARY

THURSDAY, NOVEMBER 10.

LONDON INSTITUTION, at 7 30.—Acoustics of the Orchestra: Dr. W H. Stone

LONDON MATHEMATICAL SOCIETY, at 8—Annual General Meeting Recent Researches on Quartic and Quintic Surfaces: By Prof Cayley.—The Retiring President's Address.

FRIDAY, NOVEMBER 11.

ASTRONOMICAL SOCIETY, at 8

MONDAY, NOVEMBER 14.

LONDON INSTITUTION, at 4—Chemical Action. Prof. Odling

TUESDAY, NOVEMBER 15

ANTHROPOLOGICAL SOCIETY, at 8—Observations on the Condition of the Blood-Corpuscles in Certain Races. Dr R. H Bakewell—Tribal Affinities among the Aborigines of Australia: Mr. C Staniland Wake—Description of Australian Aborigines and Half-Castes, with Exhibition of Skulls: Dr. Robert Peel.

ZOOLOGICAL SOCIETY, at 9—On the Form and Structure of the Manatee (*Manatus Americanus*): Dr J. Murie.—Observations on the Salmonidæ in Tasmania: Mr. Morton Allport—On the Anatomy of *Ailurus fulgens*: Prof. Flower.

STATISTICAL SOCIETY, at 7 45.—On the Claims of Science to Public Recognition and Support, with Special Reference to the so-called "Social Sciences": Dr. Guy, F R.S

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

THURSDAY, NOVEMBER 17.

LINNEAN SOCIETY, at 8—On the *Passifloreæ*: Dr. M. T. Masters—On the White-beaked Bottle-nose: Dr. James Murie.

CHEMICAL SOCIETY, at 8.—Mineralogical Notices: Prof. N. Story Maskelyne and Dr. Walter Flight.

LONDON INSTITUTION, at 8 30.—Acoustics of the Orchestra, Wind Instruments: Dr. W H. Stone.

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THURSDAY, NOVEMBER 17, 1870

THE PRESENT EPIDEMIC OF SCARLET FEVER

THE prevalence of scarlet fever at this time throughout England and Wales is a question that not only affects medical men, but is one that demands the attention of the people of England. They ought to ask themselves the question as to how it is that a malady which is placed by medical writers under the class of "preventible" diseases is allowed to prevail to the extent of destroying several hundreds of persons weekly. Scarlet fever seems to enjoy that immunity which is accorded to what are called "necessary evils;" but when we come to inquire what right this disease has to carry off ten or twelve thousand persons annually, we find that it ought no more to exist amongst us than small-pox or cholera.

Scarlet fever is essentially a contagious disease, and exhibits all the phenomena of a malady which, being communicated from one individual to another, is more or less under the control of human action. Under these circumstances, it is impossible for the Government to stop such a disease by mere Acts of Parliament, or for medical men to superintend efficiently arrangements for the prevention of its spread. It is only by the intelligent apprehension on the part of the public who are infected, that any hope of the arrest of the disease can be expected. We therefore take this opportunity of addressing the public on the subject. Unless heads of families and the public generally are acquainted with the real nature of this disease, no external organisation of any kind is sufficient for its control.

We need not refer here specifically to the returns of the Registrar-General, to show how fearfully prevalent scarlet fever has been. In London the weekly mortality has been as high as one hundred and ninety in a week, giving a mortality for London alone of nearly ten thousand a year. Professor Huxley, in his late address at Liverpool as President of the British Association, says that in the years 1863, 1864, and 1869, 90,000 persons were killed in England and Wales by scarlet fever. These figures point to a much higher mortality for scarlet fever than we have ever had to record for cholera. The point most remarkable about this mortality is, that whilst death from cholera has agitated every British community, no public anxiety has been manifested about scarlet fever. Every one has submitted to it as a necessary evil, and no one has made any efforts to diminish its prevalence.

Yet, when we come to inquire into the nature of scarlet fever, and the laws of its distribution, there seems to be no more reason why it should prevail amongst us than plague, small-pox, or cholera, whose laws of distribution we now know, and on which we can exert the most obvious control. Scarlet fever is a contagious disease, and it is not too much to say that we have *all contagious diseases under our positive control.* Their nature, and the laws of their distribution, are so well known, that it is possible to teach the humblest individual interested in their destruction the means by which it may be effected.

We need not here enter into the discussion of the nature of "poison germs," of "microzymes," or other ultimate forms which the poisons of contagious diseases may assume, but we may affirm that in every body affected with scarlet fever there is produced poisonous matter, which, passing from the diseased body, is capable of generating anew the same disease as that which affects the body from which it is derived. The proofs of this are so abundant that we cannot for a moment admit that the question is open to discussion. The point of most importance here is to know how long the "poison germs" of scarlet fever retain their vitality—the terrible power of starting anew the changes of which they are the offspring. With regard to scarlet fever, we have more evidence of these "poison germs" retaining their vitality than with many other contagious diseases. Sir Thomas Watson, in his classical lectures on the "Practice of Physic," mentions a case in which a piece of flannel worn round the neck of a scarlet fever patient, being accidentally discovered two years after, and applied to the person of a servant in the family, produced an attack of scarlet fever. Were it necessary, I could mention several instances coming within my knowledge and reading, of the scarlet fever poison lying dormant in woollen clothes for years, and not having lost its vitality, or power of communicating the disease.

Another point of importance with regard to the scarlet fever "poison germs," is the length of time which a person once affected with scarlet fever is capable of communicating the disease to others. When a person has got well of scarlet fever as far as general health goes, it is by no means the case that he is no longer capable of communicating the disease, but many days after he is strong and apparently healthy, he is capable of disseminating "poison germs" from his body. A recent instance has been recorded in one of our medical journals, of the prevalence of scarlet fever in families supplied from the milk of a particular dairy. On searching inquiries being made, it was found that the persons connected with the farm from which the milk was supplied, had been affected with scarlet fever. Although they had not been allowed to milk the cows till they had recovered from the scarlet fever, it was, nevertheless, found that they had been engaged in this occupation whilst the effects of scarlet fever in the desquamation of the cuticle of the skin was still going on. There are abundant other examples on record to show that until the desquamation of the cuticle which always follows scarlet fever is complete, no person who has had scarlet fever is safe from giving it to others.

Such then being the nature of the poison of this disease, what ought to be known in families where it breaks out, and what to be done as the result of this knowledge to prevent its spread? It is no use saying that the doctors will give all necessary directions. In the first place, it may be said that the doctor in nine cases out of ten will not give any directions at all. It is not his interest to do so; and if it were, he gets no information in his books or lectures on the subject at all. The medical profession is not required by its governing or examining bodies to know anything about public health or preventive medicine. In the next place, however admirable may be the directions of medical men, persons utterly ignorant of the nature of disease will fail to carry out the simplest directions given to them. Nothing can be a substitute for a knowledge of the first principles involved in the destruction of "poison-germs" in a family attacked with contagious disease.

What, then, ought to be done in a family when scarlet fever, or any other contagious disease, has broken out? In the first place, the entire isolation of the persons attacked should be secured. They should either be removed to a room in the house to which none but the nurse and doctor have access, or the family should be removed to some house of refuge or place where the disease does not exist. It may be urged that this cannot be done; but if it be a mere question of expense, it should be considered whether the cost of the deaths, the funerals, and the doctor's bills of a family of several children, and perhaps the father or mother, may not really, in a money point of view, be greater than any cost of isolation.

But whether isolation is attempted or not, there is another set of facts which must be borne in mind. The "poison germs" of which we have spoken can really be destroyed. If left alone they can lead a life of poisonous activity. We have the means of killing them—poisoning them in their birth, as it were—by certain substances whose properties we well know. We cannot here write a history of disinfectants, but they are well known, and the advertising sheets of every newspaper will afford information with respect to them. The most common and available are carbolic acid, permanganates of soda and potash (Condy's Fluid), chlorinated lime or soda, chloride of zinc (Burnett's Fluid), chloride of aluminium (chloralum), sulphate of iron, and others. These agents have the power of destroying the poisonous activity of scarlet fever germs. In the sick-room and around the patient they should be constantly employed. All the secretions of the person affected, whether they come from the nose, the mouth, or other excretory organs, should be immediately brought in contact with one of these agents. All linen and clothes worn by the patients should be placed in a solution of one of them. Nurses attending on the sick, and medical men touching them in any way, should not leave the room without washing their hands in one of these disinfecting fluids.

Woollen clothing that cannot well be washed should, by some agency or another, be exposed to heat. It is well known that a temperature of 212° F., the temperature of boiling water, will destroy poison germs. Woollen clothing of all kinds, such as shawls and mantles, men's clothes, as also curtains, bell-pulls, carpets, rugs, and beds, should be placed in ovens, or by some contrivance or another exposed to a heat above 212°. In St. James's, Westminster, a disinfecting apparatus has been erected in the Workhouse-yard, where the various articles mentioned can be disinfected.

Having thus indicated general principles, I must leave details. I am convinced that the holocaust of victims that we annually offer to this Moloch of scarlet fever arises from ignorance, and that a general knowledge alone of the facts above stated can suffice to drive from us this plague, so disgraceful alike to our intelligence and our humanity.

E. LANKESTER

SCHIMPER'S VEGETABLE PALÆONTOLOGY

Traité de Paléontologie Végétale. Par W. Ph. Schimper. Tome ii. Première Partie, pp. 522. (Paris, 1870.)

THE first part of the second volume of this very important work (the first volume of which was reviewed in the first number of NATURE) has just appeared, with a quarto atlas of twenty-five plates; and it maintains the high character with which it commenced. In this part the systematic description of the families, genera, and species, is carried down from *Lycopodiaceæ* to the end of Monocotyledons; and the same excellent plan is carried out, of giving under each principal group its most important botanical characters and geographical distribution, drawn from living types; thus supplementing the excessively meagre descriptions that the fossils afford, enabling the reader rightly to appreciate the strength or weakness of the evidence on which the alliances of the fossils are founded, and indicating the organs or structural points most desiderated, and to obtain which collectors should search for illustrative specimens.

The following extracts and remarks will give some insight into the general nature of this volume, and illustrate the author's views as to the relations between many of the most important existing types of vegetation and their fossil representatives or allies.

Dr. Schimper ranks the great recent group of club-mosses and their allies, *Lycopodineæ*, as a class, with four subordinate families; of which two, *Lycopodieæ* and *Isoeteæ*, are abundant at the present time; the other two, *Lepidodendreæ* and *Sigillarieæ*, disappeared before the Tertiary epoch.

Under the order *Lycopodiaceæ* he includes two families: of these (1), *Lycopodieæ*, which abounds at the present day, and inhabits all latitudes from the equator to the Arctic regions, Dr. Schimper recognises only seven fossil species, all congeneric with *Lycopodium* itself, and, strange to say, confined (with the exception of one doubtful species) to the coal measures—a most remarkable fact, if capable of confirmation; but, after making every allowance for the imperfection of the geological record, it appears impossible to admit that a group so well represented now-a-days should be absent from all intervening beds, including the most modern tertiaries; and it is a startling proof either of the vagueness of the characters by which we ally our fossils to existing types, or of the imperfection of the said record. Upon the whole, and under such suspicious circumstances, we should be rather disposed to doubt the fossils being Lycopodiums at all.

Dr. Dawson's four species of this order, from the Devonian of North America, Dr. Schimper considers not to be recognisable as such; so that, little as his own data can tell us of the fossil *Lycopodineæ*, there is a lower depth of obscurity still.

Of the second family, *Lepidodendreæ*, consisting mainly of arborescent plants, all are extinct: it includes the principal genus *Lepidodendron*, with fifty-six species, all carboniferous, and with a host of synonyms, generic and specific:—*Halonia*, *Lepidoploios*, *Knorria*, and *Ulodendron*, all well known to English palæontologists. *Lepidophylla* and *Lepidostrobi* he regards as respectively leaves and fruits of some one or other of the above.

To the third family, *Isoeteæ*, which includes some fifty or sixty quill-worts and pill-worts of the temperate regions of the globe, he attributes three fossil genera; *Isoetes* itself (tertiary), and the remarkable *Psilophyton* of Dr. Dawson, which he regards as allied to *Pilularia* and not to *Psilotum*. But here again we are confronted by the suspicious fact, that nothing resembling *Psilophyton* has hitherto occurred between the Devonian and the present epoch!

The fourth family, *Sigillarieæ*, is only known in a fossil state. *Sigillaria* is the principal genus, with eighty-three species. *Stigmaria* Schimper regards as roots, but not of *Sigillaria* only, for in a supplementary note to this genus he announces the discovery of a specimen of *Knorria longifolia* (one of the *Lepidodendreæ*), the roots of which are a *Stigmaria*, the base of the stem is his own genus *Ancistrophyllum*, and the upper part is *Didymophyllum Schottini* of Goppert; thus knocking four genera into one at a blow. Add to this the fact that the leaves and organs of fructification, now unknown or unrecognised, may represent two more genera, and that there is a suspicious look of *Ulodendron* in one point of the structure, and we have as instructive an example of the condition of our knowledge of the carboniferous flora as can be desired.

Passing from Acotyledones to Cotyledones, or flowering plants, Dr. Schimper's first class is of *Cycadeæ*. They date doubtfully from the middle of the carboniferous, allowing *Noeggerathia*, &c. to belong to this alliance; in the Permian they are pretty certainly present, and they abound in the Trias and Jura formations. Of the two tribes composing this group, the *Zamieæ*, which now extend from the Old to the New World, seem to have appeared first; both these and the *Cycadeæ* proper (which are now confined to the Old World) are found in greatest abundance in the Jurassic age, whence they decline and disappear in the cretaceous, except *Zamiæ*, of which one species persists to the middle of the Tertiary epoch.

Here again, if we hold that the geological record tells a fragment of truth, we must believe that the Cycads and Zamias, which occupy so very wide an area of the globe in the present day, and whose organs and tissues are so well suited for petrifaction, had all but disappeared from the globe during the lapse of countless ages, to reappear in numbers, and that over a most extensive area.

No fewer than thirteen genera of fossil *Zamieæ* are described, and about twenty of *Cycadeæ*, including a multitude of species; both genera and species are however very badly, if at all, defined, being most fragmentary; and Dr. Schimper was of course ignorant of Mr. Carruthers' paper on the Fossil *Cycadeæ* of the secondary rocks of Great Britain, in the twenty-sixth volume of the Linnean Transactions, which throws great light on the subject. The genus *Trigonocarpus* Dr. Schimper doubtfully regards as consisting of fruits of *Cycadeæ* (describing fifteen species), together with *Rhabdocarpus* (twenty-four species), *Cardiocarpus* (twenty-one), and *Carpolithes* (nine)

Ascending in the scale of vegetable life, we come to the great group of Conifers, which seem to have presented such remarkable facilities for petrifaction in all ages from the carboniferous onwards, and which is divided into four great sections. 1. *Abietaceæ*, with four families, *Walchieæ* and *Voltzieæ*, of very doubtful affinity, if Conifers at all, and of which the one is usually placed in *Lycopodiaceæ*, and the other in *Cupressineæ*—neither have any recent representative; *Araucariæ*, comprising four extinct genera, together with *Araucaria* itself; and *Abietieæ* proper, including *Pinus* with 101 species. All appear tertiary; many are miocene, extending from Spitzbergen southwards; and, judging from the number found in single localities, as at Armissan, it would appear that either the pine-forests of those days were, unlike the present, formed of numerous species, or, what is more probable, that the supposed specific characters are worthless. Of larch, four miocene species are enumerated; of cedars, three cretaceous; of Abies, twenty-two species, all tertiary.

2. *Taxodiaceæ* form Dr. Schimper's second order of Conifers; it includes *Sequoia* with twelve species, one of which, the well-known miocene *S. Langsdorffii*, he regards, with Heer, as "almost identical with the Californian *Taxodium sempervirens*, now confined to Mexico." In its fossil state it extends from Arctic East and West America and Greenland to Bonn, Italy, and Greece. Another existing *Taxodium*, again, contains a miocene species, absolutely identical with the living *T. distichum* of the Southern States of North America, where it forms a large proportion of the arboreous vegetation of the Great Dismal Swamp. In a fossil state this also extends from Greenland and Spitzbergen to North Italy. Two other existing genera of this group, the North Chinese *Glyptostrobus*, and the South African *Widdringtonia*, are both supposed to be found in the European miocene formations.

3. Of *Cupressineæ*, to which the modern cypress, arborvitæ, juniper, and the extensive genus *Callitris* of Australia belong, there are fossil types supposed to belong to recent genera; viz. of *Libocedrus*, now confined to New Zealand and South Chili, there are two Spitzbergen and a European species, all miocene; of *Thuja*, five, all found in amber beds. To the American and Japanese White Cedars (*Chamæcyparis*) two European fossils are referred. The four junipers are a very doubtful lot.

Taxaceæ, represented throughout the northern hemisphere by one living yew, in Japan by the Maiden-hair tree (*Salisburia*), and in the tropics and south temperate regions of both the old and new worlds by various species of *Podocarpus*, have been supposed to be discovered in small numbers in a fossil state. Thus, of *Salisburia*, the existing Japanese species is believed to be found in the miocene of Greenland and near Verona, and two other American species are described—all miocene. The fossil yews are all doubtful, and the *Podocarpi*—eight in all—are probably equally so.

Gnetaceæ, to which the curious existing genus *Gnetum* (tropical), *Ephedra* (temperate), and *Welwitschia* (tropical) belong, are represented by two modern species of *Ephedra*.

The volume closes with 130 pages of Monocotyledonous fossils—a very miscellaneous and for the most part exceedingly obscure assemblage. The existence of this class in the carboniferous epoch is regarded by Dr. Schimper as not proven, and more than doubtful. In the Trias it is pretty well established by *Yuccites* and *Æthophyllum*, but there is a hiatus between that age and the Jurassic, when an obscure set of water-plants and perhaps *Pandanus* occur, to be followed by another hiatus till the cretaceous epoch, when *Gramineæ*, *Cyperaceæ*, and several of the principal orders of monocotyledons appear in some force. Whether, however, we ascend or descend in the scale of vegetable life, as represented by fossil monocotyledons, we feel the generic and even the ordinal determinations of the fossils to be throughout very doubtful, and to be reducible in most cases to the category of bold guesses. To refer the majority of living and growing monocotyledons, and especially of such orders as *Gramineæ*, *Cyperaceæ*, *Naiadeæ*, *Palmæ*, and *Liliaceæ*, to their genera from their leaves alone, is impossible; much more then from the fragmentary fossil remains of these organs. Such genera and species, however, have been made, and Dr. Schimper must sweep them into his net; and the more ungrateful the task, the more obliged we should be to him or performing it so well and so fully.

We shall look for the Dicotyledonous volume of this great and most useful work with much interest, and lay an account of it before our readers as soon as it appears

J. D. HOOKER

THE YOSEMITE VALLEY AND THE SIERRA NEVADA OF CALIFORNIA

The Yosemite Guide-book. By J. D. Whitney. Published by authority of the Legislature of California. 1869.

MR. J. D. WHITNEY, State Geologist of California, has prepared a guide-book to the Yosemite Valley and the adjacent country, which is a model of its kind. It is well written, and is eminently lucid in its descriptions; it is amply illustrated, and has two clear maps on a large scale half a mile and two miles to the inch); and it is admirably printed (at the University Press, Cambridge, U.S.). The unpretending title of the work gives but a poor idea of its contents; botanists, geologists, and geographers will find pleasure in reading it, although it is ostensibly put forth or the sake of the ordinary tourist. It is a valuable contribution to our knowledge of the Pacific coasts of America, and throws light upon a large district which remains very imperfectly known.

The British public has been already tolerably familiarised with the most striking characteristics of the Yosemite[*] Valley, Mariposa County, California, by the fine picture of Mr. Bierstadt; and some excellent photographs (by Watkins, of San Francisco), which are now lying before us, show that that artist has not drawn upon his imagination. Some of our readers, however, may perhaps need to be told that it is not only a very remarkable valley, but boasts the possession of the highest waterfalls (with an equal volume of water) in the world. It is trough-shaped; a cross section of it is like the letter U, and in this respect, as well as in its length, breadth, and the steepness of its cliffs, it is comparable to the Valley of Lauterbrunnen in Switzerland, but the dimensions of its cliffs exceed those of that valley, as much as its chief waterfall (the great Yosemite Fall) surpasses the Staubbach. This magnificent cascade makes a descent of more than half a mile in sheer height, and 500,000 cubic feet of water pass over it per hour during the month of June. It is made up of two falls—an upper one, which has a vertical descent of 1,500 feet, and a lower one of 400 feet. The remainder of the total height is consumed by a series of cascades between the two; but seen from the opposite side of the valley, the effect is increased rather than diminished by the subdivision, and it well deserves all the praise which has been lavished upon it.

Mr. Whitney does not believe that the peculiar trough form of the valley has been even modified by glacier action. There are no proofs, he says, "that glaciers have ever occupied the valley, or any part of it," and he scouts the notion that it was *produced* by glacial agency. "In short," he says, "we are led irresistibly to the adoption of a theory of the origin of the Yosemite in a way which has hardly yet been recognised as one of those in which valleys may be formed, probably for the reason that there are so few cases in which such an event can be absolutely proved to have occurred. We conceive that during the process of upheaval of the Sierra, or possibly at some time after that had taken place, there was at the Yosemite a subsidence of a limited area, marked by lines of 'fault' or fissures crossing each other somewhat nearly at right angles. In other and more simple language, the bottom of the valley sank down to an unknown depth, owing to its support being withdrawn from underneath, during some of those convulsive movements which must have attended the upheaval of so extensive and elevated a chain, no matter how slow we may imagine the process to have been." It should be mentioned that the Yosemite Valley is exclusively granite, no remains of sedimentary rocks having been found within it.

Although the Yosemite Valley itself is not ice-ground, in its neighbourhood there are very emphatic traces of glacial action, and it is said that in some places the polish is so perfect upon the rocks, that "the surface is often seen from a distance to glitter with the light reflected from it, as from a mirror." It is remarkable that no glaciers are known to exist at the present time throughout the entire length of the Sierra Nevada in California. Throughout the entire region, from 35° to 42° N. lat., Mr. Whitney asserts that there is not a single glacier. Yet Mount Shasta, at the northern extreme, with a height of pretty well 15,000 feet, is more or less permanently covered with snow upon its upper 6,000 feet, and at the southern end of the district there are numerous peaks (now being surveyed) which range from 14,000 to 15,000 feet, and even higher. We confess that we do not understand how "masses of snow, several miles long, and hundreds of feet in thickness, remain all summer without showing any indication of becoming glacier ice," if they remain permanently of these dimensions; but as such seems to be the fact, there is no use in disputing it.

The most valuable portions of Mr. Whitney's book are those which he has devoted to elucidating the topography of the mountain ranges bordering upon, or in proximity to, the Pacific Coast. We learn from him in a clearer way than we have seen it put before, that there are two great ranges of mountains running throughout the length of California, which are orographically distinct. These he terms the Coast Ranges, and the Sierra Nevada. They are roughly parallel to each other, and to the coast line; and they are divided by the valleys of the Sacramento and San Joaquin rivers. The coast ranges are, geologically speaking, younger than the Sierra Nevada, and they are chiefly made up of cretaceous and tertiary strata. They are also of comparatively small elevation, and their highest summits attain only 8,000 feet. The core of the Sierra Nevada, on the other hand, is mainly granitic, flanked on both sides by metamorphic slates, and has a much greater elevation.

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[*] The word Yosemite means a full-grown grisly bear.

Both of these ranges are totally distinct from the Rocky Mountains, which are hundreds of miles further inland. The only drawback to travel among them is the "poor Indian," who watches travellers from a distance, and signals by smokes. We can well understand the enthusiasm of Mr. Whitney for the mountains of California, and we heartily hope with him, that neither the Yosemite valley nor the grove of Sequoias which—by a unique act of Congress—were ceded to the State for public use, resort, and recreation, for all time, will be suffered to fall into the hands of the acute but unpatriotic speculators who are endeavouring to use them for their own purposes, and who will, if they are not "sat upon," undoubtedly turn them into "gigantic institutions for fleecing the public." W.

OUR BOOK SHELF

The Science and Art of Arithmetic: Part II. Vulgar Fractions; Part III. Approximate Calculations. By A. Sonnenschein and H. A. Nesbitt, M.A. 260 pp. (London: Whittaker & Co. 1870.)

THE authors of this excellent school arithmetic are to be congratulated on having brought their work to a successful termination. In our notice of Part I., which appeared in a former number of NATURE,* we pointed out the principles by which the authors had been guided, and as we believed those principles to be sound, and the authors to have carried them out successfully, we had no hesitation in commending the work as we did. The same good arrangement, ample store of illustration, and copious examples for practice, are to be found in this volume as had place in the first. The fulness with which the elementary portions were treated appeared to us to be a merit rather than a defect. From such wealth of illustration each teacher could select what was most suited to his purpose. In this volume we have more advanced subjects treated in like manner. But an analysis of the contents will give a good idea of the work. Under Part II. we have the subject of Vulgar Fractions clearly treated, with applications to Practice, and a chapter which treats of Proportion, the Chain Rule, Compound Proportion, and Proportional Parts. In Part III. are chapters on Converging Fractions, Decimals with their properties, and several applications to Money, Weights, Measures, &c., the Metric System, Progressions, Interest, Discount, Stocks, Evolution, and a good chapter on Arithmetical Complements. There is also a chapter in which we have Continued Product to a given limit, Compound Interest, Equation of Payments, Complex Decimals, Duodecimals, and International Calculations. At the end of the work are given 250 Miscellaneous Exercises. There is enough here to satisfy any youthful arithmetician, and the methods employed are the "latest out." The complete work gives ample evidence that it is the composition of men who have given much time and thought to the subject, and have had much tuitional experience. R. T.

Die Schmetterlinge Deutschlands und der Schweiz, systematisch bearbeitet. Von H. von Heinemann. Zweite Abtheilung, Kleinschmetterlinge. Band II. Die Motten und Federmotten, Heft 1. 8vo. (Brunswick: C. A. Schwetschke and Son, 1870.)

M. HEINEMANN'S work on the Lepidoptera of Germany and Switzerland is well known to entomologists, by whom it is highly valued. It contains an admirable systematic description of the species of butterflies and moths inhabiting the above-mentioned countries, and has been carried out by the author in so conscientious a manner that the students of European Lepidoptera can hardly wish for a better handbook. The author is now approaching the conclusion of his labours. The first section, including the larger forms of Lepidoptera, was completed some years ago; of the second section, the first volume, published in 1863 and 1864, contained the descriptions of the Tortrices and Pyralides, and the part now before us commences the true Microlepidoptera, the Tineæ and Pterophori. The former are exceedingly numerous, and the present portion contains descriptions of the species of only five out of the thirteen families into which the author divides the group. W. S. D.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Teachings of Tribulation—"The Captain"

THE technical questions suggested by the loss of Her Majesty's ship *Captain* have received their full share of attention—indeed, more than their share, for able leaders and letters in the newspapers will not teach us how to build war ships. I do not intend to trouble you with freeboards, turrets, and metacentres. Nor do I propose to dive into all the published evidence and statements, in order to discover some delinquent on whom to saddle the blame of the terrible disaster.

The case of the *Captain* is an example, on a scale sufficiently startling to attract the notice of the whole nation, of a want in our administrative arrangements which has hitherto escaped the notice of the many. The few who have long deplored this want did not require that a quarter of a million of money and 500 gallant lives should be sacrificed in order to prove its existence. Employing, then, the loss of the *Captain* as merely one very striking instance out of thousands of others that pass unheeded every day, permit me to suggest what is the administrative lesson taught by it. The facts must first be briefly stated. This cannot be better done than by quoting a portion of the judgment of the court-martial, who tell us that they "find it their duty to record the conviction they entertain that the *Captain* was built in deference to public opinion as expressed in Parliament and through other channels, in opposition to the views and opinions of the Controller of the Navy and his department, and that the evidence all tends [*sic* in *Times*' report] that the Controller of the Navy and his department generally disapproved her construction."

Now let us analyse the system the working of which justified the above reiterated condemnation. Beginning at the top, we have a Minister of State, a gentleman usually of high character and great general attainments, but not necessarily conversant with naval architecture. Being a party politician, he may at any moment vacate his post on some question totally unconnected with his department. In order to provide him with the knowledge which he does not pretend to possess, and to supply the element of permanence in which he is also deficient, he has under him secretaries, superintendents of works, and scientific advisers. These persons are in the strictest sense of the terms also subordinates of the Minister, bound to obey his orders. Outside this department are two bodies, independent of the Minister, and capable of bringing enormous pressure to bear upon him, namely, inventors and the public—the first interested, the second ignorant of science and owing no responsibility to any one. I disclaim any personal allusions in this analysis. Well, here we have two forces pulling the Minister different ways. The whole question is, whether the departmental, or, as we may call it, the home, force is strong enough to enable the Minister to resist, when necessary, the foreign invader; whether reiterated adjurations, noisy clamour, and threatened loss of popularity will silence and overbear official counsels. The question is already answered. The official counsels were overborne. It may be argued, however, that the official counsels may be wrong and the public right. But the official counsels in the case before us were right and the public wrong. Here we are landed in a difficulty. How is the Minister, who by our hypothesis has no knowledge of the question at issue, to tell when his advisers are right and when the public? He cannot tell. But he exercises his discretion. Now, it must be remembered that his advisers are his subordinates, and therefore, taking men as we find them on an average, not in a position calculated to prompt them to oppose very strenuously a policy on which they see that their superior is set. Moreover, the advisers, who in the case before us are but two or three individuals, may be, as it was believed they were, prejudiced against the contrivance under consideration. It would be natural that the Minister should make a considerable deduction from the weight of their remonstrances on account of the departmental jealousy by which he might imagine them to be more or less tinctured. Thus he is at sea, as deficient in the elements of stability as the *Captain* herself.

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* See NATURE, Vol. II., p. 186.

An obvious remedy for such a state of things might be to appoint as permanent heads to our great technical departments men thoroughly acquainted with their duties who could act on their own independent judgment. But this would subvert that perfect and inviolable edifice, the British Constitution Far be it from a humble unit like myself to attempt such sacrilege!

What remains, then, as we cannot repress inventors and silence public clamour if we would, than to give the Minister stronger and more independent scientific support than that which was found in the case of the *Captain* too weak to prevent the most humiliating and disastrous blunder of modern times?

The suggestion I now venture to make is not new, nor do I make it now, on the pinch of the moment, for the first time. I brought it more than a year ago before a committee of the British Association, of which I was chairman My proposal was, and is, that a powerful body of the most eminent men in every branch of science should be constituted a permanent paid Council for consultative, as distinguished from executive, purposes Space does not admit of my detailing the constitution, mode of electing, and functions of this body. But, having long had the matter in my mind, I may say that I see no difficulty in securing the main conditions of varied and profound acquirements, and of due official relation to, yet thorough independence of, the Ministry and politics of the day. I need hardly say that such a consultative Council should comprise not only men distinguished in abstract science, but also men representing all branches of the sea and land forces, all technical departments, the public works, and the principal arts and manufactures of the country.

No mistake can be greater than to consider this proposal revolutionary, as some at first sight have done. It is in fact only a consolidation and systematisation of agencies actually in existence. The principle of supplying the country gentlemen who become Ministers of State with scientific advice through permanent secretaries and other subordinates, and through temporary committees entrusted with specific inquiries, has long been in force. It is certain that these individuals and bodies are often selected capriciously, and it is not saying too much to assert that the results of their labours would have been more valuable if their functions had been less narrow and their existence less precarious. The great domain of physical science cannot be parcelled out in neat little squares like a chess-board; its varied districts, as Nature has planned them, run into and mix with each other so intimately that in order to trace the boundaries of one, some knowledge at least of the adjoining tracts is necessary. Special committees, however well chosen, are seldom even numerically strong enough to comply with these conditions.

The Council now advocated purposes to substitute for innumerable, scattered, temporary, incomplete, hand-to-mouth expedients a permanent, properly selected organisation. In one case the work is done somehow—we see to our cost how, in the other it will be done as well as human intelligence can do it; but in both cases the very same work will be done—namely, that of bearing really the burden of responsibility which Ministers only bear nominally. The principle will be the same under the existing and the proposed régime, but whereas it is now only recognised, it would then be realised. The details of the proposed reform, which are present to my own mind, would occupy more space than you could spare on one occasion from other important subjects. Nor is it possible in the brief limits of one letter to meet all those objections, now so well known to me, which start up directly this subject is mooted. Should, however, the remarks I have ventured to offer prove of sufficient interest to provoke discussion, I will on a future occasion solicit your permission to extend them.—I am, Sir, obediently yours,

Oct 22 ALEX. STRANGE, Lieut.-Colonel

The Earliest Mention of the Aurora Borealis

THE first appearance of the Aurora Borealis noticed in Mr. E. J. Lowe's "Natural Phenomena and Chronology of the Seasons" is that on Jan. 30, 1560. Other appearances are mentioned under the years 1564, 1574, and 1575. No further record of it appears until Nov. 10, 1707, when it was seen in Ireland. Five more displays are noticed between this and the memorable one of Feb. 23, 1716, which, happening to take place on the day of Lord Derwentwater's execution, obtained for the phenomenon in the north of England the appellation of "Lord Derwentwater's Lights." On March 6 of the same year occurred another grand display, which is referred to in the chronologies of remarkable occurrences published in the almanacks of last century as "The Great Amazing Light in the North," continuing to be seen (more or less) at several times since, yearly. Previous displays in this century had probably not been visible in London. The phenomenon is thus described, with an attempt at explanation, in the *Flying Post* of March 8:—

"Last Tuesday night, as soon as it was dark, a pale sort of a light broke out in the north-west part of our horizon, which looked like the dawn of day, or rather like the moon breaking through the clouds It darted many streams towards all parts of the sky, which looked like smoke. It proceeded towards the S.E., and continued by several intervals till midnight, when it totally disappeared. Some ignorant people, whose ideas are on such occasions stronger than their senses, fancied they saw armies engaged, giants with flaming swords, fiery comets, dragons, and the like dreadful figures; and others fancied they heard the report of fire-arms, and smelt powder; whereas there was nothing but what may easily be accounted for from natural causes, the sun having been hot for two days past, and particularly that afternoon, by which vapours were exhaled both from the earth and water, and the sulphurous particles mixed with them taking fire might occasion that light, and some coruscations, as is very common over marshy and fenny places in spring and summer nights."

The writer goes on to observe that "the disaffected party have worked this up to a prodigy, and interpret it to favour their cause," which accounts for a very obvious design to write the phenomenon down. Another display, not in Mr. Lowe's list, was witnessed at Leominster, on Feb 21, 1718, as appears by a letter in the *Weekly Journal* of March 1. The streamers are there compared to the tail of the great comet of 1681.

London, Nov. 7 R. G.

THE fallacy of trusting for scientific information to any other than a recognised scientific source, cannot be better illustrated than by Mr. Pocklington's letter in your issue of Nov. 3. He there seems to thinks that the statements of the editor of a volume of popular poems on a matter of science are worthy of notice, and therefore thinks it worth while to inquire whether or not it is true that no aurora borealis ever appeared before 1715. The absurdity of such a rash statement is so apparent that it seems almost superfluous to show it. In 1754 a book was published by M. de Mairan, entitled, "Traité Physique et Historique de l'Aurore Boréale," in which he collects from all the writers, ancient and modern up to that date, accounts of all the Auroræ Boreales which had been seen. Their total number amounts to 1,441 between the years A.D. 583 and 1751.

These are divided as follows: From A.D. 583 to 1354, 26 were recorded; 1354 to 1560, 34; 1560 to 1592, 69; 1592 to 1633, 70; 1633 to 1684, 34; 1684 to 1721, 219; 1721 to 1745, 961; 1745 to 1751, 28. Of these, 972 occurred in the winter half year, and 469 in the summer half year, the greatest numbers occurring in March and October Since that date the two most remarkable displays have been those of the 23rd of October, 1804, and the 24th of October, 1847. An account of the latter aurora was published at Cambridge in the same year, giving twelve large coloured lithographic views of the brilliant display which are, without doubt, the best views ever given of any Aurora.

J. P. EARWAKER

Merton College, Oxford, Nov. 5

THE quotation given by C. Pocklington in your last issue as the words of the Editor of Routledge's edition of Collins's Poems, is the very note given by Dr. Langhorne in the "Poetical Works of William Collins," published in the year 1808, in a small book entitled "The Laurel," and as it has not been reprinted word for word its sense is somewhat obscured. In the original it runs thus:—

"By 'Young Aurora' Collins undoubtedly *meant* the first appearance of the Northern Lights, which happened about the year 1715, at least it is *most* highly probable, from *this* peculiar circumstance, that no ancient writer *whatever has* taken any notice of them, nor even any *one* modern, previous to the above *period*."

The words restored are those italicised. The passage, as it came from Dr. Langhorne, is excusable for the knowledge displayed, but cut up as it is by the *modern* editor, shows great lack of it; and more than this, the honesty is not what every one would admire. Both, however, are incorrect. Dr. Halley, in the Phil. Trans., No. 347, page 406, gives a history of auroral observations, and for the information of "C. P." I have extracted a few particulars showing that the "northern lights have been observed and recorded long before 1715.

The first account, says Dr. Halley, recorded in English annals is that of the appearance which was noticed January 30, 1560, and called "Burning Spears," by the author of a book entitled "A Description of Meteors," by W. F., D.D. London: 1654. The next of a like kind was the appearance recorded by Stow, which occurred on October 7, 1564.

In 1574, Camden and Stow inform us, an Aurora Borealis was seen for *two* successive nights, viz., the 14th and 15th of November, with appearances similar to those observed in 1716, and which are not commonly noticed. The same phenomenon was twice seen in Brabant in 1575, viz., on the 13th of February and the 28th of September, and the circumstances attending it were described by Cornelius Gemma, who compares them to spears, fortified cities, and armies fighting in the air. In the year 1580, M. Mastline observed these *phasmata*, as he calls them, at Baknang, in the county of Wurtemburg, in Germany, no less than seven times in the space of twelve months; and again at several different times in 1581. On September 2, 1621, the same phenomenon was seen all over France; and it was particularly described by Gassendus in his "Physics," who gave it the name of "Aurora Borealis." Another was seen all over Germany in November 1623, and was described by Kepler. Since that time, for more than eighty years, we have no account of any such phenomenon either at home or abroad. In 1707 Mr. Neve observed one of small continuance in Ireland. In the years 1707 and 1708 this sort of light had been seen no less than *five* times.

There is not the least doubt in my mind that the commentator of Collins must have been wholly ignorant of the literature of scientific records, else he would never have said what he did on the lines in question.

In the Orkneys the Northern Lights are known by the name of the "merry dancers." And it is not at all surprising among an unphilosophical people, that this, one of the grandest phenomena in nature, should be the subject on which the imagination fondly dwells.

The various conflicts of Odin may probably have been suggested by the dancing and flickerings of these Lights.

In the "Prosa Edda" there is a direct allusion to the Aurora Borealis; at least, the translation given in Mallett's "Northern Antiquities," edited by J. A. Blackwell (1847), page 404. It says:—"From his skull," continued Thridi, "they formed the Heavens, which they placed over the earth, and set a dwarf at the corner of each of the four quarters. These dwarfs are called East, West, North, and South. They afterwards took the wandering sparks and red hot flakes that had been cast out of Muspellheim, and placed them in the Heavens, both above and below, to give light unto the world, and assigned to every *other errant coruscation* a prescribed locality and motion."

If by "errant coruscation" he meant all the meteoric phenomena including the Northern Lights, then we have in this "Edda" the most ancient record of this observation.

JOHN JEREMIAH

43, Red Lion Street, Clerkenwell, Nov. 4

Hereditary Deformities

THE following instance of hereditary deformity is taken from Mr. L. W. Dillwyn's "Materials for a Fauna and Flora of Swansea and the Neighbourhood" (Swansea, 1848), a privately printed and therefore little known book. It will be seen that the evidence respecting the origin of the malformation is not conclusive: "In 1804 there was in the Neath Valley a remarkable breed of a sort of sheep-dog, with nothing more than a flat depression, about half an inch broad, between the nostrils, and was said to have originated in a bitch which had her nose longitudinally cloven by some accident. The breed retained this deformity for several years, but I believe it is now extinct."

The same book contains the following very remarkable illustration of the dispersion of species by means of oceanic currents: "On the sandy sea-shore, opposite the race-course on Crumlyn Burrows, and more than a mile from any sort of house or garden, Mr. L. L. Dillwyn, in 1839, found a thriving young plant of *Yucca gloriosa*, and it had all the appearance of having risen from a seed which the tide had cast there. Notwithstanding its exposed situation, and the looseness of the soil, this native of Carolina was not materially injured by the unusually severe winter of 1840-1, and Mr. Moggridge informs me that for two or three years it continued to thrive, till it was destroyed by a heap of shingle, which a violent storm and high tide threw over it."

R. G.

London, Nov. 7

Fertilisation of Plants

WITH candle in hand I have pored through all that you have printed of the speeches delivered by the members of the British Association at Liverpool, till I made a full stop at page 482, where I found "Observations on Protandry and Protogyny in British Plants, by A. W. Bennett, F.L.S.—The arrangement of the reproductive organs in hermaphrodite plants, the presence in the same flower of both pistil and stamens, suggested to the minds of the older botanists no other idea than that of self-fertilisation. It is, however, now generally admitted that even in hermaphrodite flowers, cross-fertilisation is the rule, self-fertilisation the exception. Two sets of facts have been especially observed—in particular by Darwin in this country, Hildebrand in Germany, and Delpino in Italy—to favour cross-fertilisation in hermaphrodite flowers, the phenomena of dimorphism and trimorphism, and the special arrangements which render it easier for the pollen to be brushed off by insects visiting the flowers than to fall on its own stigma." I cannot understand what this special arrangement which renders it easier for the pollen to be brushed off by insects visiting the flowers, can mean, when applied to the flowers of our forced peaches, French beans, &c., in blossom during the dark months of December, January, February, and March, when there is no insect on the wing.

As I do not keep bees in my garden, and there are none kept within a mile of it, and if there should a swarm of bees take up an abode near us, I destroy it, my peaches blossom and set their fruit without the assistance of the bee or any other insect. It is the sun that sets the blossoms, so to speak; and consequently we make the best use of every gleam of sunshine that by chance may visit the earth during the blooming-time of our forced "things."

From this fact it must be evident that the Creator did not leave the all-important functions of the fertilisation of flowers to the insects which are simply in search of food, though it amuses our great thinking and closely observing philosophers to try to assure us plodding practicals of our sheer ignorance in the *modus operandi* of the fertilisation of hermaphrodite flowers.

That insects disperse the pollen of flowers there cannot be a doubt. Neither is there a doubt that hermaphrodite flowers fertilise their own pistils during the dark months of winter, without the presence of plundering insects. An industrious morphologist might find much employment in an early forced peach-house, in flower, say in January, on which the sun may not have gleamed since the first blossom opened.

A PEACH-GROWER

[Our correspondent should read the article in NATURE, No. 1, by Mr. Bennett, "On the Fertilisation of Winter Flowering Plants," which he will find to be in accordance with his own views.—*Ed.*]

Chip Hats

IN the last number of NATURE it is stated that palm-leaf was formerly imported to St. Alban's for the purpose of making *chip* hats. Allow me to state that the trade has never ceased. Large quantities of palm-leaf are imported under the name of "Brazilian grass," and many persons are constantly employed in St. Alban's and the villages around in plaiting these hats, principally for exportation. They are called *grass* hats.

Kew, Nov. 16

F. H. HOOKER

The Electric Telegraph and Earthquakes

THE use of the electric telegraph for recording earthquakes is not so new as the *Echo* (quoted in your last number, p. 35) supposes. Dr. Hector, Director of the Geological Survey, has, ever since the last great convulsions, systematically used the telegraph for this purpose in New Zealand, where indeed it forms an important element in the admirable system he has inaugurated for recording shocks throughout the islands. In his last letter he writes as follows:—"Not long ago, one operator asked another 200 miles distant 'Did you feel that?' and got the answer, 'No. What? Yes; there it is,' all in a breath, so to speak!"

Dr. Hector, writing in August last, goes on to say, "I have called attention to the coincidence of the aurora in both hemispheres on April 5, and can't help thinking that when our observations are sufficiently extended, we shall find many phenomena that are looked upon as local to be general. Could not NATURE give us a column recording such phenomena as auroras, earthquakes, tornados, &c., experienced in all parts of the world, somewhat in the form of an almanac? At the present one has to rummage over all sorts of periodicals, and after all find the matter most imperfectly recorded. We shall soon have a capital earthquake register here, since I induced the Government to cause the observers to report every shock they felt in the colony. Their number and coincidence is very remarkable, and I shall publish the results as soon as I have collected a sufficient number. I feel a great want of a good table eusmometer which should be cheap enough to distribute to all telegraph stations." Perhaps some of your readers can inform Dr. Hector where he can procure this great desideratum.

JOS. D. HOOKER

Ocean Currents

AMONG the "Notes" in last week's number it is mentioned, on the authority of the *Inverness Courier*, that a number of glass globes had lately been washed ashore on the western coast of the Isle of Lewis. The question is asked, "Have these been used for some experiments made for the purpose of ascertaining the course of some ocean current?" I have seen precisely similar globes which had been cast ashore after rough weather on the western coast of Shetland; and I ascertained that they were floats used by Norwegian fishermen for buoying their long lines. North-easterly winds had drifted them to that part of the coast where they were found; and the same cause may be assigned for the occurrence of the glass globes or floats in the outer Hebrides.

J. GWYN JEFFREYS

Nov. 12

IN reference to the statement (NATURE, Nov. 10) that glass globes have been washed ashore on the west side of the Isle of Lewis, may not these be floats which are used by the fishermen of Newfoundland? These are, I believe, occasionally found as far to the north-east as Nova Zembla, and this fact is, I think, not indicative of "some ocean current," but of the *aerial* current from the south-west, which is so prevalent in the north temperate region, and which may be called the *return* current of the north-east trade wind of the north tropical region.

This south-west wind from the sea modifies the summer heat and the winter cold of west coasts in the north hemisphere, and produces their so-called 'insular climate."

GEORGE GREENWOOD, Col.

Brookwood Park, Alresford, Nov. 12

The Milky Way

THERE appears to be in Wales a remnant of a tradition connected with the Milky Way. During a short stay in Caermarthenshire, an old man, well read in local history, and who is apparently the oracle of the neighbourhood of Llangadock, directed my attention one evening to the Milky Way, remarking at the time "we shall have fair weather to-morrow, as you see it is in the south," meaning that the wind will blow from that quarter. My friend supported this extraordinary statement by appealing to the Welsh word "Heol y Gwynt," the *road or way of the wind.* Can any of your readers inform me whether this belt of stars is the subject of a fable in Britain, or how it came to be connected with foretelling the weather? The Scandinavians call it the "Road of Winter;" possibly "Heol y Gwynt" may be traced to northern influence, but, in the absence of facts, I will not commit myself to this explanation.

JOHN JEREMIAH

43, Red Lion Street, Clerkenwell, Nov. 12

P.S.—"Heol y Gwynt" is the only proper Welsh name for the Milky Way, and is not a mere local one.

The Colour of Butterflies' Wings

IN making some experiments a short time ago, I came across a fact of which I was hitherto ignorant. I wished to test the effect of acid on the colours of the wings of a butterfly or moth, and with this view applied muriatic acid to a dried and set specimen of the Six-spotted Burnet (*Zygæna filipendulæ*). The red parts immediately became yellow, while the dark parts were unaltered. In subsequent experiments the red was the only colour in any moth which underwent any change. When there was no red there was no change, and the only change was from red to yellow. Next I applied the acid to the red parts of the Red Admiral Butterfly (*Vanessa atalanta*), when, to my surprise, no change took place. Comparative examination under the microscope in no way cleared up the matter. I now seek for an explanation of this phenomenon, which appears to point out a clear difference in the nature of the moth and butterfly's wing. A remarkable fact, perhaps connected with this, is that a yellow variety is known of almost every moth containing red in the wings. Perhaps some of your scientific readers may be able to throw some light upon this subject.

E. V. F.

Winchester, Oct. 18

A New Mode of Evolving Light

A SINGULAR phenomenon of the evolution of light has been recently observed by me. By tearing sharply a piece of twilled calico into strips in a room well guarded from light, a perceptible luminosity was clearly distinguishable, which appeared at its maximum at the final parting of the fabric. This phenomenon is exceedingly well marked in dry, new calico, and appears to me due to the dressing, as after being washed no light is evolved. Whether attributable to electricity, phosphorescence, or fluorescence, I leave for further investigation. The light appears similar to that produced on breaking a lump of sugar in the dark. So far as I can ascertain, the phenomenon of light being evolved on tearing a fabric is new, hence I hope worthy of notice in your valuable journal.

ANDREW PRITCHARD

Canonbury, N.

Philology and Darwinism

IN NATURE, No. 30, I attempted to show that the analogy between Mr. Darwin's teachings as regards plants and animals, and the conclusions of comparative philologists, broke down, when we compared man's conscious influence on plants, &c., to his more and more enlightened control of language. Man's influence on organic forms tends to produce variety, while, with increase of knowledge, language is becoming more uniform. Mr. Ransom (No. 32) replied that the difference I insisted on seemed imaginary; and if man's object was to produce uniformity in plants or animals, that then the domesticated species would be likely to become less varied than the wild species of the family. Now it seems to me that if man had any such intention, no care on his part could produce permanent types yielding so little divergence in the individuals during enormous time as those produced by nature; a permanence so marked that geology only throws light on the law of evolution, in anything like a direct way, through the study of the mammalia (see Prof. Huxley's recent address on the progress of Palæontology), and even with regard to the mammalia naturalists of high standing refuse to see anything but permanent and all but uniform types, necessitating the hypothesis of special creation. How man could obtain by any possible efforts (and with some breeds his aim is uniformity) to maintain species as invariable as nature has done, is what is hard to conceive

On the other hand, as man's mental faculties become blunter, his consciousness less vivid, and his material conditions harder, his language is more and more in a state of flux, branching forth continually into dialects which mark one group of men from another. Thus in some Polynesian islands we have a number of languages as distinct as those of great part of Europe; and missionaries complain that the Bible requires to be translated anew to the same tribe more than once in a century; in some cases the very numerals in a few generations becoming changed, partly from whimsical customs, and partly from want of frequent intercourse, and of any literature being at the disposal of the tribe. In the progress of time, we may look forward to a period when the language of Shakespeare, Milton, and Addison, which is considered as good English as that of our daily papers—may be the language of the world, but we can scarcely expect that the planet will ever hold only one species of animal or plant.

S. J.

NATURAL SELECTION—MR. WALLACE'S REPLY TO MR. BENNETT

MR. A. W. BENNETT'S article entitled "The Theory of Natural Selection from a Mathematical Point of View," contains several criticisms on my own writings, and touches on some points which have not yet been fully discussed. I propose, therefore, to reply to such of these as appear to be of sufficient importance.

The first objection brought forward (and which had been already advanced by the Duke of Argyll) is, that the very title of Mr. Darwin's celebrated work is a misnomer, and that the real "origin of species" is that spontaneous tendency to variation which has not yet been accounted for. Mr. Bennett further remarks, that throughout my volume of "Essays" I appear to be unconscious that the theory I advocate does not go to the root of the matter; and this unconsciousness is not apparent only, for I maintain, and am prepared to prove, that the theory, if true, does go to the root of the question of the origin of species. The objection, which, from its being so often quoted and now again brought forward, is evidently thought to be an important one, is founded on a misapprehension of the right meaning of words. It ignores the fact that the word "species" denotes something more than "variety" or "individual." A species is an organic form which, for periods of great and indefinite length as compared with the duration of human life, fluctuates only within narrow limits. But the "spontaneous tendency to variation" is altogether antagonistic to such comparative stability, and would, if unchecked, entirely destroy all "species." Abolish, if possible, selection and survival of the fittest, so that every spontaneous variation should survive in equal proportion with all others, and the result must inevitably be an endless variety of *unstable forms*, no one of which would answer to what we mean by the word "species." No other cause but selection, has yet been discovered capable of perpetuating and giving stability to some forms and causing the disappearance of hosts of others, and therefore Mr Darwin's book, if there is any truth in it at all, has a logical claim to its title It shows how "species," or stable forms, are produced out of unstable spontaneous variations; which is certainly to trace their "origin." The distinction of "species" and "individual" is equally important. A horse or a number of horses, as such, do not constitute a species. It is the comparative *permanence* of the form as distinguished from the ass, quagga, zebra, tapir, camel, &c., that makes them one. Were there a mass of intermediate forms connecting all these animals by fine gradations, and hardly a dozen individuals alike—as would probably be the case had selection not acted—there might be a few horses, but there would be no such thing as a species of horse. That could only be produced by some power capable of eliminating intermediate forms as they arise, and preserving all of the true horse type, and such a power was first shown to exist by Mr. Darwin. The origin of varieties and of individuals is one thing, the origin of species another.

Mr. Bennett next discusses the phenomena of "mimicry," and proposes to show, by mathematical calculations, that the effects could not be produced by natural selection. But, at the very outset, he makes an important error, which seriously affects his subsequent reasonings; for he leads his readers to understand that there is only one completely mimicking species of *Leptalis*, while the majority are of the normal white butterfly type. The fact is, however, that but few species of *Leptalis* retain the simple colouring of their allies the Pieridæ, while the great majority are either coloured like the Heliconidæ, or show a considerable amount of colour or marking in that direction. He is also apparently unaware that some Heliconidæ (*Ithomia eurimediæ*, for example) approximate in colour to the normal white and yellow species of *Leptalis*, and thus renders it much less difficult to understand how a sufficient amount of variation in colour might occur at a first step, to produce a resemblance which, viewed at some considerable distance, would be deceptive, and therefore useful.

We next come to the demonstration by means of figures, and we here find still more serious errors. Mr. Bennett says, that supposing a *Leptalis* may vary in twenty different ways, one only being the direction required,—"the chance of any individual producing a descendant which will take its place in the succeeding generation varying in the required direction, is $\frac{1}{20}$; the chance of this operation being repeated in the second generation is $\frac{1}{20^2} = \frac{1}{400}$; the chance of this occurring for ten successive generations is $\frac{1}{20^{10}}$, or about one in ten billions;" whence it is concluded that there are overwhelming chances against any progressive variation in the right direction ever taking place. But first, I do not admit the assumption that only one variation out of twenty would be in the right direction; when it is remembered how great is the variety of the Heliconidæ, both in colour and marking. It seems more likely that one-fourth or one-third at least would help to approximate to some of them, and thus be useful. Taking, however, Mr. Bennett's own figures, there are three great oversights in this one short sentence. The first is, that each *Leptalis* produces, not one only, but perhaps twenty or fifty offspring; the second is, that the right variation has, by the hypothesis, a greater chance of surviving than the rest; and the third, that at each succeeding generation the influence of heredity becomes more and more powerful, causing the chance of the right variation being reproduced to become greater and greater.

Now with these three modifications the weight of the argument is entirely destroyed; for, allowing the *Leptalis* to produce only twenty offspring (a small number for a butterfly), the chances become even that one out of the twenty varies in the right direction. But nineteen out of the twenty, on the average, are soon killed off by the various causes that keep down the population of the species, and the chances are very much in favour of that one surviving which, by the hypothesis, has varied in the right direction. It is not pretended that this one would survive always, or even on the average, but in a large number of cases it would certainly do so; and taking Mr. Bennett's own estimate of a million individuals as the population of a rare species, we may fairly estimate that in a quarter, or say even in a tenth part of these, the surviving offspring would possess the favourable variation. But now a new factor enters into the problem, of which Mr. Bennett takes no account. Those that have already varied tend to leave offspring varying in the same direction as themselves; and as these will all have an advantage, the offspring of the one-tenth will increase at the expense of those of the nine-tenths; and this tendency being still more powerful in the third generation, with the additional advantage as the numbers increase of the chance of both parents being favourable varieties, we may fairly expect the favourable to have completely exterminated the unfavourable variations, and to have firmly established themselves as a well-marked race. The enormous possible rapidity of multiplication, enabling a pair of individuals to produce millions in a few generations; the survival of the fittest, giving to favourable variations—not their bare numerical chance, as Mr. Bennett supposes, but—a certainty in the long run of living at the expense of the rest; and the powerful influence of heredity, which actually increases the *tendency* to produce the favourable variations with each succeeding generation,—are three of the main foundation-stones of the theory of natural selection, yet all three are ignored in this attempted mathematical demonstration of its insufficiency.

There is one other point in the theory of the origin of "mimicry" that deserves notice. It is, that the modifications leading to it are much more easy to explain than those leading to new genera and families, because the changes effected are wholly superficial and are almost entirely confined to colour. Now colour is both more variable than any other character, and is less intimately correlated with structure, so that great changes of colour may rapidly occur without in any other way affecting the individual, as we see in almost all our domestic animals. Experiments in breeding show that very large spontaneous variations of colour are frequent in insects; and thus the number of steps to produce a required amount of change may be much fewer than in cases of structural modification, in which every other part of the organism has to be co-ordinated to work harmoniously with the modified organ.

I may here take the opportunity of denying that I have argued, as Mr. Bennett says I have, that "an infinitesimal and inappreciable distinction may make the difference of a slightly longer span of life being allowed to the butterfly to lay its eggs in safety;" and I cannot imagine how he could have imputed to me anything so absurd. What I have maintained is, that for natural selection to act, either in producing "mimicry," or structural changes, no large or special variations are required, because the usual amount of *variability* which occurs in every part of every organism is sufficient. ("Contributions," pp. 287—291.) But so far from supposing this to be "infinitesimal" or "inappreciable," I show that it is so palpable and so readily appreciated by horticulturists and breeders as to have enabled them to produce all the wonderful variety in our domestic animals and cultivated plants. And every entomologist knows that similar variability exists in insects, and that the constantly occurring variations of colour are especially great.

Mr. Bennett next returns to the laws of variation, and, because Mr. Darwin says that we are profoundly ignorant of these (although he himself has done so much to elucidate them), maintains that we cannot really know anything of the origin of species. As well might it be said that, because we are ignorant of the laws by which metals are produced and trees developed, we cannot know anything of the origin of steamships and railways. Spontaneous "variations" are but the materials out of which "species" are formed, and we do not require to know how the former are produced in order to learn the origin of the latter. But though we may not know the laws which determine each variation in detail, the general causes which lead to variation are not difficult to perceive. We do not know all the laws and causes that have given their peculiar form to each mountain or each valley, but we know a good deal of the general causes which have produced them, and we can perceive that the reason no two are exactly alike is, the number and complexity of the causes and the endless variety of conditions under which these causes have acted. In the far more complex operations of the development and growth of organisms, affected as we know they are by almost infinitely numerous and ever varying external and internal causes, it would be a much greater mystery if there were no variations, and if absolutely identical forms were produced by constant diversity of conditions. Even the successive offspring of the same parents are developed under very different conditions. At each succeeding year, and at every different period of each year, the parents have changed in age, in size, in vigour, health, and constitution; they may be living in a different locality, have different food, and be subjected to very different physical and mental influences. Add to this the effect of cross unions of distinct individuals, each with its own characteristic peculiarities, which are in varying degrees transmitted to the offspring; and further, that these modified offspring are submitted to a somewhat different set of conditions from the parents, and intercross perhaps with a distinct set of individuals and then add the effects of atavism in bringing up long lost ancestral characters, and it can hardly be said that the almost universal fact of "spontaneous variation" is quite unaccounted for. But, as I have already remarked, this variability could never by itself produce *species*, but must absolutely prevent their production without the eliminating, accumulating, and fixing powers of selection, multiplication, and heredity.

In Mr. Bennett's concluding passages he advances a theory of his own on the subject of "mimicry," to the effect that it is connected with intelligence or instinct, "and runs almost *pari passu* with the development of the nervous system." In support of this view he asserts that it is "strongly developed in birds." This is erroneous. In birds it is very rare, only two or three cases being known, and these not nearly so remarkable as hundreds that occur in insects; and in mammalia, with the exception of one doubtful case, it is absolutely unknown. This view, therefore, is directly opposed by the facts.

I have only one more point to notice, a charge of inconsistency against myself. Mr. Bennett quotes me to the effect that man's chief peculiarities of form and structure were developed before his intellect had raised him above the condition of the brutes, and also *imputes* to me the belief that certain peculiarities in his structure (the absence of hair on his body, for example) "must have been in some way connected with *his* reasoning powers." But this is Dr. Laycock's view, which I have expressly repudiated, and I have never used a word to show that I believed that man has modified his own structure in any important degree, by the conscious or unconscious exercise of his reasoning powers. I have, it is true, declared my belief that "some intelligence" has acted on him, but I have also, I think, made it quite clear that I did not believe it to be his own intelligence. The inconsistency, therefore, is of Mr. Bennett's making.

I think I have now noticed the chief points in this last assault on the theory of Natural Selection, which has failed, like all preceding ones. Its author also exhibits the usual inability to keep steadily before him the great fundamental principles of the theory he is discussing, so that his arguments continually break down owing to his taking a partial and wholly inadequate view of its mode of operation. In the case of "mimicry" he is not sufficiently careful in his statement of the facts, and this, combined with his imperfect grasp of the theory, entirely neutralises the elaborate numerical proofs which at first sight appear so overwhelming. ALFRED R. WALLACE

SCIENCE IN PARIS

IN the course of an article on the present condition of Paris, the *Engineer* gives the following account of the effects of the war. The use of the electric light is common to both sides in the present struggle, but the French have used it largely. The apparatus set up on Montmartre is arranged by M. Bazin, and is electro-magnetic. The central cylinder supports four series of double coils covered with copper wire enveloped in silk; the cylinder is rotated by a small steam-engine of 3-horse power, making 400 revolutions per minute. The lamp used is of the ordinary form, with the Foucault-Dubosc regulator. The reflector is parabolic in form, and the whole is surrounded by a shield to hide it from the enemy. This light, from its elevated position, commands the whole of Paris, and the plains around. A spectator on Montmartre sees distinctly the details of the *façade* of a building which stands 2,600 metres off; at 2,900 metres a man may be seen standing at a window, at 3,000 metres a mass of cavalry or infantry is distinguishable, and at 4,000 metres the dome of the Invalides, with its bands of gold, is brilliant. A man cannot be seen on the dome at that distance, but on walking towards the building all soon becomes clear. On the ramparts, at 3,800 metres from Montmartre, the light is sufficient to read an ordinary newspaper.

Thus, though the practical effect of the lamp only extends about 300 metres from its position, the field is illuminated to the extent of 700 metres, for the benefit of all placed between the light and the object. Thus a sentinel on the ramparts can see about 3,000 metres from the *enceinte*, and by this means strict watch is kept upon the plains around the city at night, as far, in one direction, as 1,000 metres beyond St. Denis. M. Bazin is now occupied in applying his apparatus to the purposes of night telegraphs, by the adoption of the system of flashes—mentioned some time since in the *Engineer*—and with the aid of coloured lenses. A corvette—the *Coligny*—already possesses such a signal apparatus, and the signals are distinctly visible at more than eight miles' distance. The action of the lamp, and also the movements of the apparatus, are remarkably steady, and M Bazin has received high testimonials from the authorities of the good working of his instrument.

M. Viollet-le-Duc, who is M. Alphand's second in command of the corps of civil engineers and architects aiding the military authorities, has made an interesting report to General Trochu respecting the works executed during the past month by the auxiliary engineers around Paris. It appears, according to this document, that the expense of these works has been only 105,000f., while under the military system they would have cost 230,000f. We have not seen the report, but we presume that M. Viollet-le-Duc, and the other architects and engineers, gave their services and advice gratis, and this would, of course, save the country a considerable sum.

The Government has voted the sum of 40,000 francs to enable M. Dupuy de Lôme to carry out his proposed plan of navigable balloons. The subject has been twice discussed in the Academy of Sciences, and although some members have advocated the use of a small steam-engine or other motive power, M. Dupuy de Lôme is no doubt wise in adhering to manual power, which presents all the force necessary with none of the inconveniences of machinery, and an adaptability to circumstances which no machinery can possibly possess. The men will form a crew to aid the landing of the balloon, or in extricating it from any difficulty, while any engine would be, when not in use, a dead weight and awkward encumbrance. Moreover an aërostat, with steam or other power, is now in construction by another inventor or adapter.

During the discussion M. Dupuy de Lôme showed by calculations that his balloon would have a constant ascensional force up to 870 metres altitude, but beyond that to 1,110 metres a little gas must be lost. At all events, between 250 metres and 870 metres the altitude could oscillate (by means of the extra pocket or swimming bladder) without loss of gas; and, of course, there would be the usual expedient of ballast to be depended on also. Dr. Monra advocated the use of heated air in place of gas. Fifty deg. Centigrade would be sufficient, and the heat might be retained by making the balloon double—that is to say, one balloon within another. The Aigle Mongolfière, said the Doctor, used to be inflated in twenty minutes, while it takes a whole day to fill a balloon (upwards of 1000 cubic metres) with gas. Mongolfier balloons sent up within twenty leagues of Paris would certainly fall in the city, and it is a pity they have not been tried before this. Should the siege not soon be raised, will not the English aeronauts or others try and send a few letters into Paris?

PROFESSOR HELMHOLTZ ON FARADAY

We have been favoured by Prof. Tyndall with the following translation of a portion of the preface to the German Edition of "Faraday as a Discoverer," recently superintended by Professor Helmholtz:—

"The name of Faraday is one to be held in reverence by all natural philosophers. Many times in London, in connection with lectures which I delivered at the Royal Institution, I had myself the privilege of his obliging help and the pleasure of his amiable society. The perfect simplicity, modesty, and undimmed purity of his character gave to him a fascination which I have never experienced in any other man. I had therefore a duty of gratitude to fulfil towards him.

"But apart from this, and apart from that friendship for Faraday's younger associate and successor, the author of this book, which induced me to undertake the task, I believed that I should render a service to German readers by facilitating, as far as in me lay, an insight into the action and character of a mind so richly and peculiarly endowed, and so entirely the product of natural growth.

"It is, moreover, by no means for the philosopher only that such an insight possesses interest. His interest, certainly, is the most immediate, for it has hardly been the lot of any single man to make a series of discoveries so great and so pregnant with the weightiest consequences as those of Faraday. Most of them burst upon the world as surprises, the products, apparently, of an inconceivable instinct; and Faraday himself, even subsequently, was hardly able to describe in clear terms the intellectual combinations which led to them. These discoveries, moreover, were all of a kind calculated to influence in the profoundest manner our notions of the nature of Force. In the presence of Faraday's magneto-electric and diamagnetic discoveries more particularly, it was impossible for the old notions of forces acting at a distance to maintain themselves, without submitting to essential expansions and alterations. The clearer expression of these changes is at the present hour the object of physical science.

"In what way such extraordinary results were achieved is naturally a question of the first interest to the investigator who strives after similar though more modest ends. But Faraday's development appears to me to possess no small human interest in relation to many theoretic questions of psychology, and to the art of education. The external conditions under which he cultivated those striking capacities which excite our wonder were the simplest that can be imagined. He was completely self-taught; brought up in humble circumstances, having received no more than the commonest instruction, and having been only favoured by fortune in the circumstance, that when he was a poor apprentice to a bookbinder, he found, at the right time, a helper in Humphry Davy, who recognised his peculiar gifts, and procured for him the possibility, though in a subordinate position, of working in the direction towards which his genius impelled him.

"And throughout his whole life and labours, the advantages and disadvantages of such a mode of development reveal themselves in simpler and larger traits than in the case of most other similar celebrated names. The principal advantage rose undoubtedly from the fact that his intellect was not too soon subjected to theoretic fetters, but enjoyed its freedom in the presence of natural phenomena; and that instead of book-learning he permitted the fulness of Nature herself to operate upon his open mind. The disadvantages are, perhaps, of a subordinate kind; but they reveal themselves in quite as unmistakeable a manner when he strives to give expression to his ideas, and to supply, by all kinds of sensuous imagery, the want of mathematical culture. This is manifestly the way in which he alighted upon his Lines of Force, his Ray Vibrations, and other notions, which bewildered the investigators of his time, and the truer and clearer meaning of which has been in part made out by mathematical theory since Faraday himself ceased from his labours.

"And still, in this unlearned son of a smith, who held fast throughout his life the pious creed of his fathers, ran a vein of philosophy which gave him the right to be ranked among the foremost of those engaged in the general intellectual travail of our age. That as Tyndall informs us, he retained the term "Natural Philosophy," usual in England to express physical science, and the name "Philosopher" for the cultivator of that science—lies essentially in the nature of his work. After the science of our age, in its laudable efforts to make human knowledge a true image of the actual universe, had shattered many an old metaphysical idol, it halted amid the transmitted forms of physical ideas regarding Matter, Force, Atoms, and Imponderables. These names were even converted into new metaphysical shibboleths by those who thought themselves most advanced in the way of enlightenment. It was these ideas that Faraday sought in his riper labours to purify from everything theoretical, which was not the true and immediate expression of the facts. More especially he opposed the action of forces at a distance, the assumption of two electric fluids and of two magnetic fluids, and, in like manner, all hypotheses which contradicted the law of the conservation of force, of which he had an early presage, though he singularly misapprehended its mathematical expression. And in these precise directions he exercised, in the first place, the most unmistakeable influence on the physicists of England. The mathematicians among them, especially, labour to render theories of phenomena the pure and true expression of the laws of fact, to the exclusion of all arbitrary theoretic devices. In this way Faraday's ideas, though in a modified form, often reveal themselves with their true significance assigned to them."

EARTHQUAKE OF OCTOBER 20, 1870

WE have been favoured with the following particulars of this earthquake by Prof. Newton, of Cambridge, U.S.A. :—

On Thursday morning, Oct. 20, an earthquake vibration was felt throughout Canada, and the northern part of the United States from Maine to Iowa. It seems to have been more severe in Canada and in New England. In many places the shock was sufficient to throw down chimneys, crack the walls of buildings, and do other damage. It was remarkably severe for the region of country visited. At New Haven, as well as in many other places, there were two distinct series of vibrations. Prof. Twining has carefully collected information from several persons as to the time of the occurrence and duration of the vibrations. The beginning of the first shock was at $11^h\ 19^m\ 45^s$ A.M. New Haven mean time. It lasted ten seconds, and its individual vibrations were about two-thirds of a second in duration, or one and one-third second for a complete double vibration. After an interval of five seconds there was a second series like the first lasting eleven seconds. The motion was not a simple oscillation, but there was a rocking motion, indicating a vertical component in the movement of the earth. The vibrations were not severe enough to arrest universal attention, though multitudes felt a peculiar sensation without recognising the cause. The direction of the vibration was N.N.E. to S.S.E. At Cambridge, Mass., according to Prof. Winlock, the direction was about 10° north of east, as determined by the appearance of the sides of a vessel containing milk. Mr. Farmer, at Boston, gives $11^h\ 25^m\ 37^s$ for the time of the ending of the vibrations, Cambridge mean time. This would imply that the shock reached Boston a minute and three fourths earlier than New Haven. At Cleveland, Ohio, several clocks were stopped by the earthquake, each indicating very nearly $10^h\ 45^m$ A.M. This is approximately the instant at which the shock reached New Haven. It is reported that the shock reached Quebec 30 seconds before it did Montreal, the telegraph operator of the former city being in the act of inquiring of the operator in the latter one respecting the earthquake, when it arrived at Montreal. These data seem to show that the general progress of the wave was from North to South. Slight vibrations were felt as far south as Richmond, Va., and as far west as Dubuque, Iowa.

NOVEMBER METEORS OBSERVED AT THE RADCLIFFE OBSERVATORY

THE following is a list of Meteors observed at this Observatory by Mr. Lucas, on the nights of November 12, 13, and 14, 1870.

On Nov. 12 a watch was kept from $7^h\ 15^m$ to $8^h\ 30^m$, and from 11^h to $13^h\ 30^m$.

At $8^h\ 20^m$ a meteor was seen of the 4th magnitude, from Capella, a little to northward.

At $12^h\ 29^m$, one of the 1st magnitude, white, flashed from α Cephei to α Cygni.

At $12^h\ 48^m$, one of the 1st magnitude, from α Ursæ Majoris to Polaris, below the stratus cloud which overspread the sky, and nearly hid all the stars, the two mentioned being just visible. Duration, $1^s{\cdot}5$.

At $12^h\ 57^m$, one of the 1st magnitude, white, seen for an instant about 7° or 8° east of Polaris, appearing to burst over the point of appearance; motion southwards.

Nov. 13.—At $8^h\ 5^m$, one of the 3rd magnitude, visible for 3^s, downwards towards the south.

At $8^h\ 20^m$, one of the 1st magnitude, visible for 2^s, with a long train, from ε Cygni to α Aquilæ.

At $9^h\ 38^m$, brighter than Jupiter, of a blue colour, visible for 4^s, from near Capella to ο Ursæ Majoris.

The sky was overcast at $11^h\ 30^m$, and continued so nearly all the night, raining from 14^m to 16^m.

Nov. 14.—At $10^h\ 54^m$, one of the 1st magnitude, white, visible for $1^s{\cdot}5$. From β Aurigæ to a cloud near 25 Lynxis; direction, N.E.

At $11^h\ 1^m$, one of the 1st magnitude, visible for $1^s{\cdot}5$. From a cloud a little to the west of Rigal; downwards.

At $11^h\ 19^m$, one of the 1st magnitude, visible for 2^s; near [illegible] and χ Andromedæ to α Cygni.

At $12^h\ 33^m$, one of the 4th magnitude. From α Cephei downwards; a short path.

Cloudy from 10^h to $11^h\ 30^m$; tolerably fine afterwards till 13, when clouds covered the sky again.

ROBERT MAIN

NOTES

WE are glad to announce that the Government has expressed its intention to aid in the most ample manner the proposed Eclipse Expeditions. In making this announcement we feel that what has recently appeared in the daily Press renders certain explanations desirable, which otherwise might have been omitted. In our last number we stated that deputations had been appointed both by the Joint Committee and the Council of the British Association to wait upon Mr. Gladstone. To this we must now add that the letter of the Secretary of the Joint Committee, which was to ask Mr. Gladstone to appoint a time to receive a deputation, was not sent to Mr. Gladstone, and did not ask that a time should be named; in fact it was a letter sent to the *Treasury*, apparently only for their information, and was nothing more than a copy of the resolution passed at the meeting of the Joint Committee. After a week had elapsed without any answer being received by the Joint Committee to the letter which it was supposed had been sent to Mr. Gladstone, asking him to name a time for a deputation; a member of the Joint Committee, Mr. Lockyer, called at the Treasury to inquire the reason of the delay in the answer, and of course he was informed that no letter had been received requiring any answer. Upon it being represented that a delay in the announcement of the Government intentions until a proper letter could be received from the Secretary of the Royal Society would be fatal to the Expedition, Mr. Lockyer was requested to state the actual requirements of the Scientific Bodies to Mr. Lowe, and upon his having done so, the Chancellor of the Exchequer, who, so far as we know, heard then of the expedition for the first time, at once expressed his opinion that such an expedition was one eminently worthy of Government aid, and that the Government would do all that was requisite to further the objects sought. This decision of the Government was announced to the Royal Astronomical Society by the Astronomer Royal on Friday last, and since then, the arrangements for the Expedition have moved apace. There will be a ship furnished by the Government to carry observers to Spain. There will be funds to convey observers overland to Naples, and a ship to carry them on to Messina. The various European governments have been requested to aid the various parties, and, generally, the influence of the Government is being brought to bear in every way. This taken in connection with what has appeared in these columns before, and a letter which the Astronomer Royal has addressed to the *Daily News*, shows that now all the facts are out, the whole blame of the long delay must fall on the officers of the Joint Committee, who, apparently unable to perform the duties entrusted to them, still did not call the Committee together to receive instructions We should not write in so decided a tone on such a painful subject did we not feel that it is simply an act of justice to the Government to state, as plainly as our information enables us to do, exactly where the real blame rests.

It will have been observed that Dr. Carpenter's letter in our number for Oct. 27, called in question, on the authority of Sir R. I. Murchison, the accuracy of our report of the conversation which followed the reading of Mr. Gwyn Jeffreys' paper in Section C of the British Association. We have communicated, in consequence, with Mr. Jeffreys, by whom we were favoured with the report in question, and who assures us that "to the best of his recollection and belief the report was quite accurate as conveying the purport of Sir Roderick Murchison's remark." While, therefore, we are unable to decide the relative accuracy of two different recollections of the same circumstance, we cannot but congratulate ourselves that the misunderstanding, if it were so, has drawn out such a full exposition of Dr. Carpenter's views of the bearings of the *Porcupine* discoveries on modern geological and physico-geographical theories.

In addition to the notices of papers to be read at the meetings of the scientific societies in London, we shall be glad to insert in our "Diary" the titles of those to be read at the leading provincial societies, when of sufficient interest, and if forwarded to us in good time.

Brasenose College, Oxford, offers an open fellowship for Natural Science or Mathematics in February next. Candidates for Natural Science must communicate with the Principal by letter not later than the 20th December. As this is one of the few colleges which have hitherto held entirely aloof from Natural Science, this fellowship is clear evidence that the value of such studies is yearly becoming more appreciated at Oxford.

There will be offered for competition, at St. John's College, Cambridge, in 1871, an Exhibition of 50*l.* per annum for proficiency in Natural Science, the Exhibition to be tenable for three years in case the Exhibitioner have passed within two years the previous examination as required for candidates for honours: otherwise the Exhibition to cease at the end of two years. The candidates will be examined in (1) Chemistry, including practical work in the laboratory: (2) Physics, viz., electricity, heat, light; (3) Physiology. They will also have the opportunity of being examined in one or more of the following subjects—(4) Geology, (5) Anatomy, (6) Botany, provided that they give notice of the subjects in which they wish to be examined four weeks prior to the examination. No candidate will be examined in more than three of these six subjects, whereof one at least must be chosen from the former group. It is the wish of the masters and seniors that excellence in some single department should be specially regarded by the candidates. They may also, if they think fit, offer themselves for examination in any of the classical or mathematical subjects. The Exhibitions are not limited in respect to the age of candidates. The examination will take place on April 21st and 22nd, and the names of candidates must be sent in at least ten days beforehand.

The series of science lectures addressed to working men at Manchester are a great success. Three weeks ago Prof. Huxley opened the series by a very interesting lecture on Coral Islands. On Wednesday se'nnight, Prof. Roscoe lectured on Spectrum Analysis, and this week Mr. Huggins lectured on the Spectrum Analysis of the Stars. These lectures, which are to be continued through the winter, have been literally crowded by most attentive audiences, and since full reports are published in the Manchester daily papers, there is every hope that much permanent benefit will be derived from them.

The following are the lecture arrangements at the Royal Institution of Great Britain for 1870-71:—Christmas lectures (adapted to a juvenile auditory), Prof. Odling—Six lectures "On Burning and Unburning," on Dec. 27, 29, 31, 1870; Jan. 3, 5, 7, 1871. Before Easter, 1871, Prof. Michael Foster—Eleven lectures "On the Nutrition of Animals," on Tuesdays, Jan. 17 to March 28; Prof. Odling—Eleven lectures "On Davy's Discoveries in Chemistry," on Thursdays, Jan. 19 to March 30: Mr. W. H. Channing—Four lectures "On the Progress of Civilisation," on Saturdays, Jan. 21 to Feb. 11; Prof. Jowett—Three lectures "On Socrates," on Saturdays, Feb. 18, 25, and March 4; Mr. H. O'Neil—Four lectures "On the Spirit of the Age," on Saturdays, March 11 to April 1. The Friday evening meetings will commence on Jan. 20 The Friday evening discourses before Easter will probably be given by Profs. Tyndall and Odling, the Dean of Westminster, Mr. E. J. Reed, Mr. James W. Douglas, Dr. Carpenter, Capt. Noble, Prof. Clerk Maxwell, Mr. Norman Lockyer, and Mr. W. Mattieu Williams. After Easter:—Prof. J. J. Sylvester—Three lectures "On Emanuel Kant," on Tuesdays, April 18, 25, and May 2. Mr. Charles Brooke—Two lectures "On Force and Energy," on Tuesdays, May 9 and 16. Prof. Tyndall—Eight lectures, on Thursdays, April 20 to June 8. Mr. Norman Lockyer—Eight lectures "On Astronomy," on Saturdays, April 22 to June 10.

The *British Medical Journal* reports that at a meeting of a committee held in Edinburgh on November 7, it was stated that the form of the national memorial to the late Sir James Y. Simpson had been agreed upon as follows: 1. A monument and statue in Edinburgh; 2. A marble bust in Westminster Abbey; 3. A hospital in Edinburgh for the diseases of women, constructed on those principles which Sir James so often and so clearly enforced; 4. Similar hospitals in London and Dublin, should sufficient funds be obtained. It was also stated that a sum of 1,950*l.* had already been subscribed.

After the conclusion of the ordinary business of the meeting of the Royal Medical and Chirurgical Society on Tuesday the 8th inst., the meeting was made special for the purpose of confirming, or rejecting, the following resolution, proposed by Mr. Paget, seconded by Dr. Quain, and passed at a special meeting of the Society on October 25th:—"That the Council be requested to consider whether, while maintaining the charter and constitution of the Royal Medical and Chirurgical Society, it may be possible to obtain a more complete co-operation with the Pathological, Obstetrical, Clinical, and Epidemiological, or other societies for the promotion of Medical Science." After a lengthened debate, Dr. Paget's resolution was confirmed by a small majority.

The *Chemical News* gives a short account of a convenient form of spectroscope for use in a laboratory, by Mr. John Browning. It is so constructed that it may be kept in close proximity to a chemical laboratory without injury. The prism is provided with a cover, which should be put on with a little bees'-wax; or, better still, bees'-wax and tallow. This prism, with cover complete, can be removed, and replaced without deranging the adjustment of the instrument, to allow of a bottle prism being substituted for the purpose of taking the refractive index or dispersive power of any liquid The stand of the instrument is of wood, and the whole is enclosed in a circular cover which fits tightly round the base of the instrument, and has no other joint or opening.

THE Leeds Naturalists' Field Club will hold the following conversational open meetings, at the Rooms, South Parade, every alternate Monday evening, at eight o'clock:—(1870), November 7, "A November Day at Boston Spa," Mr. J. W. Taylor; November 21, "Geology as a Study," Mr. L. Acomb; December 5, "Life History of the Painted Lady Butterfly (*Vanessa Cardui*)," Mr. W. Turner; December 19, "Animalculæ," Mr. W. Coates; (1871), January 2, "The History of a Mushroom," Mr. J. Abbot; January 16, "Wasps," Mr. W. D. Roebuck; January 30, "Our Trees and their Uses," Mr. Jas. Brodie; February 13, "Protozoa," Mr. T. Hick, B.A.

THE Annual Exhibition of the Photographic Society of London was opened at the Architects' Gallery, No. 9, Conduit Street, by a private reception of their friends by the members of the Society, and the exhibition will remain open to the public until the last day of the current month of November. The exhibition is quite as large as was the eminently successful exhibition of last year, and certainly leaves the impression that there is manifest and unmistakeable evidence of progress in the art during the year. The work of many of the leading and best known exhibitors is in advance of their exhibited specimens of the last year, and there are some individual photographs now to be seen upon the walls of the gallery which it can hardly be too much to say are of higher excellence, both as pictures and as photographs, than any that have before been produced. We may mention in particular ten large portraits by Col. Stuart Wortley, taken direct from life, and marvellous specimens of tasteful and exquisite portraiture; two large portraits, also from life, by Mr. Warwick Brookes, of scarcely inferior power and excellence; Vandyke and Brown's solar camera enlargements of the poet Longfellow; and Mr. Blanchard's large Rembrandtesque life portraits. Of large landscape work, Mr. Care, of Worcester, deserves special notice; and Mr. Robinson's sea pieces, "Turn of the Tide" and "Outward Bound," are admirable bits of true nature. Mr. Robert Faulkner has some very pretty applications of the instantaneous branch of the art in catching the fitful aspects and characteristics of children; and Mr. Vernon Heath has two exquisite bits of true scenery and foliage; nor ought we to pass by "Hop-picking," by Mr. Stephen Thompson, and Captain Lyons' Indian pictures; and we may add to this list Mr. Manners Gordon's small pictures from dry plates.

HERR A. PETERMANN has published a brief paper, in which he recapitulates the main results of the various North Pole Expeditions of the present year. Herr von Heuglin and Count Zeil, of the German Expedition, remained from July 15 till Sept. 15 in and near East Spitzbergen, which they explored, mostly in boats, from 77° to 79° N. lat. They claim to have discovered extensive land to the east of Spitzbergen. This land, Herr Petermann maintains, it is a mistake to identify with the land discovered by Gillis in 1707, which lay 80° N. lat. The land which was seen from the White Mountain of Spitzbergen by the Swedish Expedition in 1864, 80 nautical miles to the east, was put down on the map as a neck of land lying 79° N. lat. "Herr von Heuglin and Count Zeil," says Herr Petermann, "have now discovered, 36 nautical miles to the east of Spitzbergen, a continent, extending from 79° to 78° N. lat.—therefore, from north to south, at least 60 (German) miles—which contains numerous sharply-pointed peaks, and which, in case it is really connected with Gillis Land, might at least equal Spitzbergen in size." This is claimed as the most important polar discovery that has been made for some years. Herr Heuglin has brought home with him from East Spitzbergen fourteen chests of geological, zoological, and botanical specimens. The news of the war reached the explorers in September. Count Zeil, a lieutenant in the Second Royal Würtemberg Jäger Battalion, at once hastened home to Stuttgart, and, having had an audience of the King on October 20, proceeded forthwith to his regiment in France. Herr Petermann announces that the Russian Expedition, which has been accompanied by the famous academician, Von Middendorff, has prosecuted interesting scientific researches between Novaia Zemlia and Iceland. Among other things, it is said, he has identified the Gulf-Stream as far as Novaia Zemlia at the very considerable temperature of +10° Réaumur. With reference to an article by Herr Petermann on the subject of the Gulf-Stream, Herr von Middendorff writes to him:—"I am extremely glad that your theory respecting the extension of the Gulf-Stream is not only confirmed, but has even been greatly surpassed; you were bold, but Mother Nature is still bolder."

AT a time when so much is being said about the value of fungi in general as profitable and wholesome articles of food, and also when France is being so largely overrun by foreign troops, the following notes on the Truffle cultivation in the Department of the Dordogne, written a short time before the outbreak of the present war, may be of some interest. It shows the money value of these delicacies, and how profitable a business is their cultivation. The method adopted for propagating them is to sow acorns, and the best truffles are found under the resulting oak-trees; but the evergreen oak, and juniper trees are also grown for the same purpose. An instance is cited of a person who inherited a piece of land worth 8*l.*, and who thus sowed it with acorns; the truffles thereby obtained realise now as much as 160*l.* a year. There are many varieties easily distinguishable to those accustomed to the trade. The truffle is dearer in Périgord than in Paris, where it is mixed with an inferior quality, and therefore can be sold at a lower price. It comes to perfection about the middle of November, but large quantities are collected and sent to market in September and October. These are called "fleurs," and are without smell. It is pretended that they have not come to maturity, and that a large portion of the produce is thus spoilt. The total revenue derived from the truffle commerce amounts to 20,000*l.* a year in the Arrondissement of Tarlat, and to about the same amount in Périgueux.

IT is interesting to note the progress the Japanese are making in the art of printing, &c. Hitherto they have only been acquainted with the Chinese mode of printing, from engraved wooden blocks. Lately, however, they have engaged the services of an English gentleman, to set up for them an establishment for type-founding, electrotyping, and printing on the Western method, and to give them such instructions in these arts as will enable them afterwards to carry on the business. Type-founding and electrotyping have now for the first time been introduced into Japan.

AT the last ordinary meeting of the Hackney Scientific Association, held on Nov. 8th, Mr. Henry T. Vivian announced that he had discovered the variability of ϵ Herculis, a small star to the N.E. of π. From observations extending back to the autumn of last year, he had deduced a period of about 21 days, with probably a second longer period. The amount of variability in the star's lustre is from a large 5th mag. to a small 6th. The star is numbered 60 in Map 10 of Mr. Proctor's New Star Atlas, although in Map 8 it is numbered 69; evidently an error on the part of the engraver.

THE MICROSCOPE

THE VALUE OF NITRATE OF SILVER AS A REAGENT FOR DEMONSTRATING MINUTE STRUCTURE.—Since Von Recklinghausen drew attention to the beautiful results to be obtained by the use of a weak solution of nitrate of silver, in the study of the finer distribution of vessels, and especially of the lymphatics, much discussion has taken place as to the trustworthiness of results obtained by this method, and there are some histologists of merit who maintain that the whole thing is false and delusive in its effect. The method consists in immersing perfectly fresh (warm from the animal) tissues in a ½ per cent. aqueous solution of nitrate of silver, leaving them there for from five to ten minutes, and then, after thorough washing, mounting in glycerine, and exposing to sunlight for half an hour, or two or three hours as the case may be. If desired, the tissue may be plunged while fresh into serum (preserved with just a trace of iodine), and after two hours' maceration therein may be smoothed with a small paint-brush, to remove superficial epithelium (as, for instance, in the centrum tendineum of the guinea-pig), and then placed in the silver solution. It is, however, preferred by Dr. Klein, of Vienna, to brush away the superficial epithelium of the thoracic surface, if the lymphatics of the centrum tendineum are to be examined, before removing the diaphragm from the body, whilst perfectly fresh and warm, warm water being used for this purpose, and a camel's-hair paint-brush: immediately after this "pencilling" the centrum tendineum is cut out and placed in the nitrate of silver solution. In the same way, in researches on the cornea the conjunctival epithelium may be removed in the living animal and the silver solution then applied. Dr. Klein, Professor Stricker's assistant, has found that the best results were to be obtained with the cornea, by removing the conjunctival epithelium, and rubbing the corneal surface well with lunar caustic, and removing the cornea in half an hour's time. This method proves admirable with frogs, and is probably applicable to other corneæ. The results of the nitrate-of-silver-staining as seen in the centrum tendineum, are that both thoracic and peritoneal epithelium (where not pencilled away) are stained, the outlines of the cells being brought out very distinctly. In addition to this the epithelium lining the vessels which run in the centrum is also brought out, and especially the curiously crenated epithelium of the fine lymphatics is demonstrated. Moreover, Recklinghausen pointed out that small branched cavities become delineated connected by their branches to one another, and opening into the lymphatics with the crenate epithelium. These lacunar channels, "the juice canals," are brought into view by the action of the silver in staining the substance of their walls but leaving the cavity free. A *successful* silver preparation of the centrum tendineum of a guinea-pig, for instance, shows these structures clearly, but it is not always possible to succeed. Besides the remarkable epithelium of the lymphatics and the juice-canals, a hexagonal pavement of enormous cells is demonstrated on the nerve sheaths, and an exceedingly fine cell pavement on the adventitia of the small veins in some beautiful silver preparations of the "centrum" made by Dr. Burdon-Sanderson. The results in the cornea are very remarkable, for the whole substance of that tissue in the frog is demonstrated to be filled with oblong radiating cavities, communicating with one another by their branches, and of so angular and regular a character as to give the appearance of a mosaic-work or some such elaborate device. These cavities correspond to the juice-canals in the centrum tendineum, it is by their means that the cornea is nourished, it is they which contain the large star-like cells or protoplasm-masses brought out by gold chloride, and it is along their branches that the divided portions of these cells wander so remarkably in the process of inflammation. And now we are told by some persons that these are artificial productions; that the juice-canals and the radiate lacunæ, and the crenate lymphatic epithelium too, are due to fortuitous coagulation caused by the silver, that they do not represent structures existing in life. There are two sets of objectionists, some who deny the whole thing, others who go so far as to deny the juice-canals and stellate lacunæ, but admit the epithelium. An exponent of the first order is a certain Dr. Robinski, who in a paper published in the excellent *Archives de Physiologie* of Brown-Séquard, pretends that the crenate epithelium which so many have seen in the finer lymph vessels, as well as the juice-canals, are due to imperfect retention of portions of the superficial epithelium, and gives a drawing intended to show this. The author of the paper has simply failed utterly and entirely in using the method, and the outrageous view which he advances does not require refutation. Everyone knows who has made silver preparations successfully that the thoracic, the lymph-canal, and the peritoneal epithelium may all be seen lying one over the other in parts of a preparation, and that the form of the second prevents its being mistaken for the first or third. Others maintain that the crenate lymph-epithelium is simply a form due to the silver, and not to cells at all. This is impossible, because the nucleus and cell-contents are sometimes clearly demonstrated, and moreover the same form of epithelium may be readily obtained from large lymph-sacs, of some of which it is characteristic. The juice-canals, if they appear doubtful at all when taken by themselves (which indeed they do not if a *good* preparation is examined), when seen in the light of the stellate lacunæ of the frog's cornea, are clearly understood, and must be fully admitted as normal living structures. There are, however, persons who object to these, among whom is so eminent a histologist as Professor Schweigger-Seidel, and also that accomplished micrographer, Dr. Ranvier, of Paris. They actually hold that these forms are *post-mortem* products, the effect of the action of nitrate of silver on the albumen and gelatine of the tissue. It is almost as strong a position to take up as that of certain schoolmen who maintained that we know nothing of the anatomy of living animals, since the air admitted on cutting them open, and the scissors, produced all the various viscera seen in a dissection. There are some strong facts which we have witnessed which render such a view untenable. In the first place, the stellate lacunæ of the frog's cornea are shown in a preparation in our possession, with the intermediate substance stained by *gold* and the lacunæ and cells unstained, in fact, exactly the same appearance as is produced with silver. Hence the stellate form of the lacunæ cannot be due to a specific action of the silver. Again, by silvering the living cornea whilst it is still part of the frog, the structure is brought out, and may be obtained of all degrees of intensity, the *form*, however, always remaining the same, and not varying with the amount of silver allowed to act on it, as might be expected had we to do with a precipitation-form, which should appear less completely developed when less silver is used. Moreover, the living cells were thus demonstrated lying in these stellate lacunæ, creeping up the radiating branches, drawing back again, and creeping along others, thus obtaining that curious direction to their movements which one always observes in studying them in the fresh, unstained, inflamed cornea. The necessity of admitting that the normal structure of the frog's cornea is brought out by the silver method, covers also the question as to the similar structures in the centrum tendineum. The "Saft-canalchen" must equally be admitted as having a living existence, and are not due to the imaginary precipitating action of nitrate of silver on some albuminoid, as Schweigger-Seidel would have us believe, but a parallel to which he cannot find. The chief difficulty with regard to the crenate epithelium of the lymph vessels in the centrum tendineum is to explain why we only see one layer of these cells; they should appear all round the wall of the vessel so as to produce a double layer as we look through it, but we only see one layer, the other not being stained. The action of light, the absorption of the silver, and other causes, may be hazarded as explanations of this; but it would be satisfactory to get a double layer clearly shown.

So far, then, from agreeing with Schweigger-Seidel and other objectors, that the silver method of Von Recklinghausen "gives no true indication of the structure of the cornea," nor of similar structures, and "should be abandoned,"* we believe that when carefully applied it furnishes most admirable results, difficult to attain in any other way at present in our hands, and is worthy of all confidence, and should be used as a means of investigation in other structures besides those to which it has been already applied.

E. R. L.

REPORT OF THE KEW COMMITTEE OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE FOR 1869-70

THE Committee of the Kew Observatory submit to the Council of the British Association the following statement of their proceedings during the past year:—

At the meeting of the General Committee at Exeter it was resolved that the existing relations between the Kew Committee and the British Association be referred to the Council to report thereon. In consequence of this resolution, the Kew Committee, on the 23rd November, 1869, prepared for the information of the Council a statement on the past and present condition of the Observatory, which was presented to the Council on the 11th December. In this statement it was shown that while the establishment at Kew Observatory received its main support from the British Association, and was under the control of that body, yet much of the apparatus in use at Kew was furnished from other sources. Thus, the Royal Society had from the Government-Grant Fund supplied the establishment with the apparatus for testing barometers, with that for testing sextants, with the dividing-machine for constructing standard thermometers, and also with the set of self-recording magnetographs at present in use, while from the Donation Fund they had furnished the photoheliograph and the Whitworth lathe and planing-machine. The Royal Society had likewise defrayed from the Donation Fund the expense of introducing gas into the Observatory, and of building a house for the verification of magnetic instruments, besides which they had borne from the Government-Grant Fund since 1863 the whole expense of working the Photoheliograph (including the purchase of a chronometer) and of reducing its results. The instruments used at Kew for determining the absolute magnetic elements are the property of Her Majesty's Government, and have been lent to the Kew Observatory by the Magnetic Office at Woolwich, under the direction of Sir E. Sabine, and many of those magnetic instruments with which Kew has been the means of furnishing scientific travellers have been derived from the same source. Of late Kew has become the central observatory of the Meteorological Committee, and a commodious workshop has been erected near the Observatory by that committee, since otherwise the main building would have been too small for the access of work consequent upon the arrangement entered into. The statement prepared by the Kew Committee contained likewise a summary of the scientific work done at the Observatory, as well as some interesting historical remarks connected with the origin of the establishment, drawn up by Sir C. Wheatstone, and in this shape it was submitted to the Council of the British Association. The Council then recommended "That the present existing relations between the Kew Observatory and the British Association be continued unaltered until the completion, in 1872, of the magnetical and solar decennial period, and that after that date all connection between them should cease." In consequence of this recommendation, the Kew Committee were led to contemplate the dissolution of the Kew establishment in 1872, and they became anxious to make such arrangements as might enable them to complete their scientific labours in a creditable manner before the time of the anticipated dissolution. The magnetic work in particular caused them anxiety; for the annual income of the establishment is insufficient to permit of that work being fully completed by the time of the annual meeting of the Association in 1872. Under these circumstances the chairman offered to supplement the deficiency. It will be seen by this report that the magnetical tabulations and reductions are now proceeding very fast. The recommendation of the Council was also a matter of anxiety to the superintendent, Mr. Stewart; and as the Professorship of Natural Philosophy at Owens College, Manchester, became vacant about this time, he applied for the appointment and was successful in obtaining it. This will render it necessary for Mr. Stewart to reside in Manchester, but the staff at the Observatory are such that Mr. Stewart will undertake by their aid to assist the committee in the superintendence of the work of the Observatory until 1872.

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* Berichte der Math. Phys. Classe der Kon. Sachs. Gesellsch. 1869. p. 352.

(A) WORK DONE BY KEW OBSERVATORY UNDER THE DIRECTION OF THE BRITISH ASSOCIATION.

1. *Magnetic Work.*—In the present state of magnetical science it would appear to be desirable to preserve as completely as possible the details of observations, so that future theorists may have a large and valuable source of information by which to test their speculations. The Committee are therefore desirous that by the autumn of 1872 a manuscript record should be completed, containing all the hourly tabulated values from the Kew Magnetographs arranged in monthly tables. This record should be carefully preserved along with the original photographic traces, in the archives of the Association. Pursuing the method indicated by Sir E. Sabine, and adopting the separating values finally determined by him, the Committee further propose to obtain monthly results indicating the following points for each of the three magnetic elements, distributed according to the hour of the day:—

1. Aggregate of disturbance tending to increase the numerical values.
2. Aggregate of disturbance tending to decrease the same.
3. Solar-diurnal range of the undisturbed observations.

They suggest that the monthly results embodying these facts should be published in detail. Finally, they propose to continue the discussion of the lunar-diurnal variations commenced by Sir E. Sabine, and carried on by him up to the end of the year 1864. In order to work this scheme with sufficient rapidity to complete it before the autumn of 1872, additional assistance has been procured, the expense of which has been defrayed by the chairman. Mr. Whipple, magnetical assistant, has displayed much zeal and ability in organising the work and in superintending its immediate execution. Already the hourly numerical values of the three magnetic elements have been obtained and tabulated in monthly forms from the commencement of the series in 1858 to the present date; and considerable progress has also been made in the next step of the reduction. A unifilar, formerly employed by Captain Haig, and of which the constants have been determined at the Observatory, has been lent to Lieut. Elagin, of the Russian navy, for use in the Japanese seas and elsewhere. A dip-circle, by Dover, has been verified and sent to Prof. Jelinek, of Vienna, and another, by the same maker, has been verified for Dr. A. B. Meyer, for use in the East Indies. This gentleman has likewise received magnetic instruction at the Observatory. A dip-circle by Adie, furnished with a deflecting cylinder apparatus, has been verified and despatched to Prof. Bolzani, of the University of Kasan. Three dipping-needles have likewise been constructed for Dr. Bergsma, of Batavia, and one for Mr. Chambers, of the Colaba Observatory, Bombay. A deflection-bar has been procured and verified for the Russian Central Observatory. A declinometer has been sent to the Lisbon Observatory, and a Fox's circle has been lent to Dr. Neumayer, after having been repaired by Adie. The instrument devised by Mr. Broun for the purpose of estimating the magnetic dip by means of soft iron, and constructed at the expense of the British Association in pursuance of a resolution of that body passed at the Oxford meeting, has been forwarded to that gentleman at his request. The usual monthly absolute determinations of the magnetic elements continue to be made by Mr. Whipple, magnetic assistant. A paper embodying the results of the absolute observations of dip and horizontal force, made at Kew from April 1863 to April 1869, has been communicated by the superintendent to the Royal Society, and published in the "Proceedings" of that body. The results obtained evidence the accuracy with which the monthly observations have been made by Mr. Whipple. The self-recording magnetographs are in constant operation as heretofore, also under his charge; and the photographic department connected with these instruments remains under the charge of Mr. Page.

2. *Meteorological work.*—The meteorological work of the Observatory continues in the charge of Mr. Baker. Since the Exeter meeting, 150 barometers have been verified, and thirty have been rejected; 1,160 thermometers and 103 hydrometers have likewise been verified. Nineteen standard thermometers have been constructed for Prof. Tait, and two for the Meteorological Office. The self-recording meteorological instruments now in work at Kew will be again mentioned in the second division of this report. These are in the charge of Mr. Baker, the photography being superintended by Mr. Page.

3. *Photoheliograph.*—The Kew heliograph, in charge of Mr. Warren De la Rue, continues to be worked in a satisfactory manner. During the past year 351 pictures have been taken on 237 days. It was considered desirable that six prints should be obtained from each of the negatives of the sun pictures taken at the Observatory during the whole time that the photoheliograph should remain at work, which will probably be from February 1862 to February 1872. In order to accomplish this, an outlay of 120*l.* spread over two years was found to be necessary, and this sum has been voted from the Donation Fund of the Royal Society. A large number of these prints has already been obtained, and it is proposed to present complete sets to the following institutions:—

The Royal Astronomical Society,
The Imperial Academy of Paris,
The Imperial Academy of St. Petersburg,
The Royal Society of Berlin,
The Smithsonian Institution, United States,

leaving one set for the Royal Society. A paper embodying the positions and areas of the sun-groups observed at Kew during the years 1864, 1865, and 1866, as well as fortnightly values of the spotted solar area from 1832 to 1868, has been communicated to the Royal Society by Messrs. Warren De La Rue, Stewart, and Loewy. This paper is in course of publication in the "Philosophical Transactions," and will shortly be distributed. A table exhibiting the number of sun-spots recorded at Kew during the year 1869, after the manner of Hofrath Schwabe, has been communicated to the Astronomical Society, and published in their monthly notices. M. Otto Struve, director of the Imperial Observatory at Pulkowa, visited England in the month of August last. He brought with him, for the Kew Observatory, some sun-pictures made at Wilna with the photoheliograph, which, it will be recollected was made some years ago, under the direction of Mr. De La Rue, by Mr. Dallmeyer. This instrument combines several important improvements on the original Kew model, the value of which is forcibly brought out in the superior definition of the Wilna sun-pictures. As, however, the series of the ten-yearly record at Kew was commenced with the instrument as originally constructed, it was not deemed desirable to alter it in any way until the series had been completed and reduced, and the corrections for optical distortion ascertained and applied. In the event of the sun-work being continued after 1872, it will be desirable to do so with a new and improved heliograph. M. O. Struve proposed to exchange the complete series of pictures obtained at Wilna for that made at Kew. He also stated that it is contemplated to erect a second heliograph at the Central Observatory at Pulkowa.

4. *Miscellaneous Work.*—A few experiments have been made on the rotation of a disc *in vacuo.* By an arrangement devised by Mr. Beckley, a very perfect carbonic-acid vacuum has been obtained, the residual pressure being 0·02 inch as indicated by a mercurial gauge with a contracted tube, but it was believed that the vacuum was even more perfect. A disc of paper and one of ebonite gave very sensible heat effects in such a vacuum, and it was hoped that the experiments might have been satisfactorily completed; but while they were in progress the pressure of the outer atmosphere shattered the receiver into a number of pieces, fortunately without any injury to the experimenters. Another receiver has now been made, and it is purposed in future to use it with a cover. A transit instrument has been lent to Mr. G. J. Symons, and one sextant has been verified.

(B) WORK DONE AT KEW AS THE CENTRAL OBSERVATORY OF THE METEOROLOGICAL COMMITTEE.

It is stated in the report for 1867 that the Meteorological Committee had appointed Mr. Balfour Stewart as their secretary, on the understanding that he should, with the concurrence of the Kew Committee, retain his office of Superintendent of the Kew Observatory. On the 8th October, 1869, Mr. Stewart resigned his appointment as Secretary to the Meteorological Committee and Director of their Central Observatory—a step which took effect on the 31st of March, 1870, and which was followed by a modification of the relation between the two committees. The Meteorological Committee, at their meeting on 12th November, 1869, resolved that they were prepared to make the following proposals to the Council of the British Association:—

I. That Kew be continued as one of the ordinary self-recording observatories, in which case the committee would be prepared to allot to it annually 250*l.*; or,

II. In addition to the foregoing work, that Kew be maintained as the central observatory for examination of records and tabulations from all the other observatories, in which case the committee will be prepared to allot a further annual sum of 400*l.* The Kew Committee, having been furnished with this resolution of the Meteorological Committee, resolved that it be recommended to the Council of the British Association that Kew be continued for the next two years as one of the ordinary self-recording observatories of the Meteorological Committee, that body allowing it annually 250*l.*; and that, in addition, it be maintained as the central observatory for the examination of the records and tabulations from all the other observatories, for the further sum of 400*l.* per annum. This arrangement was approved by the council; and it was thereupon resolved by the Kew Committee, that out of the 650*l.* received from the Meteorological Committee, 200*l.* be given to Mr. Stewart for superintending the meteorological work of the Observatory, this resolution to take effect after 31st March, 1870.

1. *Work done at Kew as one of the Observatories of the Meteorological Committee.*—The barograph, thermograph, and anemograph furnished by the Meteorological Committee are kept in constant operation. Mr. Baker is in charge of these instruments. From the first two instruments traces in duplicate are obtained, one set being sent to the Meteorological Office and one retained at Kew; as regards the anemograph, the original records are sent, while a copy by hand of these on tracing-paper is retained. The tabulations from the curves of the Kew instrument are made by Messrs. Baker, Page, and Foster.

2. *Verification of Records.*—The system of checks devised by the Kew Committee for testing the accuracy of the observations made at the different observatories continues to be followed, the only alteration being that the Kew staff, at the suggestion of the Meteorological Office, have undertaken to rule on the barograms and thermograms a set of zero lines, which are of great use in pantagraphic operations. Mr. Rigby continues to perform the main part of this work; Mr. Baker, Meteorological Assistant, having the general superintendence of the department.

3. *Occasional Assistance.*—The Meteorological Committee have availed themselves of the permission to have the occasional services of Mr. Beckley, Mechanical Assistant at Kew; and he has lately been visiting the various observatories of the Meteorological Committee. The self-recording rain-gauge mentioned in last report as having been devised by Mr. Beckley has been adopted by the Meteorological Committee, and instruments of this kind are at present being constructed for their various observatories. The staff at Kew continue to make occasional absolute hygrometrical observations by means of Regnault's instrument, with the view of testing the accuracy of the method of deducing the dew-point from the observations with the dry and wet bulb thermometers. Two erections have been made in the grounds adjoining the Observatory, and on one of these a large Robinson's anemometer is placed, while a small instrument of the same kind is placed on the other. By this means the indications of the large and those of the small-sized instrument may be compared with each other. The cost of this experiment has been defrayed by the Meteorological Committee.

J. P. GASSIOT, Chairman

Kew Observatory, Sept. 9, 1870

SCIENTIFIC SERIALS

THE November number of the *Geological Magazine* (No. 77) opens with an article by the editor, Mr. H. Woodward, on Fossil Crustacea from various formations. The species described and figured by the author are *Scyllaridia bellii* from the London clay of Sheppey, and *Palæga carteri* from the lower chalk. One of the most important papers in the number is on the coal-bearing rocks of Southern Chile by MM. Lebour and Mundle. The coal appears to be only a kind of lignite; it is found in beds of tertiary age. Mr. Lucy contributes a paper on the Post-pliocene Drift of Charnwood Forest; Mr. H. F. Hall, a note on the Glacial and Post-glacial deposits in the neighbourhood of Llandudno; Mr. L. C. Miall, a paper on the formation of swallow-holes in mountain limestone; and Mr. E. Wilson, a notice of some altered clay-beds and sections in Tideswell Dale, Derbyshire. The remainder of the number is occupied as usual by reviews, notices, and miscellaneous matter.

THE October number of the *American Naturalist* (vol. iv., No. 8) is chiefly devoted to Geological and Archæological subjects. It opens with an address on recent advances in Geology, delivered by Mr. J. H. Foster to the American Association for the Advancement of Science, and contains also a paper on the Primitive Vegetation of the Earth by Dr. J. W. Dawson, and a note on Indian Stone Implements by Mr. J. H. Gregory. The only other paper is a short note on bud-variations in the colour of the flowers in *Trillium* and *Wisteria.* This number also contains an interesting illustrated report of the nineteenth meeting of the American Association, including a valuable paper by Dr. A. S. Packard, jun., on the embryology of *Limulus polyphemus*; and another, by Prof. E. D. Cope, on the Structural Characteristics of the Cranium in the Lower Vertebrata.

Proceedings and Communications of the Essex Institute.—The first part of volume vi. was published in the spring of this year. Of four papers given in it, three relate to entomological subjects; these are descriptions (with figures) of numerous species of ants from Mexico, by Mr. Edward Norton; an excellent monograph of the Phalangea (Harvest Spiders) of the United States, by Dr. Horatio C. Wood, also illustrated; and an important notice of insects inhabiting salt water, by Dr. A.

S. Packard. The fourth paper is a continuation of Mr. A. E. Verrill's Synopsis of the Polyps and Corals collected by the American North Pacific Exploring Expedition in the years 1853 to 1856; this includes descriptions of a great many new species, and is illustrated with two plates. This part also contains the "Proceedings" of the Essex Institute for the year 1868.

The third part of the *Zeitschrift* of the German Geological Society, including its proceedings for the months of May, June, and July, opens with a long and important memoir, by M. F. J. Wurtenberger, on the Tertiary Formation of the Klettgau, a district situated on the northern border of the Swiss Molasse. The tertiary deposits, which are referred by the author to the Miocene and Oligocene periods, are of both freshwater and marine origin, and appear to be very complicated; fossils are scarce in them, except in certain localities, which have furnished the remains of plants and animals in considerable abundance. Prof. Ferdinand Roemer describes and figures a new fossil *Python* from the Island of Eubœa, under the name of *P. euboicus*. The specimen is contained in a slab of tertiary calcareous marl slate, and the portion preserved indicates that the snake would have been about nine-and-a-half feet long when living. M. H. Laspeyres communicates a monographic revision of the genus *Leaia* (T. R. Jones) belonging to the Phyllopodous Crustacea. He discusses the systematic position of the genus, which he places among the Linmadiadæ, and of which he describes and figures five species, one of them (*L. wettinensis*) as new, and two others as having been previously described as varieties of *L. Leidyi*. All the other papers relate to mineralogical subjects; they include a continuation of Prof. G. vom Rath's valuable geognostico-mineralogical Italian fragments; an analytical notice of the Palatinite of Norheim, by Prof A. Kenngott; and a notice of the occurrence of zircon in the hypersthenite of the Radauthal near Harzburg, by Prof. Gustav Rose.

THE *Journal of Botany* for November contains a continuation of Dr. Seemann's Revision of the Bignoniaceæ and several articles of special interest to British botanists. There is also a reprint of an interesting paper by Dr. Parry, of Washington, read at the recent meeting of the British Association, on the North American Desert Flora, between 32° and 42° north latitude. He calls particular attention to the contrast between the annual and perennial vegetation of desert tracts, the former being very evanescent and rapid in its growth; the latter either storing up a large amount of surplus nourishment in their thick tuberous or tap roots, or, in the case of trees and shrubs, possessing exposed stems and foliage of the most scant and starved character; spine-clad branches and green-backed stems are, in many places, made to supply the office of leaves, or, where these latter are present, they are often chiefly coated with resinous varnish, or clothed with tomentose hairs or scales, serving to check evaporation.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, Nov. 1.—Prof. Newton, F.R.S., V.P., in the chair. The Secretary read a Report on the additions that had been made to the Society's Menagerie during the months of June, July, August, and September. Amongst the more valuable acquisitions, particular attention was called to a collection of animals from Chili, purchased in the month of July last. Of twenty-two species obtained in this collection, no less than twelve were new to the Society's series, and some of these, such as Burmeister's Cariama (*Chunga burmeisteri*) and the small Coscoroba Swan (*Cygnus coscoroba*) were of special interest. Another valuable addition was a male of the Ethiopian Ant-bear (*Orycteropus Æthiopicus*) from Upper Nubia, purchased July 29. This animal had been placed in company with the Cape Ant-bear (*Orycteropus capensis*) acquired in June 1869, and afforded an opportunity for the comparison of the two species together.—A communication was read from Prof. W. Peters, containing an elaborate memoir on the structure of *Pectinator spekei*, a peculiar Rodent of Eastern Africa. The specimens on which Dr. Peter's memoir was based, had been obtained by Mr. William Jesse, during his travels as Zoologist in company with the Abyssinian Expedition.—A seventh letter on the Ornithology of Buenos Ayres, addressed to the Society by Mr. W. H. Hudson, was read.—Prof. Newton exhibited a chick of *Anarhynchus frontalis*, a rare wader from New Zealand, remarkable for possessing an asymmetrical bill.—Mr. C. Darwin, F.R.S., communicated a note on the habits of the Pampas Woodpecker (*Chrysoptilus campestris*), in reply to some observations on this subject made by Mr. W. H. Hudson, in one of his previous letters.—Six communications were read from Dr. J. E. Gray, F.R.S., on various points connected with the Natural History of the Testudinata. The first of these contained notes on three Tortoises living in the Society's Gardens, one of which was believed to be new to science, and was proposed to be called *Testudo chilensis*. The second contained descriptions of two new species of Indian Tortoises in the collection of Mr. T. C. Jerdon. The third related to the family *Dermatemydæ*, and embraced the description of a species of this group living in the Society's Gardens. The fourth contained notes on a West African River-Tortoise (*Cyclanosteus senegalensis*), also living in the Society's Gardens. The fifth contained notes on *Bartlettia*, a proposed new genus of freshwater Tortoises, belonging to the family *Peltocephalidæ*, and the sixth notes on the species of *Rhinoclemmys*, in the British Museum. A communication was read from Mr. W. Theobald, containing critical observations on a paper by Dr. J. E. Gray, on the families and genera of Tortoises, which had been recently published in the Society's "Proceedings." Amongst other remarks, Mr. Theobald stated the skull upon which Dr. Gray had established his *Testudo* (*Scapia*) *falconeri*, appeared to have originally formed part of one of the typical specimens of *Testudo phayrei*, in the Indian Museum, Calcutta.—A paper was read by Mr. A. G. Butler, containing a list of Diurnal Lepidoptera, collected by Mr. Spaight in Northern India.—A communication was read from the Rev. O. P. Cambridge, containing descriptions of some new genera and species of *Araneidea*.—A communication was read from Mr. W. Vincent Legge, containing notes on a species of *Prinia* from Ceylon. A communication was read from Surgeon Francis Day, containing a memoir on the Fishes of the Andaman Islands. Mr. Day's list embraced no less than 255 species, chiefly marine, which had been collected during the short space of a three weeks' visit to those Islands.

Linnean Society, Nov. 3.—Mr. G. Bentham, President, in the chair. A paper was read by Dr. Mansel Weale, "On the Fertilisation of certain Orchids and Asclepiads." He described several species of *Habenaria*, some of them new, from South Africa, and explained the process of fecundation, which he believes to be self fertilisation by the agency of insects. Some of the species, however, fructify comparatively seldom. At night some of the species emit a delicious fragrance, and these are chiefly visited by nocturnal moths. Dr. Weale also contributed a note on a solitary bee from South Africa.

Entomological Society, Nov. 7.—Mr. H. W. Bates, V.P., in the chair.—Mr. T. H. Briggs, of Lincoln's Inn, was elected a member.—Exhibitions of British *Lepidoptera* were made by Mr. Bond, Mr. Howard Vaughan, and Mr. McLachlan; of British *Coleoptera*, by Mr. F. Smith and Mr. Dunning. Mr. Edward Saunders exhibited *Xixuthrus heros*, a gigantic species of *Prionidæ*, from the Feejee Islands. Mr. Albert Muller exhibited galls of *Cynips agama*, and reniform spangles of *Cynips renum* both on the under-side of leaves of the oak.—Communications were read respecting swarms of *Chlorops lineata* which occurred in houses at Cambridge and elsewhere; and respecting the injury done to pear, plum, and cherry trees by the grubs of *Blennocampa Cerasi*.—The following papers were read: "On Butterflies from Basuto-land," by Mr. Roland Trimen; "On *Cerambycidæ* from the Amazons, by Mr. H. W. Bates: "On *Curculionidæ* from Australia," by Mr. F. P. Pascoe; "Notes on the *Eurytominæ*," by Mr. F. Walker.

Anthropological Society, Nov. 1.—Dr. Charnock, V.P. in the chair. Mr. Matthew Heslop, Mr. J. R. Mortimer Mr. Ankithum Venkata Nursing Row, Mr. James Hope, and Mr. Walter M. Parker, were elected members. Mr. William Storey, M.D., was elected local secretary for Malta; and Mr. Frank Wilson was elected local secretary for St. Paul's de Loanda, W. Africa. A verbal communication was made by Mr. Charlesworth, giving some details of his discovery, and the exhumation from a Tertiary formation in East Anglia, of a nearly perfect skeleton of one of the gigantic Bovine animals which the Roma legions met with when they first penetrated into the dense forest of Belgium and Gaul, and which are described by Cæsar unde the names of *Uri* and *Bisontes*. Dr. R. Charnock, V.P., F.S.A. read a paper on "The People of the Isle of Marken."

Ethnological Society, Nov. 8.—Dr. A. Campbell, Vice-president, in the chair. The following new members were announced:—The Earl of Antrim, Mr. W. Bragge, Mr. H. Rivett Carnac, and Mr. J. E. Lee.—A carved wooden implement found beneath 27 feet of guano in the Island of South Guanape, was exhibited and described by Mr. Josiah Harris. Col. A. Lane Fox exhibited a rudely worked stone implement from Borneo, being the first which has reached this country from that locality.—A note was read from Mr. C. R. Markham relative to the term *Aymara*. He maintained that the people inhabiting the basin of Lake Titicaca were known, at the time of the Spanish conquest, as Collas and not as Aymaras. An elaborate reply to this communication was made by Mr. David Forbes, F.R.S., who showed that the term Colla is merely a geographical expression.—A paper was then read "On the Kimmerian and Atlantean Races," by Mr. Hector McLean. The author described the Atlanteans as a dark race, forming a considerable ingredient in the population of Spain, southern and central France, South Wales, and the south and west of Ireland and Scotland; whilst the Kimmerians were a fair people, often of tall stature, with gaunt features, ruddy complexion, grey eyes, and red or yellow hair. These Kimmerians occupied the British Isles, and mixed with the Atlanteans previously to the arrival of the Scandinavians and Teutons. By means of the names of places, the author traced them in their migrations westwards from their original home between the Don and the Volga. They were known as the Galli or Galatæ, names signifying *white* people. The author concluded that the present population of England is more truly British or Kimmerian than the Welsh, and that our language is not the direct descendant of the language of the Saxon conquerors, but the descendant of both that of the conquerors and a kindred native language. The discussion was sustained by Mr J. F. Campbell (of Islay), Dr. O'Callaghan, Mr. Hyde Clarke, Mr. Pusey, and Dr. Nicholas.

Chemical Society, Nov. 3.—Prof. Williamson, F.R.S., in the chair. The following gentlemen were elected as fellows:—D. Howard, T. Muter, C. W. Siemens, F.R.S On opening this first meeting in the new session, the President alluded in a few earnest words to the loss the Society had recently sustained through the death of two of the most distinguished of its members. The following papers were read:—"On the Production of the Sulphates of the Alcohol Radicles from the Nitrites by the action of Sulphurous Acid," by E. T. Chapman. When sulphurous acid gas is passed into nitrite of amyl it is rapidly absorbed. The nitrite changes in colour from yellow to green, from green to blue; it then begins to effervesce, and at the same time becomes hot and boils violently. Nitric oxide is evolved in abundance, and a yellow liquid product remains. This liquid cannot be distilled without decomposition. It contains no nitrogen. When an attempt is made to distil it along with water, the oily liquid found in the distillate consists principally of amylic alcohol. When the liquid is distilled *per se* it blackens, gives off sulphurous acid, and yields a complex distillate which contains, amongst other things, valerianate of amyl. This circumstance stood in the way of any attempt to obtain an insight into the nature of this reaction. Mr. Chapman resolved, therefore to attack the question quantitatively. With this object he determined the nature and amount of the gas evolved by the action of excess of sulphurous acid on a known weight of the nitrite. The result proved that the gas evolved consisted of pure nitric oxide, and that the whole of the nitrogen present in the nitrate escaped in this form. The amount of sulphurous acid necessary to decompose a given quantity of nitrate was also determined by observing the quantity of sulphurous acid which disappeared when an excess of sulphurous acid acted on a known quantity of nitrite. It was then ascertained with sufficient accuracy that one atom of sulphurous acid acted upon two atoms of nitrite of amyl and liberated two atoms of nitric oxide. Lastly, the alteration of weight which nitrite of amyl undergoes by the reaction was determined. The result confirmed the supposition that the reaction consisted in the replacement of two atoms of nitric oxide by one of sulphurous acid.

$$\left.\begin{matrix}C_5H_{11}O.NO\\C_5H_{11}O.NO\end{matrix}\right\}SO_2 = \left.\begin{matrix}C_5H_{11}\\C_5H_{11}\end{matrix}\right\}SO_4 + 2\,NO$$

The resulting liquid compound had therefore the composition of neutral sulphate of amyl. It readily breaks up into amylic alcohol and sulphuric acid by boiling with water and by long standing even with cold water; treated with strong hydriodic acid it yields sulphuretted hydrogen, water, iodine, and amylic iodide; potassic bichromate and sulphuric acid cause it to yield valerianic acid. It is to be observed that it is necessary gently to warm the retort in which the nitrite is exposed to the action of a stream of dry sulphurous acid; if this is not done, the SO_2 is absorbed for some time without any reaction occurring, but when the reaction does start it is with almost explosive violence; whereas, if gentle heating has been applied from the beginning, the reaction starts at once and goes on regularly. It is also desirable to pass through the apparatus carbonic acid or hydrogen before the SO_2 has been passed into the nitrite, and to do this also afterwards, for the purpose of excluding air, the oxygen of which would unite with the NO, and the SO_2 would then be expelled. Sulphurous acid and butylic nitrite react upon one another in a manner analogous to that of SO_2 or amylic nitrite, but the resulting product is even more unstable. Sulphurous acid and nitrite of ethyl do not readily act upon each other, at least not at the common temperature. Mr. Chapman then proceeded to the theoretical considerations which are suggested by the above facts. Are these compounds, properly speaking, sulphates of alcohol radicles, or only bodies isomeric with them? The reaction of the amyl compound with water is very different from that of sulphate of ethyl under similar circumstances; it does not, when boiled with water, form an acid analogous to isethionic acid, but splits up into sulphuric acid and amylic alcohol. This would suggest a different linking of the molecules; most probably in the common amylic sulphate the two organic radicles are linked to oxygen directly, and by oxygen to sulphur; in the amyl compound obtained from the nitrite, one of the radicles is attached directly to sulphur and the other indirectly through the oxygen. Mr. Chapman then described the apparatus by which the nature of the reaction of sulphurous acid on amylic nitrite has been determined. The evolved nitric oxide was transformed into nitric acid, and this treated with barytic carbonate. In the discussion following this paper, Dr. Debus and Mr. Harcourt expressed their apprehension that along with the barytic nitrate also some barytic nitrite may have formed. Mr. Chapman replied that he had taken great care to ensure the complete transformation into nitric acid; that to this end he had passed great quantities of oxygen into the collecting cylinder, let the mixture stand for twenty-four hours over the water in the cylinder, and lastly applied gentle heat to it. As for the possibility of the water acting as a base and reducing the formed nitrous acid, there was the counteracting presence of SO_2, which had passed unabsorbed through the amylic nitrite into the collecting cylinder, and part of which must have been oxydised to sulphuric acid. Regarding Mr. Chapman's theoretical views of the union of SO_3 and $\left.\begin{matrix}C_2H_5\\C_2H_5\end{matrix}\right\}O$, the President thought that it is the same as that of SO_3 with H_2O, that the two C_2H_5 are linked to SO_3 by the oxygen, just like the two H are in hydric sulphate.—Mr. Vacher read a paper by Mr. Elliott "On the determination of Sulphur in cast-iron." This determination consists in the liberation of the sulphur in the form of sulphuretted hydrogen, absorption of the latter by solution of soda and estimation of the H_2S in the acidified soda solution by means of a standard solution of iodine. In the course of his experiments, Mr. Elliott noticed that a portion of the iron was not attacked by HCl, and that this insoluble residue yielded on fusing with pure caustic soda (prepared from sodium) sodic sulphate. From this Mr. Elliott draws the conclusion that cast-iron contains sulphuric acid as well as sulphur. In somewhat similar manner the presence of phosphoric acid in cast-iron was ascertained. Dr. Debus doubted the probability of the occurrence of sulphuric acid in iron which had once been in a molten state. Mr. Church remarked that he had noticed in dissolving natural pyrites in HCl, that very often an insoluble sulphide and phosphide remained. At the same time, he considered the fusion of those residues with caustic alkalis as a sure means of converting the sulphur and the phosphorus into the respective acids, the alkalis being under such circumstances very powerful oxydising agents. Mr. Harcourt expressed his concurrence with Mr. Church's remarks about the oxydising action in this case of caustic soda. Mr. Vacher did not wish to support Mr. Elliott's view about the presence of sulphuric acid in cast-iron, but there was the fact of one portion of the sulphur not being liberated by HCl, and this forces to the conclusion that cast-iron contains sulphur in another form than in that of the ordinary sulphide.—E. A. Letts, "On the Composition of Hyposulphites." Though the salts of the hyposulphurous acid have been comparatively well studied, much difference exists as to their chemical constitution. Rose states that an atom of water is retained by all the hyposulphites, and is essential to their composition. On the other hand, Pope and other chemists maintain that most, if not all the hyposulphites, can be obtained in the anhydrous condition. Mr. Letts undertook some experiments to ascertain whether hydrogen was to be regarded as an essential constituent of the hyposulphites. The salts submitted to investigation were those of sodium, barium, lead, strontium, magnesium, nickel, and cobalt. The sodium salt lost all its water by drying *in vacuo* over sulphuric acid The barium salt gives off its water by drying at 100° C. The plumbic hyposulphite, prepared by adding a solution of lead acetate to one of sodic hyposulphite, crystallises out from the liquid without any water at all. This, then, is a case demonstrating without doubt that hydrogen is *not* needed to complete the chemical constitution of a hyposulphite. The strontium salt retained, even after drying at 200° C., half a molecule of water. The magnesium-salt crystallising with six atoms of water, loses three of them at 100° C.; but an attempt to expel more water causes the decomposition of this salt. The nickel hyposulphite, the crystals of which have also six H_2O, do not part with any of it without undergoing decomposition. The cobaltic hyposulphite is even more unstable than the former salt. The President observed that the contents of the last paper set at rest the doubts which hitherto existed as to the constitution of the hyposulphites, and showed that the water which they ordinarily contain is not essential to their chemical existence.

BERLIN

German Chemical Society, October 10.—President, Prof. Rammelsberg. The President reported on the means employed by the society to promote disinfection of the battle-fields and hospitals. A great number of German and foreign chemical manufacturers had kindly responded to a circular requesting gifts of chemical disinfectants. From England, Messrs. Berger, Spence, & Co., Ballman and Condy, F. C. Calvert & Co, Crowther and Graeson, L. Demuth & Co., C. Kurtz and Sons, C. Lowe and Co., G. Lunge, George Miller and Co., T. Storey and Co., had sent various disinfecting agents. A table detailing the right use of disinfectants had been published and sent to the proper medical authorities and members of the society who have undertaken the superintendence of disinfection in various towns. The seat of war has been visited for the same purpose by six members of the society. The following papers were then read:—A. W. Hofmann: "The history of Nitriles," a reply to M. Mendelejeff, who had published certain views, not knowing the same to have been promulgated before by the author.—T. Thomson: "On the double chloride of Beryllium and Platinum." This salt is isomorphous with the corresponding calcium salt, and not with the magnesium salts, as had been supposed. The same author on "The supposed Connection of the law of Avogadro with the Mechanical Theory of Heat." A mathematical deduction lately published by Naumann, contains, according to Thomson, an error invalidating its argument.—C. Rammelsberg, in a lecture on the relation of mineralogy and chemistry, urges upon mineralogists to apply the modern formulæ.—A. Bauer described an alloy of lead and platinum of the formula Pt Pb.

October 24.—H. Wichelhaus described β Nitronaphtol. This compound, which cannot be produced in the ordinary way, may be obtained by treating alcoholic solution of β naphthol with nitric acid, as was lately recommended by Bolley. The same chemist has obtained Triacetamide by applying a similar method to that employed by Kekulé in the preparation of diacetamide. The latter is obtained by acting on acetonitrile with acetic acid; the former by replacing the acid by acetic anhydride: $CH_3 CN + (C_2 H_3 O)_2 O = (C H_3 C O)_3 N$. The three amides have nearly the same physical properties. The diamide, according to Kekulé, forms salts. The triamide is an anhydride converted by $P_2 O_5$ into acetonitrile and acetic anhydride.—Petersen on Nitrochlorophenoles By continuing the researches of Baer-Predari, the author has produced five of the six possible isomeric bodies of the above constitution.—C. Rammelsberg, on Yttrocerite, determines the formula of the mineral $Ce F_2$, $2Y F_2$, $9 Ca F_2$, 3 aq. Berzelius had found 1 Y 2 Ce. Both Y and Ce represent rather groups of metals than well defined single elements. In a discussion following this communication, G. Rose called attention to the isomorphism of yttrocerite and fluorspar, thinking that most likely the water found in the former (2½ per cent.) does not form a constituent part of the mineral.

BOOKS RECEIVED

ENGLISH.—The Science of Building: E. W Tarn (Lockwoods)—Elementary Treatise of Natural Philosophy: A P. Deschanel (Blackie and Sons).—Text books of Science, Metals: C L Bloxam (Longmans)—Virgil's Bucolics in English Verse: R M Millington (Longmans)—Osteology of the Mammalia Prof Flower (Macmillan and Co)—The Academy, vol 1

AMERICAN—Kirklos on Experimental Investigations into the Relationship of certain Lines, pt. 1. J. Harris (J. Lovell, Montreal).

FOREIGN—(Through Williams and Norgate)—Biologische Studien: E. Haeckel.—Untersuchungen uber den Bau des knochernen Vogelkopfes: Dr. H Magnus—Der Schadel des Maskenschweines: Dr C J. C. Lucae—Beitrage zur vergleichenden Neurologie der Wirbelthiere. N. von Miklucho-Maclay.

PAMPHLETS RECEIVED

Is a Ship-canal practicable? by S T Abert (Cincinnati)—Annual Report of the Director of the Cincinnati Observatory—Fossil Sponge Spicules in the Greensand of Haldon and Blackdown: E Parfitt—Crustacea Podothalmata, and the Histology of their Shells. E Parfitt—The Improvement of English Orthography: D P Fry—The Rainfall of the St Mary Church-road, Torquay: W Pengelly—The Rainfall in Devonshire, 1869: W Pengelly—The supposed Influence of the Moon on the Rainfall: W Pengelly—Notes on Vessels made of Bovey Lignite and of Kimmeridge Coal: W Pengelly—The Ash hole and Bent-bone Caves at Brixham: W Pengelly—The Literature of the Caverns near Yealhampton: W. Pengelly.—Geography in relation to Physical Science: W. Hughes.

DIARY

THURSDAY, NOVEMBER 17.

LONDON INSTITUTION, at 7 30.—Acoustics of the Orchestra, Wind Instruments: Dr. W. H Stone.

CHEMICAL SOCIETY, at 8—Mineralogical Notices: Prof. N. Story Maskelyne and Dr Walter Flight

LINNEAN SOCIETY, at 8—On the *Passifloreæ*: Dr. M T. Masters.—On the White-beaked Bottle-nose. Dr. James Murie

SOCIETY OF ANTIQUARIES, at 8 30—Egyptian Antiquities, with remarks by Dr. Birch. Mr W. R. Cooper.

SUNDAY, NOVEMBER 20.

SUNDAY LECTURE SOCIETY, at 3 30—On the Antiquity of Man: Dr. Cobbold.

MONDAY, NOVEMBER 21.

LONDON INSTITUTION, at 4—Chemical Action: Prof. Odling.

TUESDAY, NOVEMBER 22.

ETHNOLOGICAL SOCIETY, at 8.—On the Concord, the Origin of Pronouns, and the Formation of Classes or Genders of Nouns: Dr W. H. J. Bleek.—On the Position of the Australian Languages: Dr. W H. J Bleek.

WEDNESDAY, NOVEMBER 23

GEOLOGICAL SOCIETY, at 8—On some Points in South-African Geology: Mr G. W. Stow—Note on some Reptilian Fossils from Gozzo: Mr J W. Hulke.—On the Discovery of a Bone-bed in the Lowest of the Lynton Grey Beds, North Devon: Dr. F. Royston Fairbank.

SOUTH KENSINGTON MUSEUM, at 2 30—On the Clavecin and the Pianoforte Ernst Pauer

ROYAL SOCIETY OF LITERATURE, at 8 30.—On the three Seals of Edward the Confessor. Walter De Gray Birch.

THURSDAY, NOVEMBER 24.

LONDON INSTITUTION, at 7 30.—On the Precious Metals and their Distribution: Prof Morris

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THURSDAY, NOVEMBER 24, 1870

THE CLAIMS OF SCIENCE

THE Statistical Society, which held its first meeting for the session 1870-71 on Tuesday, the 15th inst., had the claims of Science brought before it in a paper read to it by Dr. Guy, one of its vice-presidents. The paper was written with the practical aim of commending and furthering a scheme which the Statistical Society has set on foot, and in which it has invited the Institute of Actuaries, the Social Science Association, and several other scientific bodies, to participate. The object these societies have in view is to provide a common home in which they shall enjoy the advantage of fixity of tenure and the sense of permanence, with suitable and economical arrangements for carrying on their scientific work. They wish to provide for themselves a common theatre, convenient offices, spacious libraries, and—in the case of societies requiring moderate museum accommodation—museums. All this the societies aim at accomplishing within moderate limits and at a reasonable cost; for they feel very naturally that when the Government has made provision at Burlington House for six leading societies, and other institutions have provided their own isolated accommodation, there no longer remains any place or pretence for a large and comprehensive scientific centre. The building contemplated by the associated societies would have all the unity of character now practicable, if its principal tenants were to consist of societies having a common aim. Such an aim is to be found in the culture of the sciences now known as "social," or societies which make man himself, as the unit of society, the object of their study.

If we define Science as "knowledge in its most definite, condensed, and exquisite form, dealing with worthy objects, and applied to worthy uses," it may be stated, as a truth worthy of general acceptance, that every branch of knowledge that is, by common consent, stamped with the word *science*, aims at some useful and worthy object, studies a certain defined order of things, which it identifies by accurate descriptions and exact definitions, by expressive words and phrases; which it arranges in lucid order, under classes and sub-classes; on which it brings to bear the most delicate instruments and most refined methods of analysis; to which it applies, as far as practicable, the rules of logic and the figures of arithmetic; crowning the entire edifice, if it proves equal to the burthen, with some comprehensive numerical theory.

Passing from this general view of science, and coming to that branch of it now known as *social*, we may trace the seeds of it back to the parish registers of 1538 and the enactment of Henry VIII., respecting leases for three lives, or twenty-one years, through the London Bills of Mortality and the commentaries of Grount and Petty, through the early attempts of Halley to construct a table of mortality from the death registers of Breslau, through the prison inspections of John Howard, up to the establishment of the Statistical Society in 1834, and the foundation of the Social Science Association in 1857; the Statistical Society having, as is well known, been set on foot with the object of collecting "facts calculated to illustrate the condition and prospects of society," which was what Gottfried Ochenwall, of Göttingen, who coined the word *Statistik*, really meant by that word. The Social Science Association, therefore, was a second development and a modified culture of that branch or division of human knowledge—that science of States—to which had been previously given the name of Statistics. The two societies have a common aim—the improvement of man's condition, physical, intellectual, and moral, through the patient heaping up, intelligent sorting, and critical examination of the elements of a knowledge which, properly applied, is power indeed.

This social science, of which the *Institute of Actuaries* cultivates a very important section, differs from most other sciences chiefly in this, that its units are of variable magnitude, and that its truths and principles, gathered from large assemblages of such units, admit of application only to like collections of facts, not to the individual units themselves. The actuary has the function of first establishing truths of this order, and then applying them; the statist must look to the statesman to carry into effect the practical works of justice and benevolence. The association of the Statistical Society and Institute of Actuaries with the Social Science Association and Law Amendment Society is, therefore, one pointed out by the nature of things; and we may hope to see them some day working side by side under one roof with one common aim—"*the improvement of man's estate.*" But this principle of association admits of being carried much farther, so as at length to embrace in one group, under one roof, all the societies or associations that make man himself, as a physical and moral unit, the object of their study.

The section of Dr. Guy's paper that treated of *scientific societies and associations*, consisted of an historical retrospect of the rise and origin of most of the societies now existing, finishing with some details of the number and composition of the Statistical Society's members, and of the number of members of the allied societies. Into these details we shall not enter, but we shall restrict ourselves, in what we have yet to say, to the views expressed by Dr. Guy on the subject of the claims of science to public recognition and support. After pointing out that science has found favour, encouragement, and support under every form of Government, that kings have acknowledged that it adds lustre even to thrones, and republics have deemed it quite consistent with their sterner virtue to hold out to it the hand of fellowship—a recent notable example of which has been afforded in the pecuniary assistance and means of transport afforded by the United States to two parties of its citizens bent upon voyages to Spain and Sicily to view the total eclipse on the 22nd of December, an example which our Government has at last, however, willingly consented to follow,—the paper proceeded to do justice to our own Government. The refusal, followed by a slow repentance, was quite an exception to the rule in England. It could only have occurred during one of those cold fits of economy to which the nation is subject at the close of some feverish paroxysm of prodigal expenditure; or it may have been an outbreak of the hypochondriac fancy that they are on the brink of ruin, which is apt to seize the richest nations no less than the wealthiest individuals. It is not difficult to show that Science, in the sense of knowledge of the more precise, exact, and exquisite order, has claims to public recognition and support on the ground of benefits conferred on the nation in the shape both of honour and profit; that it shares with righteousness the prerogative of exalting a nation (for the love of truth, which causes men to seek after knowledge and the patient industry and self-denial which are the first conditions of the search, are among the manly virtues that give strength and solidity to a people); that it must be preferred before learning, as being more practical, and coming into more direct contact with the realities of life; before art, as less apt to be turned to unworthy uses, more sure not to become an agent of effeminacy and luxury.

Of the good gifts which Science showers upon mankind, we may find grand and convincing examples in the works of the hygienic heroes of the last century—Sir George Baker and his masterly demonstration of the cause of the Devonshire colic, Captain Cook and his successful prevention of scurvy, John Howard and his prison work, ending in the destruction of the Jail Fever, and Jenner, with his discovery of vaccination. We fully sympathise with the concluding words of this part of our author's paper:—"By what figures of arithmetic shall I attempt to measure the greatness of these four gifts of science, freely bestowed upon us, and upon all men everywhere, in the short space of a single generation? I believe it to be no exaggeration to affirm that the great war of the French Revolution was brought to a successful issue as much through the lives thus saved as by the valour of our soldiers and sailors. Such have been the triumphs, such the precious gifts, of this one science of hygiène." Other illustrations of the same class, that is to say, showing direct profit to the nation, may be drawn from the Science of Chemistry, of which the whole history, from first to last, is one unbroken series of purely scientific discoveries made for love of truth, without thought or hope of reward, but, sooner or later, turning to profit in the hands of our manufacturers.

We might cite examples from the discoveries of Davy and Daniell, and the arts of electrotyping and photography, discoveries appealing to universal experience of the manifold obligations under which science and scientific men have laid mankind for all the arts which make our civilised existence to differ from the rude life of the savage. The Penny Post, with its world-wide benefits, is the result of a scientific demonstration belonging to the methods and domain of Social Science.

We conclude with the following statement of the special claims of the Statistical Society and its associates in the culture of Social Science:—"The scientific labours of our members, inspired by a mere love of truth, looking to no pecuniary reward, and bearing directly on the very questions which come under discussion in the Legislature, are in many cases a direct saving of expense to the nation. An important (perhaps a very costly) return is made to Parliament. It abounds in tables and columns of figures. The work of analysis, which must be undertaken if the return is not to become so much waste paper, if Parliament and the public are to profit by the expense incurred—this work of analysis is done by some member of the society seized with a wholesome curiosity to know the truth. He bestows upon it time, and thought, and the skill acquired by practice; he submits his work to the criticism of the Society, his paper is published in its *Journal*, at its proper cost; and thus the public and the Government save money and become possessed of wholesome and fruitful truths." These are claims which, we think, the Government will feel bound to recognise, and we wish the cultivators of the Social Societies every success when they come to represent them in the proper quarter.

THE SOURCES OF PHOSPHATIC MANURES

"PRACTICE with Science" is the title of a volume of essays (the second of a series), issuing from the Royal Agricultural College, Cirencester, and containing contributions from the members of the staff of that institution. Amongst other papers is an interesting account by Prof. Thiselton Dyer of the geological distribution of Tricalcic Phosphate; that is to say, a sketch of the chief sources of mineral phosphate of lime, whether as apatite, osteolite, phosphatite, coprolite, or guano. Mr. Dyer points out the abundance of phosphate of lime in igneous rocks, but hesitates about tracing its origin in such beds either to direct chemical combination, or to the inclusion of organically-formed phosphate in the rocks in question. He does not, in short, discuss the possibility of the combination of phosphoric acid and lime in the primæval state of the globe without the intervention of life, which one distinguished geologist at least denies. Mr. Dyer traces the occurrence of tricalcic phosphate in the various sedimentary deposits with great care, having obviously taken much trouble to render his statement an exhaustive one. He considers the many structureless masses of phosphatic deposits which occur "as residuary evidence of formerly existing life, of which they are to some extent the measure," as graphite is in other cases. A greater influence in the production of these masses is attributed to animal than to vegetal life, though marine plants are stated to be especially rich in phosphate of lime, and have undoubtedly played their part in its introduction into sedimentary strata. Mr. Dyer mentions that the recent Brachiopod Lingula has 86 per cent. of phosphate of lime in the mineral ingredients of its shell; and the occurrence of large quantities of phosphate of lime in the great Laurentian and Silurian formations is noticed by him in detail, as well as its occurrence in Devonian and Carboniferous limestones. In emerging to the group of mesozoic strata, we leave behind almost entirely those veins and beds of "phosphate" which occur in the older and more changed rocks, where the segregation of the phosphate of lime has been more completely effected, owing to the greater age of the beds. In mesozoic and tertiary strata we find those nodules which have so erroneously been confused with "coprolites"—the droppings of fish, which are not unfrequently preserved in the fine sediment of the Liassic and the Rhætic beds of the chalk—though beds of flaggy phosphate also occur in some deposits of this age.

Mr. Dyer accepts the history of the origin of these nodules which I have advocated (Geol. Magazine, vol. v.), in describing those which occur below the Suffolk Crags. Clay has a remarkable power of detaching phosphate of lime from its solution in carbonated water; and the phosphatic nodules are bits of clay which have become imbedded with great quantities of bones, and in some cases, very probably—as suggested by Mr. Seeley, of Cambridge, with regard to the Cambridge nodules—with sea-weed too; whence, by the intervention of gas-charged water, they have extracted the phosphate: hence all beds of phosphatic nodules occur near to argillaceous strata of special character. Much of this process, no doubt, went on whilst the bones and clay-lumps lay on the ancient shores, and were daily washed and infiltrated by the sea-water, or lay entirely submerged in masses: but Mr. Dyer thinks that the process of transference would continue after the beds had been left high and dry, and may be now going on; though I think it is clear that the phosphate of lime in the nodules came from bones which have been destroyed and lost in the process, having been very different in mineral condition to the fragments which now remain amongst the nodules of these valuable "bone-beds." Mr. Dyer notices Rhætic, Jurassic, Cretaceous, and Tertiary accumulations of phosphatic nodules. There is one which has not been hitherto recorded, and which is not alluded to in this paper, but is interesting, and in a well-known locality; it occurs in the Wealden series, near Brook, in the Isle of Wight, and is in parts five or six feet thick. The nodules are light-coloured, and aggregated into masses so as to form a solid bed, and not a pebbly conglomerate, as is usual.

The distribution and origin of Guano is briefly given. True guano is simply the dung of sea-fowl, and can only accumulate in rainless districts. Guano rock is the result of the action of water on this matter and subjacent calcareous coral rocks; the celebrated Sombrerite is of this nature. It is very possible that much of the palæozoic phosphatic rock may have been produced in this way, in those beds, at any rate, which we may believe to have been formed subsequently to the evolution of terrestrial vertebrate forms of life.

The other essays in this volume treat of more strictly agricultural subjects, and are accordingly of more limited interest.

E. RAY LANKESTER

SCIENTIFIC YEAR BOOKS

The Year-Book of Facts in Science and Art. By John Timbs. Pp. 288. (London: Lockwood and Co., 1870.)

Annual of Scientific Discovery, or Year-Book of Facts in Science and Art for 1870. Edited by John Trowbridge, aided by Samuel Kneeland, M.D., and W. R. Nichols. Pp. xxii. and 354. (Boston: Gould and Lincoln; London: Trübner and Co., 1870.)

L'Année Scientifique et Industrielle. Par Louis Figuier. Quatorzième Année (1869), pp. 606. (Paris: Hachette; London: Williams and Norgate, 1870.)

Causeries Scientifiques. Neuvième Année (1869). Par Henri de Parville. Pp. 363. (Paris: Rothschild; London: Williams and Norgate, 1870.)

Annuaire Scientifique. Par P. P. Dehérain. Neuvième Année (1869), pp. 387. (Paris: Masson; London: Williams and Norgate, 1870.)

Jahrbuch der Erfindungen. Herausgegeben von H. Hirzel, und H. Greschel. Fünfter Jahrgang, pp. 416. Leipzig: Quant; London: Williams and Norgate, 1869.)

GROUPING these volumes according to the languages in which they are written, we may dismiss the first two with a very few remarks. Mr. Timbs literally gives his readers nothing whatever but a collection of cuttings from the most miscellaneous sources, including the *Pall Mall Gazette, Times, Spectator, Illustrated News, Liverpool Albion,* &c.; while Mr. Trowbridge and his coadjutors (who have a respectable scientific status) present us with a much more perfect, although still an incomplete, picture of the leading discoveries of the year. The introductory notes by the Editor constitute the most valuable portion of the American book, which treats of the progress of science, under the respective heads of (1) Mechanics and Useful Arts, occupying 135 pages; (2) Natural Philosophy, to which 64 pages are devoted; (3) Chemistry, (4) Geology, (5) Biology, (6) Astronomy and Meteorology, and (7) Geography and Antiquities. This volume, like that of Mr. Timbs, exhibits a too free use of the scissors, but the extracts are almost invariably taken from periodicals of good scientific repute.

If our readers require any specific evidence of the English editor's unfitness for his office, we would refer them to the article headed "Singular Plant," in p. 200 of the "Year-Book of Facts." It is obvious from the most cursory perusal of the history of this "singular plant," that it is merely a fine specimen of coral, and the absurdity of the story was exposed in a number of the *Gardener's Chronicle* subsequent to that in which it originally appeared. The correction was, however, overlooked by the learned editor.

The French Year-Books differ materially from one another in their modes of arrangement. In this respect we prefer that of M. Figuier to the others. It includes a large number of subjects arranged in the following order:—Astronomy, Mechanics, Physics, Meteorology, Chemistry, Civil Engineering, Voyages and Travels, Natural History, Public Health, Physiology and Medicine, Agriculture, and the Industrial Arts.

The science-gossips of M. de Parville are a collection of papers such as a physicist might contribute to a popular journal. The matter in this volume is more digested, and is in a far less crude and fragmentary state than in the other books we have noticed, and the individual facts are dovetailed together so as to make the style agreeable and the reading continuous. It includes in its range—Astronomy, Physics, Mechanics, Chemistry, Physiology and Medicine, Natural History, Engineering, and unplaceable topics.

In some respects M. Dehérain's volume is the best of the three. Although less comprehensive in its scope than that of M. Figuier, or even than that of De Parville, it is more perfect so far as it goes. It is divided into two parts, treating respectively of the pure and of the applied sciences. Under the pure sciences he places Astronomy, Physics, Chemistry, Meteorology, Botany, Physiology, and Anthropology; while the applied sciences include Civil Engineering, Applied Chemistry, Medicine, and *Exploitation des Animaux,* for which we have no exact English equivalent. It is, we think, doubtful whether this sub-division of the sciences will bear criticism, but it is needless at present to discuss that subject. Instead of flying from flower to flower like the busy bee of our early days, M. Dehérain confines himself to one or two of the most important subjects in each department, and these he treats with far more fulness than the preceding writers. For example, under Chemistry we have an article on Explosive Compounds, containing a review of the works of Nobel, Abel, Berthelot, and Saint-Claire Deville, by the editor; and another, by M. Laudrin, on the Influence of Pressure on Chemical Phenomena, being a review of the works of MM. Berthelot and Cailletet; and these, with a biographical sketch of Professor Graham, complete the section on this science. Similarly, the only information that he gives us on Botany is included in an article by M. Vignes on the Geographical Distribution of Vegetable Species, based on the works of Sir Dalton Hooker (*sic*), and an article by himself on the Maturation of the Cereals.

Amongst the subjects most fully considered by MM. Figuier and De Parville are the Suez Canal; the cause of the explosion in the Place de Sorbonne, and the recent history of picrates and other explosive compounds; the discussion regarding the modification of the metre; the Newton-Pascal forgeries; chloral and its action; and the deleterious effects of absinthe. The question whether Coralline (one of the coal-tar colours) is or is not deleterious as a dye is fully discussed in the volumes of MM. Figuier and Dehérain.

Although it is less extensive in its range of subjects, we are inclined to prefer Hirzel and Gretschel's "Year-book of Inventions" to any of the preceding volumes. The departments of science which it includes are Astronomy, Physics and Meteorology, Mechanics and Mechanical Technology, and Chemistry and Chemical Technology. With the view of briefly explaining the arrangement adopted by the editors, we may state that under "Physics and Meteorology" are included molecular physics, acoustics, optics, the theory of heat, and electricity and magnetism. Under the heading "Mechanics and Mechanical Technology," only five subjects are considered, but they are all treated in considerable detail. They are—dynanometers, mechanism applied to locomotion (including the mountain railway system of Marsh and Fell, the road engines of Larmanjat and Thompson, velocipedes, and Kettendampff-schifffahrt or chain-steam-navigation), sewing and knitting machines, to which more than thirty pages are devoted, and new pumps constructed on various principles. This work is executed in a higher scientific spirit than any of the preceding volumes, excepting, perhaps, that of M. Dehérain.

It would carry us far beyond the proposed bounds of the present article if we were to notice, however briefly, the various German Year-Books that are devoted to special subjects, and some of which—as for example the great Year-Book of Chemistry founded by Liebig and Kopp—are complete histories of the science of which they treat. There are, however, two French Year-Books of this class, of comparatively small size, that are deserving of notice, and which we can strongly recommend to the notice of our readers, namely, M. Micé's "Rapport sur le Progrès de la Chimie Organique," of which only one volume has yet appeared, and M. Vivien de St. Martin's "L'Année Geographique," of which the eighth volume, for 1869, is now lying before us. Both of these works are models of what such volumes ought to be, and show an immense amount of labour on the part of their respective editors. We should be very glad to see something like "L'Année Geographique" attempted in this country, where we have no summary of the annual progress of geographical progress, excepting the necessarily imperfect summary contained in the anniversary address delivered by the President of the Geographical Society. G. E. D.

OUR BOOK SHELF

An Elementary Course of Botany; Structural, Physiological, and Systematical. By Prof. Arthur Henfrey. Illustrated by upwards of 500 Woodcuts. Second Edition, revised, and in part re-written, by Maxwell T. Masters, M.D. (Van Voorst: 1870.)

We heartily welcome a new edition of this standard work, brought fairly down to the present state of knowledge by one of our most active and conscientious botanists. We have not yet had time to collate the present edition with the original. Cutting the pages (what an unnecessary nuisance this is in lesson-books!), no criticism worth noting occurs to us, unless it be by way of protest against the double index—one of plant-names, the other general and glossarial. This is certainly worse than letting the book into the market uncut! Had the work been new and original, other comments would not have been wanting; as it stands, we can only congratulate the editor on the very satisfactory way in which he has accomplished his work. By the way, with regard to starch, about which Prof. Henfrey was rather strong, we can imagine the sort of haze a student who had been grinding from this edition (pp. 495-496) would manifest in his paper, were he asked to state something of the origin of that substance. Not that there is any inaccuracy in the book, but rather because possession of a greater amount of preliminary knowledge than it is reasonable to look for seems to be taken for granted. Some hint might have been given as to the head-quarters of Aleurone. Lastly, we should have preferred seeing *Amphisarca*, *Tryma*, *Diplotegia*, and their kin quietly dropped out of the edition. We doubt if there be a professor of botany in the island worth his salt who could define them. D. O.

Sketches of Creation. A Popular View of some of the Grand Conclusions of the Sciences in reference to the History of Matter and of Life. By Alexander Winchell, LL.D. (London: S. Low, Son, and Marston, 1870.)

The main portion of this volume is occupied by a sketch of the geological history of the earth; and had Prof. Winchell confined himself within strictly scientific limits, the book would have been one in every respect commendable. The titles of some of the chapters are sensational and repellent; *e.g.* "The Ordeal by Fire," "The Solar System in a Blaze," "Onward through the Ages;" and we could have wished that the author had kept aloof from speculations which are, to say the least, not profitable to the class to whom the book is addressed—on the former gaseous condition of the world and the solar system; and on the possible evolution of an animal superior to man. These parts being eliminated, the book may be safely relied on as the work of a practical geologist, who has a thorough acquaintance with his subject; and being laudably free from the excessive use of technical terms, occupies a place not precisely filled by any English treatise. The illustrations are numerous, and very various in quality. The drawing of Fingal's Cave at Staffa is a grotesque caricature; with others we are familiar in well nigh every geological handbook; especially interesting to English readers are those illustrative of the gigantic scale of geological action in the United States, as the Pictured Rocks of Lake Superior, and the Mauvaises Terres of Dacotah. Two chapters "On the Vitality of buried Vegetable Germs," and "On Prairies and their treelessness," have special reference to Prof. Winchell's well-known theory that the present vegetation of the prairies of America is lineally descended from that of the pre-glacial epoch, the seeds having retained their vitality in the ground during the whole of the intermediate time. We cannot admit that the instances quoted by the author of vegetable tissue retaining its *structure* during an enormous lapse of time, when not exposed to the oxidising influence of the air, have any bearing on the question whether germs can retain their *vitality* for the same lengthened periods; as he himself says, the proof of the theory ought to rest on direct evidence: "It must be confessed that the crucial observation has yet to be made; if vegetable germs exist in the drift, they can be discovered beforehand. I am not aware that any thorough search has ever been made for them."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Difficulties of Natural Selection

MR. Wallace's "Reply" has disappointed me. From his unrivalled knowledge of the forms of animal life in those countries where nature is the most luxuriant, and from the extraordinary interest with which he invests every subject that he handles, I had expected from him something more conclusive than that he should charge his opponent with errors which he has not committed, and should reply to his arguments by a simple begging of the question

The first "important error" with which Mr. Wallace charges me is, that "I lead my readers to understand that there is only one completely mimicking species of *Leptalis*." Where I have done so, I am unable to discover. I have, it is true, adduced one particular and striking instance as a sample of the rest, but distinctly say that "in a comparatively small area, several distinct instances of such perfect mimicry occur;" and point out how strongly, in my view, this tells against the theory of Natural Selection. In the next paragraph, "three great oversights" are alleged. Firstly, "that each *Leptalis* produces not one only, but perhaps twenty or fifty offspring" Mr. Wallace can hardly have supposed that I imagined each butterfly laid only a single egg, like the rok. The argument, however, is unaffected. In a species the numbers of which do not materially vary from year to year, it is obvious that, whatever the number of eggs laid, only one offspring from each individual, or rather two from each pair, survive to the period at which they themselves produce offspring. The "second oversight" is "that the right variation has, *by the hypothesis*, a greater chance of surviving than the rest; and the third, that at each succeeding generation the influence of heredity becomes more and more powerful." By what hypothesis? The hypothesis that these small variations in the right direction are useful to the individual—the very hypothesis against which I am contending as unproved, as neat a case of *petitio principii* as one often meets with. My "errors" in fact, amount to a non-admission of my opponent's premisses, who then naively adds, "with these three modifications the weight of the argument is entirely destroyed!" Of course it is. The "new factor of which I take no account" in the next paragraph, is again entirely dependent on the admission of the natural selectionist premisses.

With regard to the distinction between man and other animals, I much regret if I have unwittingly misrepresented Mr. Wallace's view; but if I have done so, I think it is owing to that view not having yet been clearly pronounced. Mr. Wallace distinctly states his opinion that "a superior intelligence has guided the development of man in a definite direction." ("Contributions," p. 359.) I have Mr. Wallace's own authority for saying that M. Claparède has misinterpreted him in referring this superior intelligence to a "Force supérieure," a direct action of the Creator; what alternative is there left but to suppose that it was man's own intelligence that he had in view? Whenever Mr. Wallace more clearly enunciates this portion of his theory, I think there will be no difficulty in showing that the same principle, whatever it may be, is operative in the lower creation as well as in man.

Having disposed, as I think, of Mr. Wallace's chief points of reply, I may be permitted to point out one or two errors into which he has himself, it seems to me, fallen. The changes of mimicry are, he says, "wholly superficial, and are almost entirely confined to colour." I was certainly surprised to read this, recollecting so many instances to the contrary, not only among tropical insects, but in the close approximation in form of some of our own Diptera to certain genera of Hymenoptera; and recollecting also the numerous illustrations of protective form and habit which Mr. Wallace himself gives, not only describing them but having also drawn them with such exquisite fidelity. (See "Malayan Archipelago.") In the *Kallima paralekta* of Sumatra, for instance, he says, "we thus have size, colour, form, markings, and habits, all *combining together* to produce a disguise which may be said to be absolutely perfect." ("Contributions," p. 61). Another sentence I had to read three or four times before I could believe that Mr. Wallace had penned it. In objecting to my parallelism between the development of protective resemblance and of instinct, he says, "in birds mimicry is very rare, only two or three cases being known." I do not know whether Mr. Wallace draws any subtle distinction between "mimicry" and "protective resemblance;" but if so, he should have noticed that it is the latter which I speak of as "being strongly developed in birds." I had, on reading the above sentence, to turn again to my "Contributions," to see whether I was correct in my impression that we find there the statement that "in the desert the upper plumage of *every bird* without exception is of one uniform isabelline or sand colour;" that "the ptarmigan is a fine example of protective colouring" ("Contributions," pp. 50, 51), and that two whole chapters are devoted to the wonderful protective instinct of birds in the matter of their nests.

On one point raised in my paper I am disposed somewhat to modify my views, and I do so with the greatest pleasure, in my objection, namely, to the title of Mr. Darwin's great work. Taking the origin of *species* as distinct from the origin of mere *varieties*, there is undoubtedly a sense, as Mr. Wallace points out, in which natural selection may be considered a prime factor. The law of variation is a centrifugal, the law of natural selection a centripetal force; the one acting by itself would produce a wild chaos, the other a barren uniformity: equilibrium can only be the result of their joint co-operation.

Whatever may be my "inability to grasp the theory," I hope I have shown that I have not fallen into the errors with which Mr. Wallace charges me. All the main points of the argument seem to me to be left untouched by him. He has brought forward no evidence that extremely small variations do afford any immunity from the attacks of enemies. He gives no explanation of the tendency of the *Leptalis* referred to by Mr. Bates "to produce *naturally* varieties of a nature to resemble *Ithomiæ*." He does not attempt to account for the parallelism of the development of protective resemblance and of instinct in the animal world. He fails to explain the nature of the intelligence which was operative in the creation of man, and which is a principle unknown in the rest of the organic world. Students of Nature who have spent their lives in their own country must always yield in point of experience to those who have had the advantage of comparing the faunæ and floræ of other climates, and can only arrive at their conclusions from the facts brought to their notice by travellers; these, I think, I have not misrepresented. Appeal to authority, as authority, is always to be deprecated in Science. I may, however, perhaps be permitted to strengthen my position by a quotation from a work, which I had not read at the time of writing my paper, by one who will be acknowledged to have some knowledge of the ways of Nature (Huxley's Lay Sermons, p. 323):—"After much consideration, and with assuredly no bias against Mr. Darwin's views, it is our clear conviction that, as the evidence stands, it is not absolutely proven that a group of animals, having all the characters exhibited by a species in Nature, has ever been originated by selection, whether artificial or natural."

ALFRED W. BENNETT

Westminster Hospital, Nov. 19

P.S.—Since writing the above, Mr. Jenner Weir has kindly called my attention to two papers read by him before the Entomological Society, "On the Relation between the Colour and the Edibility of Lepidoptera and their Larvæ." In one of these I find the following remarkable statement:—"Insectivorous birds, as a general rule, refuse to eat hairy larvæ, spinous larvæ, and all those whose colours are very gay, and which rarely, or only accidentally conceal themselves. On the other hand, they eat with great relish all smooth-skinned larvæ of a green or dull brown colour, which are nearly always nocturnal in their habits *or mimic the colour or appearance of the plant they frequent.*" Here at least it would seem as if *imperfect* mimicry was anything but beneficial to the individual; how can the principle of natural selection account for its propagation in these instances?

THE soul of many an anti-Darwinian will have been cheered by Mr. A. W. Bennett's paper on "The Theory of Natural Selection from a Mathematical Point of View." It is, in fact, a very admirable [illegible] special pleading, based on a skilful assumption of [illegible]misses which, to a careless or biassed observer, might seem indisputable.

The tendency to variation is spoken of as something very mysterious, of which no adequate account has ever yet been given. Yet the very simple explanation is no bad one, that where two parents are concerned in the production of any offspring, the product in part resembling each of the producers must of necessity also in part differ from each of them. Between the parents themselves, Mr. Herbert Spencer has shown that differences of age and external circumstances would ensure the requisite want of resemblance in the absence of any other cause.

"The rigid test of mathematical calculation" is then applied to the case of mimetic butterflies, with the view of showing that they could not have been produced simply according to the laws of variation, inheritance, and natural selection. In the application of this rigid test the very first step is a perfectly gratuitous assumption, "that it would require, at the very lowest calculation, 1,000 steps to enable the normal *Leptalis* to pass on its protective form." Who is to prove that fifty differences would be insufficient? An interval of a thousand years might be granted for establishing each one of these variations. Suppose even 50,000, instead of only 50 steps to be necessary, it is another gratuitous assumption that "the smallest change in the direction of the *Ithomia*, which we can conceive in any hypothesis to be beneficial to the *Leptalis*, is at the very lowest one-fiftieth of the change required to produce perfect resemblance." How small a difference must decide the choice made by a donkey placed equidistant between two bundles of hay! Certainly, then, a bird on the wing, having to choose amidst myriads of butterflies, may be determined by an almost infinitesimal distinction. Further, though the whole change may be produced by an immense number of small changes, it is not necessary to suppose that all the changes will be equally small. It is merely begging the question to assume that the first change could not possibly be large enough to be of any use. And if it may be of use, the whole mathematical calculation, based on its being useless, breaks down from the beginning. Again, since the *Leptalis* may have spent 1,000,000 years in arriving at its present likeness to the present *Ithomia*, it is impossible to assert that the normal forms of the two butterflies were as wide apart at the beginning of that period as they are at present. The mimicry having once set in, might be retained by parallel variations. This, indeed, cannot fail to be the case, if the protection is to be a lasting one; for when the *Ithomia* varies in outward appearance, unless the *Leptalis* varies in the same direction, the resemblance will be lost. This progressive mimicry would be more valuable than an imitation in which no changes occurred, since the enemies of a mimetic species would in time become aware of a fraud which had no variations at its command, as birds are said now-a-days to pounce without hesitation upon caterpillars which very much resemble twigs. Even "a rough imitation" may be useful in the first instance, and yet when hostile eyes have long been exercised, and have acquired greater and greater sharpness, finally nothing less than *absolute identity* of appearance may be thoroughly effective. Thus the perfecting of the resemblance will be no "mere freak of Nature," nor shall we be "landed in the dilemma that the *last* stages are comparatively useless" in this procedure.

The array of figures brought forward to prove that the *Leptalis* could not have made twenty steps of variation in the direction of the *Ithomia by chance*, would be much to the purpose if any exponent of the theory of Natural Selection had ever argued or supposed that it could. The calculation takes it for granted that the theory is erroneous, instead of proving it to be in error. Upon this assumption, it might have been put far more strongly, only that a stronger way of putting it would have borne on the face of it the suspicion of some inherent fallacy. It begins by supposing that there are "twenty different ways in which a *Leptalis* may vary, only one of these being in the direction ultimately required;" it might quite as truthfully, or even more so, have said a thousand instead of twenty, and then the second step would have given the chance as only one in a million, instead of one in four hundred. But while the theory of Natural Selection speaks of numerous minute useful variations, Mr. Bennett will not allow that combination of terms. Let them be numerous and minute, if you will, he says, but if small they cannot be useful, if useful they cannot be small. He claims to have Mr. Darwin's own word for it, that a large variation would not be permanent, as though Mr. Darwin had said, "living creatures have come to be what they are by successive useful deviations of structure permanently propagated, but no large deviations are permanent, and no small ones are useful." It is quite obvious that in the use of relative terms, such as great and small, Mr. Darwin neither intended to stultify himself nor has done so. A thing may be large enough to be useful without being large as compared with something twenty times its own size; and a man may be said to have a huge brain in a very small body, although the body in solid content far exceeds the brain. When Mr. Darwin says that "Natural Selection always acts with extreme slowness," he does not imply that its steps must therefore be so numerous as to be too small to confer any advantage. This would be a contradiction in terms. But the steps may be exceedingly small notwithstanding, and also sometimes separated by enormous intervals of time from one another.

In introducing his own explanation of things, Mr. Bennett affirms that "resemblances, and resemblances of the most wonderful and perfect kind" in the vegetable kingdom, "are in no sense mimetic or protective." This may be so, but it can hardly be said to be proved. When he speaks of "man's reason" having "assisted him so to modify his body as to adapt himself to the circumstances with which he is surrounded," and suggests that the instinct of animals may have assisted them also to modify their bodies by slow and gradual degrees to the same purpose, it is difficult to imagine the process intended, and still more difficult to see how "the slow and gradual degrees" will escape the rigid test of mathematical calculation which Mr. Bennett has elsewhere applied; for if the steps are great they ought not to be permanent, and if small they ought not to be useful. A theory which makes it possible for a bee to "modify its proboscis" by instinct, or for a man to treat his nose in the same manner by reason, seems harder of digestion than the Darwinian.

THOMAS R. R. STEBBING

Torquay, Nov. 12

MR. BENNETT, in his very able paper read before the British Association at Liverpool, and published in NATURE of the 10th November, calls in question the explanation given by the theory of Natural Selection of the various instances of mimicry found in the animal kingdom.

He bases his argument principally on the fact that the alterations in the early stages being useless to the animal would not be preserved, and that these changes must be very slow.

He assumes that to enable the normal *Leptalis* to imitate a species of *Ithomia*, it may be considered to have gone through at least 1,000 stages, and that no change less than one-fiftieth of the whole alteration effected would be of any use to the insect. He gives us no information as to how he arrives at these figures, and we are left with the idea that they are selected principally because they are what are called "round numbers," and are more easily dealt with in the calculation which he gives us.

Now I think that the number of stages which Mr. Bennett considers it necessary for a *Leptalis* to pass through so as to mimic an *Ithomia* is vastly too great: 1,000 stages means at least 1,000 years.

Let us look at the alteration which frequently takes place in the colouring of a butterfly, possibly in one generation, as shown by varieties of which sometimes only solitary specimens are known, figured in Newman's work on English Butterflies. I need only refer your readers to the figures of varieties of *Apatura iris*, *Epinephele janira*, *Limenitis sibylla*, *Melitæa athalia*. Now can it be contended that it required 1,000 of such stages to effect the alteration?

If any of these variations happened to be useful, there seems no reason for supposing that one stage might not make much more than $\frac{1}{50}$ of the alteration, which Mr. Bennett lays down as being the least which would be useful, and which I agree with him in considering much too small. Why might not one stage make one-fourth or one-sixth of the alteration required?

Mr. Darwin quotes a passage in his work on Natural Selection (page 32) from Sir John Sebright with regard to pigeons, in which he says that it takes three years to produce a given feather, but six years to make a head and beak. If the bony structure of an animal so far above a butterfly can be altered in six years, we surely do not require more than that time to effect an alteration in the colour of a butterfly's wing.

Mr. Bennett states that the early stages of the alteration would be useless to the insect; every one, I think, will grant this, when each stage is only one-thousandth of the whole, but not if it be a much larger quantity. Here again we may observe the instance Mr. Bennett quotes, the mimicry of *Leptalis* to *Ithomia*. *Leptalis* is normally a white insect, and as such, would be more liable to attacks from its persecutors, as shown by Mr. Wallace, while any variation which gave colour to the wing would make the insect less conspicuous, and being useful to it, would be preserved.

That we are quite ignorant of the laws regulating variation is quite true, and that when we do understand them it will throw much light on these questions is undoubted, and that we may probably find in them some additional explanation for many of the facts now accounted for by Natural Selection; and Mr. Bennett does good service in the cause of truth in reminding us of what still has to be done. S. N. CARVALHO, JR.

London, Nov. 17

FOUR years ago I advanced the opinion that Natural Selection is insufficient to explain the "Origin of Species," and that, rather, the origin of the variations of which Natural Selection is said to avail itself must be looked to for this purpose. I may perhaps, therefore, be allowed to say a few words in examination of Mr. Wallace's explanation of this point in last week's NATURE.

One of the objects of Mr. Darwin has been to show that the existence of species as an absolute entity is a mere idea of our minds; that if we could at the same moment look around us in space, and also backwards in time, we should find the organic world connected together as one whole, one great mass of beings extremely closely allied to each other, and distinguishable only by an accumulation of small and perhaps scarcely appreciable differences. A second and closely-connected object has been to show that this great mass of beings has had a common origin from one primeval ancestor (or at most a few ancestors). These two points are the chief ones involved in the "Origin of Species" question, *as it is ordinarily understood;* and if they be borne in mind, it will be seen that the doctrine of "Natural Selection, or the Survival of the Fittest," deals with only a small portion of the numerous problems involved in this great question. I am sure that Mr. Wallace, after having written as he has done about man, that in his case other influences than this survival of the fittest have been at work, may reasonably allow importance to other powers than Natural Selection in the case of other organic beings.

If Mr. Darwin's book had been entitled "The Influence of Natural Selection on the Formation of Species," some misconceptions might, perhaps, have been avoided. Its present title undoubtedly tends to convey the idea that Natural Selection is *per se* the Origin of Species. I believe Mr. Darwin, however, holds no such idea.

The picture above alluded to, of a complicated mass of beings connected together by innumerable gradations, is so different from what we find existing around us, that one of the first questions suggested by it is, where are the connecting links? This first question has never yet been answered to any extent, or with anything like adequacy. The links produced are but few, and not sufficient to bear the great weight attached to them. For at no period of the geological record do we find any traces of the general and intimate connection of beings with one another that Mr. Darwin's views would lead us to look for. The creatures composing the organic world at any one given moment were, so far as the evidence of geology goes, separated from one another by lines of demarcation of similar value to those existing among animals now.

What is wanted to explain the phenomena of various limited and defined species arising from one common ancestor is, then, first, a law, or group of laws, to throw light on the origin of variation and dispersion; and, second, another law or laws to explain the limitation and separation of the varieties so produced. It is quite out of the question to suppose that the theory of Natural Selection does all this. Those, however, who have studied Mr. Spencer's work will be well aware that his theory of evolution may be applied to deal with the question in this its more extended light. And I believe that those who wish well for the survival of Natural Selection will do well to insist on its only being considered in connection with a more extensive doctrine of evolution. This is where I think Mr. Wallace errs in his advocacy.

I will not here allude to the question of mimicry more than to say, that Mr Wallace has never answered, but rather avoided, the chief difficulties I have advanced against it; and that his theories on the subject are undoubtedly open to the objection that he insists on seeing all the phenomena from the point of view of a natural selectionist, and nothing more. As Mr. Wallace has, however, already discovered that Natural Selection, though applicable to man, is not sufficient, unsupplemented, to account for him, we may hope that he will yet see this with regard to the rest of the organic world. D. SHARP

Thornhill, Dumfriesshire

The Chromosphere

WHILST mapping down, in preparation for the coming eclipse, all the bright lines that have so far been observed and accurately measured in the chromosphere or solar prominences, I was struck with the absence of a faint yellow line, which I have myself several times observed whilst examining the contour of the sun's disc. This line is probably identical with Angstrom's absorption line 5883·0 (spectre normal du soleil), D″ lying almost midway between D′ and the line in question. There is no danger of mistaking it for the bright yellow line seen in every solar prominence, and lying near Angstrom 5865·1, since the two yellow lines were seen on each occasion at the same time on the more refrangible side of D″.

I suppose the D‴, mentioned in a late communication from Dr. Young, to be identical with the bright yellow line, for it is most improbable that he could have failed either to see or to record the bright line whilst mentioning the faint one, since the latter, as far at least as I have observed, is never visible unless in company with the former.

The only observation that I can at all identify with my own is that mentioned in NATURE, December 16, 1869, where Mr. Lockyer, speaking of the absorption line, which corresponds to the orange line of the chromosphere, says that Padre Secchi's bright line is less refrangible.

Stonyhurst Observatory S. J. PERRY

From London to Catania

A FEW practical details as to the best way of getting to Sicily, the accommodation to be found there, &c., may be of use to many readers of NATURE who are thinking of going there next month.

We have first the sea passages from London, Southampton, or Liverpool, to Messina or Malta, of which if any be chosen it will probably be that from Southampton to Malta by the P. and O. steamers, which start every Saturday at 2 P.M., and are nine days on the voyage. (Fares 20*l.* and 10*l.*) From Malta there are steamers twice a week to Messina; they touch at Catania when the weather permits them to enter the small harbour, otherwise they go on to Messina, so that passengers for Catania must in that case avail themselves of the railway.

Few probably will wish to go the whole way by sea, the land route therefore by which the Indian mails are now sent will be taken; viz., over the Brenner Pass. The night mails leave Charing Cross at 8.45 P.M., Cannon Street at 8.50 P.M., Victoria and Ludgate Hill at 8.30 P.M., and arrive in Dover in time for the Calais and Ostend boats; the line from Calais to Brussels *may* not be practicable, and so the longer passage to Ostend may be preferred; by going straight on one ought to arrive at Cologne at 4 P.M. the next day (if one goes by Calais one has three hours' rest at Brussels). The day service train, first and second-class, leaves all the stations at 7.40 A.M., and one should arrive at Cologne *via* Ostend at 10.55 P.M. (*via* Calais at 4.50 A.M. next day.) From Verviers to Cologne there are only first-class carriages in this train. The fares to Cologne by Ostend are 3*l.* 8*s.* 10*d.* first, and 2*l.* 9*s.* 5*d.* second-class, by Calais they are 3*s.* or 4*s.* more.

Those who like to go from London to Ostend or to Antwerp *direct* can leave St. Katherine's Wharf by steamer on Sunday, Tuesday, or Thursday mornings for Antwerp, or on Wednesday or Saturday mornings for Ostend, and proceed by rail to Brussels, the fares from London to Brussels being 30*s.* first and 22*s.* 3*d.* second class, *via* Antwerp; 26*s.* 8*d.* first and 20*s.* 10*d.* second class, *via* Ostend. The fare from Brussels to Cologne is about 25 francs first and 18 francs second class by the ordinary trains; express about three francs more.

The way then is by Coblentz, Mayence, Darmstadt, and Aschaffenburg to Munich. By leaving Cologne by the 6 A.M. express, one ought to get to Munich at 9.10 P.M. In times when through-tickets are granted the fare by Ostend and Cologne to Munich is 6*l.* 7*s.* 3*d.* from London (first class), and 5*l.* 10*s.*

mixed first and second; this will give some idea of what the cost will be.

After Munich the regular trains may be relied on; one can leave Munich at 9.50 P.M. (first and second class), pass Innsbruck, and crossing the Brenner in the early morning descend the Italian side in the forenoon, getting to Verona at 1.20 P.M.; or by leaving Munich at 10.15 A.M. (first, second, and third class) one may get to Verona at 5.50 A.M. next day; but in this way one misses the best of the scenery. The fares from Munich to Verona are about 37*s.*, 25*s.*, and 18*s.*

The way, then, is to Padua, Bologna, Pistoia, and so either to Florence or to Leghorn (by Pisa); from Florence to Rome, passing by Lake Trasimene; from Leghorn to Rome by the coast line, or else straight to Messina or to Naples, and then to Messina, by one of Rubattino's boats, which leave almost daily. From Rome one goes to Naples by train, and thence by boat to Messina, passing close to Stromboli. Some of the boats go on to Catania, but it is advisable to land at Messina and take the train, as often the steamer cannot get into the harbour at Catania, in which case it goes on to Malta. The railway fares from Verona to Naples come to about £4 10*s.* first; £3 5*s.* second; and £2 5*s.* third class.

By travelling *almost incessantly* one should (supposing the trains regular) get to Naples from London in five days and six nights by this route: no time is lost by spending a night at Cologne.

At Messina, the Custom House authorities are usually rather troublesome, but one does get off at last, and, passing along a most exquisite coast-line, arrives at Catania (Κατ' Αἴτνη).

The hotel at which to stay, if possible, is the Grande Albergo, kept by Herr Werdenberg, where there is every comfort, the very small salon being redeemed by the fine billiard-room. The front rooms face towards the north, and are cold and unsuited for invalids, but they (especially those of the third story) afford a most splendid view of Etna, the sunrises and sunsets seen from them being superb. The rooms at the back are much warmer, but of course give no sight of Mongibello. The Grand Hôtel Central is an Italian establishment in the Piazza dell' Università, and may be considered to be the second, though much inferior to the first-mentioned house.

In Catania itself a good post of observation would probably be the Giardino Bellini on the Corso; it is high and sufficiently large.

To go to Nicolosi a two or three-horse carriage is necessary. There are at this village two inns, one at the entrance to the village (not to be recommended), the other one, which is preferable, farther on in the village. The accommodation is of a very primitive description. Everything should be taken from Catania, as little can be got at Nicolosi.

From Nicolosi one can visit the Monti Rossi (in half-an-hour or three-quarters), from which one has a magnificent view, and one can, if one is curious enough, go down into a hole, known as the Fosse dei Palumi, a volcanic vent.

It is from Nicolosi that the ascent of Etna is made, and a description of an ascent, under especially favourable circumstances, will be found in the number of NATURE for June 23 last.

The best guide is Pietro Cravagna, who knows the mountain thoroughly, and who also speaks tolerable Italian; he is in every way to be trusted, and if another guide be necessary, it will be well to let Pietro find him. The writer was on one occasion subjected to great annoyance from the incompetence of one of the so-called guides.

The Casa del Bosco, about two-and-a-half-hours' ride from Nicolosi, is uninhabited during the winter. A fire of sticks may be made there, and a few plates, &c., will be found; the key must be got at Nicolosi; there is plenty of good water close at hand. This house might be used for purposes of observation; it has two rooms, and an outhouse for the mules.

The Casa degli Inglesi, near the top of Etna, is almost sure to be buried in the snow.

In descending from the summit it may perhaps be possible to go down into the Valle del Bove, and return to Catania by Zaffarana. Those who visit Sicily should not return home without stopping a day or so at Taormena (between Catania and Messina), and seeing a sunrise from the ruins of the theatre.

W. H. C.

The Spectrum of the Aurora

As some of your correspondents seem scarcely aware of what has already been accomplished in observation of the auroral spectrum, perhaps I may be pardoned a few remarks on t subject.

The line usually most prominent in the auroral spectrum is yellowish green one, the wave-length of which was measured Angstrom as 556 7, and its position by Professor Winlock 1280 on Huggins' scale, which, reduced to wave-length, close agrees with the determination of Angstrom.

Angstrom also observed the same line in the spectrum of t zodiacal light, in March 1867, but it seems possible it might due to faint aurora concealed by the light. He says that " is a remarkable fact that this bright band does not coincide wi any of the known rays of simple or compound gases which have as yet examined." The wave-length of H β is abo 486·2; much less than that of the auroral line. Angstrom al saw three very feeble bands near H β (F).

Professor Winlock (*American Journal of Science*, No 1869,) states that in addition to the line at 1280 Huggins' scal he saw six faint bands, viz,, at 1400, 1550, 1680, near F, 264 and near G.

In the *American Journal of Science*, Sept. 1869, it is stat that during the solar eclipse a bright line was seen in the spectru of the corona at 1474 of Kirchoff's scale, and that it coincid with an auroral line. 1474 Kirchoff corresponds to about 15 Huggins' scale.

I have also somewhere seen it stated that the auroral line 1280 coincided with a telluric line in the sun's spectrum, whi might be possibly due to oxygen.

I have myself seen several feeble bands between the green li and F, but owing to their faintness have not yet been able determine their position with much accuracy.

The red line which was so bright in the aurora of the 24th ar 25th ult. is only occasionally visible. Mr. T. W. Backhouse h observed it repeatedly, and informs me that it is sometimes visib when the aurora does not appear red to the eye, but that never recollects seeing it when some part of the sky was not re This quite agrees with my own experience. As your correspon ent, "T. F." observes, the red line probably belongs to a spectru distinct from that of the green line, and may be due to son other gas. It may, however, be only a fresh line of the same g due to different temperature. Its position from repeated dire comparison is about ·4 of the distance from Hα, to Na. as stated a week or two since. It is, therefore, not identical wi Hα, to which the ordinary red light of ignited hydrogen is du

Changes of pressure and temperature do not affect the *positi* of lines, but merely influence their breadth and intensity, maki new lines visible and expanding old ones. Sometimes, as in t well-known case of nitrogen, an entirely fresh spectrum is pr duced; but while any line remains visible its position is u changed. Hydrogen gives several such spectra, but I belie none of them have a line in the position of the auroral one.

I am at present engaged in a little research on the spect of certain gases in relation to that of the aurora; but it is n yet sufficiently advanced for publication.

It is particularly desirable that the positions of lines should accurately determined. In the case of the aurora I am acquaint with no better method for doing this than by comparison wi such a spectrum as the band spectrum of N. This is a most co venient natural scale, with thirty or forty brilliant bands; and m readily be obtained from a small tube containing rarefied air nitrogen, by the aid even of a Ruhmkorff's smallest coil.

With regard to the spectroscope, a simple flint glass pris fitted to a tube carrying an adjustable slit, and without a lenses, gives a brighter spectrum than any other form of instr ment that I am acquainted with.

HENRY R. PROCTER

Clementhorpe, North Shields, November 12

The November Meteors

ON the nights of the 12th, 13th, and 14th of November t sky was constantly watched from 5 P.M. to 7.30 A.M. T weather throughout was most unfavourable.

On Nov. 12th it was completely overcast from 7 P.M. 7.30 A.M.

On the 13th from 5 P.M. to 7 P.M. the amount of cloud w $\frac{7}{10}$, and only one meteor was seen. The sky was then obscur until near 1 A.M. of the morning of the 14th.

Nov. 14th, from 1 A.M. to 3.50 A.M., the amount of cloud w $\frac{8}{10}$, and four meteors were seen, two starting from near γ Leon

This was preceded by a hailstorm and rain, with occasional breaks in the clouds, through which we observed four meteors between 5 and 6.30 A.M.

On the evening of the 14th the sky was only half covered with clouds from 5 to 8 P.M., and eight meteors were observed between 5.48 and 6.40; one at 6.2 was of a brilliant red colour, with a pale greenish white train.

From 7.40 to 8.35 five other meteors were seen. The sky cleared for a short time towards 8 P.M., but at 9 a mist came on which obscured the heavens during the remainder of the night, clearing off, however, occasionally for a short time. I will not trouble you with the path of each separate meteor, though each was carefully noted. From the above observations I should be inclined to think that we had passed through the maximum during the afternoon of the 14th. Had there been any brilliant display during the night of the 14th, I think it would hardly have escaped me in spite of the mist.

Stonyhurst Observatory S. J. PERRY

SPAIN AND THE ECLIPSE EXPEDITION

THE following is a translation of a letter which appears in the *Astronomische Nachrichten* for Nov. 15, on the facilities offered by the Spanish Government to such foreign astronomers as purpose visiting Spain on the occasion of the approaching eclipse:—

"MADRID, *Nov.* 5.

"I have the honour to inform you that the Spanish Government, at the request of the Observatory at Madrid, and in accordance with the resolution taken at the time of the eclipse of the sun in 1860, has just agreed that similar measures shall be adopted for facilitating to foreign astronomers the observation of the approaching solar eclipse on the 22nd of December of the present year. The Government has in consequence resolved,—

"'That at the Spanish Custom Houses no duty or deposit shall be demanded on the astronomical or physical instruments that astronomers bring into Spain for the observation and study of the eclipse.'

"But as this privilege, which has been granted with readiness to astronomers, might be taken advantage of by persons noways connected with Science, the Government has deemed it necessary to adopt certain measures of precaution, the principal one of which is, to be made cognisant of the names of the persons who are making preparations to come to Spain to observe the eclipse. In consequence thereof, the Minister of Finance has directed 'that such astronomers as purpose availing themselves of the resolution above spoken of should have the goodness to make known in writing to the Observatory at Madrid their names, the number and the class of instruments which they bring, and the point of the coast or frontier where they purpose entering Spain.' These particulars will be communicated by the Observatory to the Government, which will send orders to the Custom-houses to pass without difficulty all the instruments entered on the lists the astronomers furnish. Foreign astronomers may, moreover, reckon on the sedulous protection of the provincial governors and of the local authorities, from whom they will receive all the co-operation necessary to enable them to devote themselves with entire liberty to their scientific labours.

"In the Almanac of the Observatory of Madrid for 1870 (which you have not received owing to the want of communication with Germany for several months) there is contained a somewhat detailed account of the approaching eclipse, accompanied by two maps. As you will observe, in the zone of the total eclipse there have been inserted all the principal towns, in order to assist astronomers in the selection of their stations for observing. This central line is not of great dimensions in Spain (about sixty nautical miles) yet, nevertheless, there are numerous important towns in proximity to the central line, as, for instance, San Lucar, Jerez, Puerto de Santa Maria, Puerto Real, San Fernando, Cadiz, Medina Sidonia, Estepona, and at those places observers will meet with all the resources requisite for carrying out their labours with facility. The sole disadvantage of so short a line is, that if the weather should prove unpropitious at one station, it will probably be so at the others as well.

"If you think any further details necessary, or in the case of any astronomer wishing to consult the map of the eclipse, nothing more will be necessary than to apply to the Director of the Observatory at Madrid, who tenders his services to such foreign astronomers as require them, and to whom it will afford great pleasure to aid his colleagues in bringing their scientific mission to Spain to a successful result. ANTONIO AGUILAR"

THE CONSTRUCTION OF HEAVY ARTILLERY

IN few other manufactures has it been found necessary to search so deeply into the materials nature provides in order to find out the best and strongest, and then to apply it skilfully, so as fully to develop its strength, as in the manufacture of guns. The construction of the amazingly-powerful ordnance which modern naval warfare employs is pre-eminently a question of strength of material; indeed, it may be termed *the* question of strength of material. In nothing else does man employ forces even nearly so powerful and violent. The force of steam, even when doing its mightiest work, is but faint and small compared with that of the exploding charge of gunpowder that sends from the gun a 300lb. or 600lb. shot with a velocity which carries it through thick armour plates of wrought iron. A 600lb. shot will pierce twelve inches of iron at 200 yards distance. This gigantic force is imparted to the shot in the brief fraction of a second that it is moving down the barrel of the gun. Remembering that "the gain in power is loss in time," and consequently that when the time is diminished the power is proportionately increased, we may form some conception how enormously great is that force which is exerted within the breech of a heavy gun, and which is resisted by it every time it is fired. It is a force which, if turned into foot pounds, would represent the steam power not of a ship but of a navy. Yet all its work is to be done in the space of a few inches, and it must be surrounded with iron strong enough to resist it. Here we have the skill of man grappling with enormous difficulties, searching out the strongest and most suitable material that nature supplies, and exerting all his art to apply it to the utmost advantage. The construction of these exceedingly powerful guns has been entirely developed within the last few years. The gun now manufactured in Woolwich Arsenal is more unlike the gun of 1850 than the gun of 1850 is unlike that of Queen Elizabeth's reign. The progress of twenty years surpasses that of three centuries. And the change has not been so much in enlargement of size as in difference of construction. Queen Elizabeth's pocket-pistol is not more unlike a 600-pounder in external appearance than in internal structure. The gun which is carried in the turret of one of our ironclads, and which, at a single discharge, expends as great a weight of powder and shot as the whole broadside of a good-sized frigate of our own early days, does not surpass the gun which peeped from that frigate's ports so much in size and power as in the superior scientific principles of its manufacture. We propose in the present article to give a general view of these principles. The method of manufacture will be first explained, and afterwards the principles which guide the selection of the best material. Although the material must be selected before it is manufactured, yet a knowledge of the construction of a heavy gun, and of the qualities sought by construction to be developed, will very greatly facilitate our comprehension of the reasons of choice and preference among the many kinds of iron that might be and that are used.

In explaining the construction of modern ordnance as made for the British Government, it will be best to notice the gradual progress in the manufacture since wrought-iron began to be used instead of cast-iron. This was the first great change, and from it dates a new era in this branch of industry. And it was not only a great change, but a great advance. Wrought iron is a very much superior material to cast-iron, and one which demands very much more skill in its manufacture. Cast-iron is of a granular or crystalline nature; wrought-iron is fibrous: cast-iron is hard; wrought-iron tough. The difference between them may be illustrated by the difference between glass and wood. One is strong to resist a statical strain or pressure, the other to resist a dynamical strain or blow. There is a vast difference between the two kinds of strength. A brick which is at the foundation of a lofty factory chimney supports an enormous weight, but it would be broken by a blow that would not injure a stout walking-stick. Wrought-iron having that kind of strength which resists dynamic force is therefore far preferable to cast-iron for resisting the violent and sudden shock of explosives, the most powerful dynamic strain with which man's art has to grapple. It averages three times the dynamic strength of cast-iron, that is, it will bear three times as great weight without breaking. It will yield sooner; but when cast-iron yields it breaks. In this another great advantage is gained. When a cast-iron gun breaks it does so explosively; it breaks up into fragments, and gives no warning, no indications of yielding beforehand. But a wrought-iron gun shows when its use is becoming dangerous.

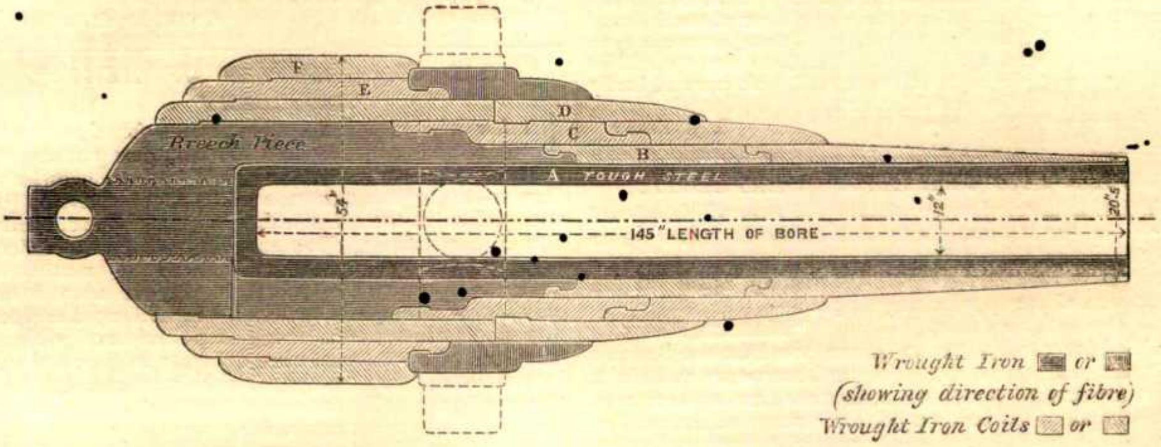

FIG. 1

Though this discussion seems rather at variance with the plan laid down, yet it is necessary to have a general knowledge of the material used in order to understand the method of manufacture. Wrought-iron, while it is so much better a material for the construction of heavy guns, is yet very difficult and expensive to work. The wrought-iron gun cannot be made as easily as the gun for which the molten metal was run into a mould and then bored out and finished exteriorly. It requires large furnaces, huge steam-hammers, and skilled workmen to give it shape. Before the wonderful appliances of modern science and machinery were invented, wrought-iron could only be made and worked in comparatively small quantities. And even now to forge the mass necessary for a gun 7, 12, or 25 tons weight, would be a most difficult and costly, perhaps in the last case an impossible undertaking. No doubt there are larger forgings used in large steam ships for cranks and shafts, and in other machinery; but these masses of wrought-iron are not heated and hammered the whole at once. Separate parts are welded together, or successive portions are heated and hammered. It is

Scale 3/8" = 1 Foot.

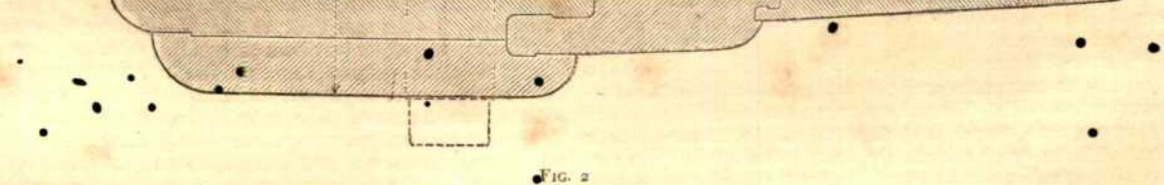

FIG. 2

needless to say that these methods would not do for the construction of a gun. The fiercely expanding gas of the exploding powder would speedily and fatally detect any plane or point of weakness. Moreover, wrought-iron is not equally strong in all directions; being fibrous in its texture, it is twice as strong with the grain as across the grain. As in the case of wood, which it is much more easy to split than to break, so it is much easier to tear the fibres of wrought-iron from each other than to break them across. It therefore follows that a gun forged in a solid mass, and bored out, would not put the strength of wrought-iron at its greatest advantage. Such a gun would be very strong along its longitudinal section, very strong to resist the strain of the gunpowder to tear out the breach; but it would be only half as strong in its transverse or cross section to resist what may be specially termed the bursting strain. These difficulties were entirely overcome by Sir William Armstrong's system of making a gun of coils of wrought iron bars. By that the difficulty of forging a large mass is altogether put away; and the fibre of the iron passing round the bore of the gun instead of along it, gives the greatest possible resistance to the bursting strain of the powder's explosion. This was a very great advance, a most valuable improvement in the manufacture. It took away the difficulty and expense which were the great obstacles to constructing ordnance of wrought-iron, and at the same time applied it in such a manner as to increase, or rather put at the utmost advantage, its strength in resisting the transverse or bursting strain of the powder's explosion, which is the most difficult and important strain to overcome. It is for this last that this invention deserves its highest praise; for gun-making is above all a question of strength of material. The best material applied in the best way is hardly strong enough to resist the enormous charges behind the enormous shot which are required to pierce the armour-plated vessels of modern warfare.

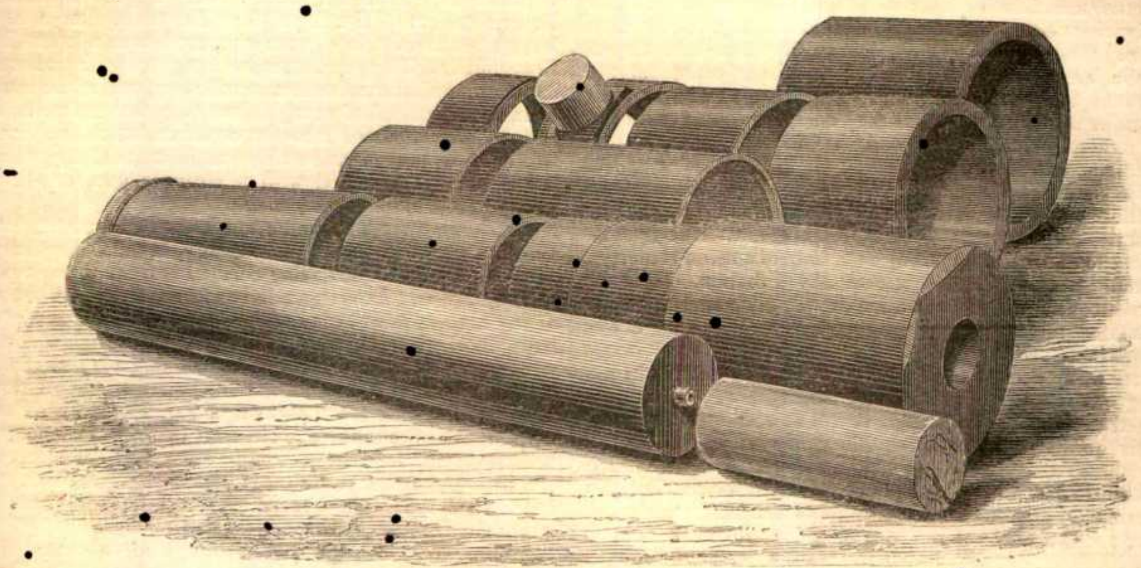

FIG. 3

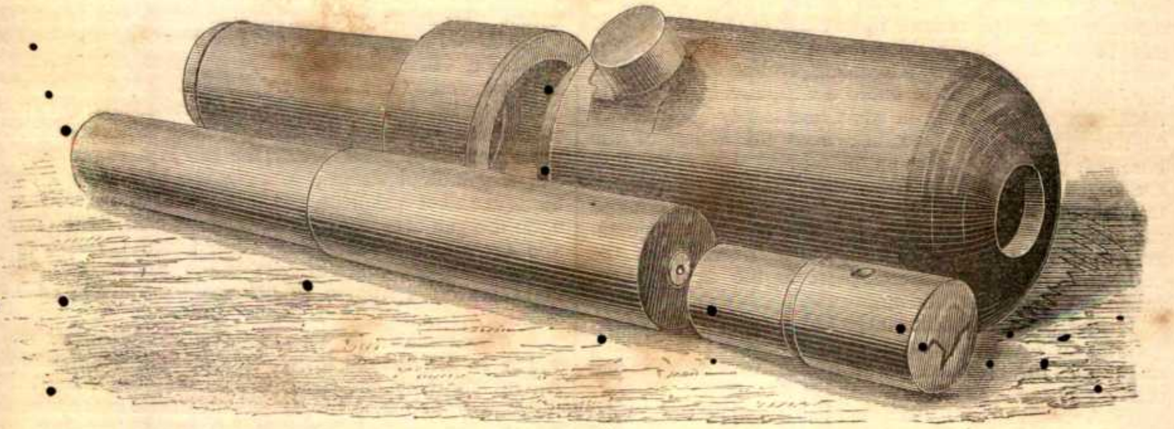

FIG. [illegible]

The method of making the coils is as follows: A long bar of iron is heated to nearly a white heat in a long low furnace, and when thus rendered soft, it is hooked on to the side of what resembles a gigantic reel of iron. This reel or core is then turned by machinery, and the glowing bar is wound upon it; being drawn from the furnace upon a travelling groove, which, aided by blows on the bar from a heavy hammer suspended above and guided sometimes by two men, keeps the bar in its proper position as it is coiled. When that is completed, it is removed from before the furnace, the reel or core taken out, and the coil allowed to cool. Afterwards it is heated in a reverberatory furnace, and welded together by blows on the ends from a steam-hammer; the edges of the coiled bar are melting from heat, and therefore unite when thus forcibly pressed on each other, so that it forms a complete hollow cylinder or tube of wrought-iron, the fibre going round the circumference. The rough surfaces are afterwards turned off in powerful lathes. These coils are made of various sizes, and several of them are required for each gun. They are *shrunk on*, that is, the outer ones are not quite large enough to go over the inner ones, but are heated, and when thus expanded are placed over their smaller brethren, whom, as they cool, they clasp in a tight embrace. Thus all the coils are in a state of tension inwards, and this was supposed to increase their power of resisting the shock of the discharge which came from within outwards. However, this theory very decidedly admits of question. Even a little observation seem sufficient to show that anything in a state of tension is thereby weakened to resist a shock in any direction. A shock produces a kind of undulation or vibratory action, so that its effect returns back in the direction in which it was imparted. In Sir Joseph Whitworth's guns the hoops were made accurately to fit each other, so that no shrinking was required; but a little shrinkage, we believe, was used to ensure close fitting. It will be seen further on that this question of the advisability of shrinkage does not apply to the guns now made for the British service. Hitherto we have only spoken of coils, which, though a main part and distinctive feature of the Armstrong gun are not the whole of it. A gun made altogether of coils would lack strength in resisting the longitudinal strain, or tendency of the discharge to tear out the breech endways, and this would be an awkward event for the gunners, if the gun fired, so to speak, at both ends. To prevent this, Sir William Armstrong had a large forged piece of iron, like a great cap, placed on the breech end of the inner tube and under the coils. The fibres of this, running longitudinally, made it strong in that direction, and guarded against a catastrophe so much to be dreaded. This, however, was a large forging, and therefore very expensive; and, also, while it strengthened the gun longitudinally, it weakened it transversely, by taking up the space nearest the bore where the greatest part of the strain was sustained, and filling it with iron whose fibres were in the direction of the bore instead of around it, as those of the coils. It is easy to see how much this took from the power of the coils to resist the lateral, transverse, or bursting strain of the discharge. The force which the expanding gas exerts on the material of the gun must necessarily be inversely as the square of the distance from the centre of the bore. A coil removed from the tube by the thickness of the forged breech-piece cannot resist the full strain of the explosion nearly so effectively as if it came at once round the tube. Its strength is applied at a disadvantage represented by the ratio of the square of the radius of the coil round the forged breech-piece to the square of the radius of a coil round the inner tube.

Besides, there is another large forging in the Armstrong gun, the trunnion-piece, which is placed round the middle, and carries the trunnions or short arms by which the gun rests on its carriage.

An Armstrong gun may be thus summed up. (The section of a 600-pounder is shown in Fig. 1 as an example.) First there is a tube of steel (A); this metal is always used for the inner part, as its hardness and closeness of grain make it better adapted for the rifling—the grooves would be quickly worn by the friction of the studs of the shot in softer metal—and also better to resist the action of the violently expanding gas of the exploding charge of powder. All attempts to make the inner tube of coils were unsuccessful; the gas at its enormous pressure searched out and took advantage of the most microscopic flaws. Next comes the large forged breech-piece behind the steel tube, and extending some distance along it. Then come the coils (in 5 sets, B, C, D, E, F), shrunk on one over another. And lastly, the trunnion piece round the middle.

The Armstrong gun, as described in our former paper, was the pattern on which all our guns were made for the British service till 1866, when very important changes, which had been proposed by R. S. Fraser, member of the Institute for Civil Engineers, and Deputy-Assistant-Superintendent of the Royal Gun Factories, after a prolonged series of trials, were approved and adopted by Government. This gentleman, not long before introduced into the manufacture of ordnance a cheaper, and, at the same time, a better kind of wrought-iron than that before used, and he has imported into the construction of the Armstrong gun very considerable modifications, by which the country is provided with a stronger gun, one-third cheaper, and more quickly made.

These are three very important items of improvement; viz. strength, cheapness, and rapidity, because simplicity, of manufacture. The saving effected is from 35 to 40 per cent. on the vast sums expended on heavy ordnance. Most of our readers have heard of the Fraser gun, but few, perhaps, know where or how it differs from the original Armstrong gun, although all our heavy ordnance is now made on this pattern. The information, therefore, may be not uninteresting, and a comparison of Fig. 2 with Fig. 1 will help to make the difference clearer. Instead of the forged breech-piece, the many small coils, and the forged trunnion-piece that form the Armstrong gun over the inner steel tube, Mr. Fraser uses one immense coil, of which the trunnions are part, and which is closed behind the tube by a large screw forming the cascable, and which is the only forging used in his gun. This will show at once how the economy is effected. Both the large forgings of the Armstrong gun, the breech-piece, and the trunnion-piece are got rid of; and instead of having many coils to be turned, and have their inner and outer surfaces reduced, upon which labour and time were expended, in addition to the waste of metal, there are only the two surfaces of the one great coil to be turned. In the 600-pounder, on the old principle, there were sixteen coils, and twice that number of surfaces, each representing labour and loss. For these reasons also the guns may be made much more quickly. This is a very important advantage, as in an emergency the country could be more quickly armed. Strength is also gained to resist the transverse strain in two ways, because the coiled iron comes next the steel tube, where the forged breech-piece used to come formerly, and so the coils are applied at greater advantage, and secondly, because the one thick coil is stronger than several thin coils, just as a triple deal is stronger than three inch deals. And further, the gun is stronger to resist the longitudinal strains, because the breech and trunnions are all of one piece, and so the force of the discharge upon the gun acts through the trunnions on the carriage, and has not, as in the old pattern, a tendency to destroy it by tearing one piece or part of the gun from another. It is converted from a longitudinal bursting strain into recoil.

It only remains to describe how this immense coil, which is the marked feature of the Fraser gun, is made. A long and thick bar, much thicker than the one used in the Armstrong pattern, is heated and coiled in the manner before described. When this has cooled, another bar, somewhat longer, is coiled upon it in an opposite direction, that is, if the first coils go from right to left, the second go over them from left to right, just as the boa constrictor overlaps his coils on the prey which he is crushing. And then a third bar is coiled in the same direction as the first. The whole is then heated in a large reverberatory furnace, and a few blows from a powerful hammer weld them into a thoroughly combined mass.

This principle of construction seems to apply the iron to the utmost possible advantage in resisting the force of the exploding charge. There is an eloquent testimony to the excellence of the system in one of the first guns made on the Fraser principle, which was tested to destruction in the preliminary trials that took place before the system was adopted, and is now to be seen in the *cemetery*, or place where such guns are preserved for inspection in the Royal Arsenal. This gun, a 64-pounder, having fired a greater weight of powder and shot than any other of its own size, and latterly with charges increased till it was destroyed, burst in this way: part of the tube, which was worn through, and the coil round the front of the tube came out and left the entire mass of the trunnion and breech-piece uninjured, so that not only would this bursting have done no injury to those who served the gun, but if a new tube and fore-part were put in, the trial might have commenced again.

Welding a coil, however large, is a much easier and less expensive process than forging and hammering into shape a mass of iron of much smaller size. However, the great size of the coils of Fraser guns of large calibre necessitated the employment of correspondingly large furnaces and machinery. These difficulties have been very successfully overcome in the Royal Gun Factories. The furnaces have been enlarged from a cubical content of 60 feet to 600 feet. At present a gun is being made of 35 tons weight, which will hurl a shot of 700lb. weight with a charge of 120lb. of powder (the battering charge for the ordinary 25-ton 600-pounder being 70lb.) All the coils for this enormous weapon have been welded without accident or hindrance. In one case as much as 28 tons of iron have been heated in one piece in the furnace, seized by the tongs, and placed in a glowing mass beneath the hammer. This is an achievement unprecedented in iron manufacture, and which reflects the highest credit on this most important Government department. Nowhere else, and for no other purpose, have such gigantic masses of metal to be heated and manipulated.

Figs. 3 and 4 show the parts of an Armstrong gun, and of a Fraser gun, before they are put together. Both are 300-pounders, and the engravings have been made from photographs of the actual guns.

NOTES

We are in a position to state that the arrangements of the Eclipse Expedition are rapidly progressing,—thanks to the untiring labours of the strong Organising Committee, which meets almost daily. As we stated before, the Government are bringing all their power to bear in favour of the work, and, should the weather be favourable, we may expect such a series of observations as has never been made of an eclipsed sun. As at present arranged, there will be four parties. Beginning with Spain, we have one to Cadiz, in charge of the Rev. S. J. Perry, and one to Gibraltar, under Captain Noble. The English branch of the Anglo-American Expedition will be under the charge of Mr. Lockyer; while there will be a fourth small expedition, under the charge of Mr. Huggins, to Oran; the Cadiz, Gibraltar, and Oran parties will leave Portsmouth on the 5th of December in the *Urgent*. The Sicilian party will leave London on the night of the 7th by the Brenner pass, a ship of war meeting them at Naples. Although not a single official astronomer has volunteered to go, there will be lack of neither skill, discipline, nor organisation; and arrangements are already being made which will ensure a full and early publication by the Organising Committee of the scientific results obtained. Printed instructions are being prepared by the Committee for each class of observations. So much for the English Government Expedition. With regard to the American one, we may add that it has been no less strongly and carefully organised, with the distinct advantages that astronomy is more cultivated in America than it is here, that the official observatories are fully represented, and that as all the observers were present at the Eclipse in 1869, they therefore may be regarded as veterans. Professors Young, Pickering, Newcomb, Peters, Watson, Harkness, and others are at present in London, and are daily affording most valuable information to the Organising Committee and the various observers.

The following memorial to Her Majesty's Government on the danger to which the scientific, literary, and art collections of Paris are now exposed, has been forwarded to the Earl of Granville from the University of Dublin:—"We, the undersigned, Provost, Fellows, and Scholars of Trinity College, and Professors of the University of Dublin, desire to express our satisfaction with the efforts made by Her Majesty's Government to restore peace in Europe, and our earnest hope—shared, we believe, by the nation at large—that these efforts may be eventually successful. But if, unhappily, our desire should not be realised, your memorialists venture to urge that the interposition of Her Majesty's Government may be directed to preserve, if possible, the great scientific, literary, and art collections of Paris, which are, in truth, the property of the whole civilised world. It is impossible to contemplate calmly the irreparable loss which the destruction of these collections, or even any serious injury to them, would inflict upon students of every nation. To avert, if possible, such a calamity, is now the duty of all; it is more especially the duty of every scientific and literary institution. Your memorialists would, therefore, in the name of our ancient University, earnestly entreat Her Majesty's Government to interpose their good offices with the belligerents, for the purpose of saving these matchless treasures from a danger which the fate of the Library of Strasburg proves to be only too real."

We understand that Dr. Neil Arnott, in addition to his recent munificent donations to the Universities, has just presented 500*l*. to the Aberdeen Mechanics' Institution, to aid in maintaining lectures in Physical Science.

At the examination for Foundation Scholarships, held in the week after Easter, 1871, one or more scholarships will be obtainable by proficiency in the Natural Sciences, at Trinity College, Cambridge. Should one scholarship only be so assigned, preference will be given to the candidate who shows the greatest proficiency in physiology and the allied subjects. The Examination in the Natural Sciences is open to all undergraduate members of the Universities of Oxford and Cambridge. The value of the scholarship is about 80*l*. per annum for five or six years.

Dr. Michael Foster (the newly-appointed Prælector of Physiology at Trinity College, Cambridge) commenced on the 14th inst. his course of lectures in a part of the new museums, which has been temporarily fitted up as a Physiological Laboratory. He gave a lucid and able exposition of the three great factors of life—contractility, as evinced chiefly in muscles; irritability, as evinced chiefly in the nervous system; and secretion. Dilating upon the much-vexed question, how far these are attributable to physical agencies, or are to be referred to another agency called "Life," he compared the latter view to a fortress closely besieged by an able band of investigators who are ever narrowing its area, and pressing the physical forces closer and closer upon it. But it has not yet capitulated. No one has a right to say that it will or will not capitulate; and till it has done so we are perfectly justified in regarding it as an entity, as a something to be taken into account in the investigation and the attempts at the explanation of living processes. He should still, therefore, use the term without committing himself to either view. He gave definitions of Physiology and Morphology. He spoke of the enormous importance of vivisection to the advance of Physiology. By it Vesalius might be said to have laid the foundations of Physiology; by it Harvey had been enabled to obtain the proofs of his great discovery. Without it all that had been written on Physiology would have gone for very little, and we should still have been in the Aristotelean mists darkened by the theories of the Schoolmen. He wished, however, to state that in the teaching of Physiology it would be necessary for him to resort to it much. He stated the plan he intended to pursue in carrying out the intentions of those who had placed him in that honourable position. Lectures he did not regard as a very fructifying mode of sowing seed. He thought it far better that men should work and see for themselves. With the munificent aid of Trinity College, he hoped, ere long, to make the physiological laboratory in Cambridge one of the best working laboratories in the country. He intended to have practical classes in addition to the lectures; and students who were competent would have opportunities for private work. It would be a labour of love to him to render practical aid to those who needed it, and to promote the study of physiology by every means in his power.—A considerable number of the senior members of the University were present, as well as undergraduates, and warmly applauded at the close of the lecture. The lectures are for the present open to all members of the University without fee.

At the recent examination for the Natural and Experimental Science Moderatorship at Trinity College, Dublin, Gold Medals were awarded to R. Apjohn, W. F. Burton, and T. F. Fleetwood (*Sch.*), and a Silver Medal to R. Barrington. The subjects examined in were—1. Physics; 2. Chemistry; 3. Mineralogy and Geology; 4. Palæontology, Zoology, and Botany. No candidate was allowed to present himself for examination in more than two of the four branches.

Rev. Prof. Haughton, M.D., F.R.S., has commenced a course of Lectures in Trinity College, Dublin, on Physical Geology, and Prof. Macalister, M.D., a course on the Anatomy, Physiology, and Classification of the Mollusca.

By the resignation of Mr. J. J. Bennett, the office of Keeper of the Botanical Collections at the British Museum is now vacant. The appointment rests actually with three only of the trustees, the Archbishop of Canterbury, the Lord Chancellor, and the Speaker of the House of Commons, by virtue of their offices. Among the whole body of the trustees, who would naturally be consulted, there are only four scientific men, the President of the Royal Society, Sir R. J. Murchison, Lord Enniskillen, and Sir Philip Grey Egerton. Now that we are about to remove our national collections to a new building erected for the purpose, the suggestion naturally arises whether this is a condition of things which is desirable to perpetuate. The whole subject of the mode of appointments to these Government offices is one well worthy of the consideration of the Royal Science Commission. Mr. Bennett entered the Museum in 1829 as assistant, and succeeded the late Robert Brown as Keeper at his death in 1858. Mr. W. Carruthers, the present senior assistant, on whom the appointment would naturally fall, and who is so well known for his researches in vegetable palæontology, entered on that office in 1859.

The first course of Cantor Lectures of the Society of Arts for the ensuing Session will be "On Artists' Colours and Pigments," by Frederick S. Barff, M.A., F.C.S., and Fellow of the Cambridge Philosophical Society. It will consist of five lectures, to be delivered on Monday evenings, the 21st and 28th November, and the 5th, 12th, and 19th December, at eight o'clock. These lectures will treat of—the Nature of Colour; Chemistry and Manufacture of Colours and Pigments; Vehicles and Media used in Painting; Fresco and Silicious Painting; Destructive Influences on Colours, &c. Other courses of lectures are under arrangement for delivery during the Session. These lectures are open to members, each of whom has the privilege of introducing two friends to each lecture.

The *Gardener's Chronicle* states that an interesting exhibition of fruit has been recently opened at Appenzell, Switzerland. Eight communes have furnished 689 exhibits, comprising 80 sorts of apples and 120 of pears. The fruits are arranged according to the height above the sea of the localities where they are grown. Thus, in the lowest zone are shown fruits which have been produced from 1,300 to 2,000 feet above the sea; in the next, those grown at an elevation of 2,000 to 2,600 feet; in the third group, those gathered at a height of 2,600 to 3,000 feet; and lastly, are exhibited fruits produced above the last-mentioned elevation. Great care has been taken to ensure the accurate nomenclature of the fruits exhibited.

A remarkably low wave of temperature passed over these islands in the middle of this month. At Blackheath the mean temperature for the week ending Nov. 16 was nearly 7° below the average. It is remarkable that the wind was in the W.S.W. during nearly the whole of the week, the air being almost saturated with moisture, and yet the rainfall scarcely appreciable, 0·04 in. For the fourteen stations in England, eight in Scotland, and one in Ireland, recorded by Mr. Glaisher in the *Gardener's Chronicle*, the lowest minimum was 19·0° at Paisley, the highest, 31·5°, at Norwich. The mean temperature was nearly the same in Scotland as in England, about 37·5°. Another singular meteorological phenomenon occurred this week in the successive thunderstorms which burst over London from 3 A.M. on Tuesday morning the 22nd to 6 A.M. on Wednesday morning the 23rd. The wind was blowing strongly from the S.W. during the whole time, with occasional violent rain, and the average temperature was about 40° F.

The Address on Medicine at the annual meeting of the British Medical Association will be delivered by Dr. George Johnson, and that in Surgery by Prof. Lister.

The current number of *Fraser's Magazine* contains the first portion of a paper on "Mystic Trees and Flowers," which will interest those who have paid any attention to the subject of Tree-worship, with regard to the origin of which no clear theory has yet been proposed. The writer considers that the religious homage paid to trees "must be referred to a distinct religious phase in the development of races, and to a period later than the ideals and myths with which poets invested them." The legends and superstitions of all countries are brought together, showing the points of convergence of the great religions of ancient races; and the connection of the folk-lore of the present day with its prototypes in all ages and in all nations. Reference is made to the recent researches into the history of the popular tales of different countries, and the whole paper teems with suggestive facts. The principal trees dwelt upon in this instalment are the apple, oak, ash, lime, willow, palm, elder, and juniper.

The *Engineer* states that when the Russian American telegraph is completed the following feat will be possible. A telegram from Alaska for New York, leaving Sitka, say at 6.40 on Monday morning, would be received at Nicoleaf, Siberia, at six minutes past one on Tuesday morning; at St. Petersburgh, Russia, at three minutes past six on Monday evening; at London twenty-two minutes past four on Monday afternoon; and at New York at forty-six minutes past eleven on Monday forenoon. Thus, allowing twenty minutes for each re-transmission, a message may start on the morning of one day, to be received and transmitted the next day, again received and sent on the afternoon of the day it starts, and finally reaches its destination on the forenoon of the first day, the whole taking place in one hour's time.

For the purpose of connecting the Madras Observatory with the midnight and noon guns, the Indian Government has voted 200*l*.

MUSICAL INTERVALS

A BRIEF summary of the remarkable papers on musical intervals, by MM. Cornu and Mercadier, published in the *Comptes Rendus* of February in last year, may perhaps interest those of the readers of NATURE who have not met with the original.

The authors remark, in the first place, that two schemes of musical intervals have been proposed, in which the ratios of the number of vibrations in a given time are as follows :—

	Octave.	Fifth.	Fourth.	Major Third.	Minor Third.	Sixth.	Seventh.
(1)	2	$\frac{3}{2}$	$\frac{4}{3}$	$\frac{3^4}{2^6}$	$\frac{2^5}{3^3}$	$\frac{3^3}{2^4}$	$\frac{3^5}{2^7}$
(2)	2	$\frac{3}{2}$	$\frac{4}{3}$	$\frac{5}{4}$	$\frac{6}{5}$	$\frac{5}{3}$	$\frac{15}{8}$

and the object of the paper is to examine the claims of each for adoption.

On comparing these two systems, it may be observed that the Octave, Fifth, and Fourth are the same in both, and that the other intervals are connected by the following relations :—

$$\frac{3^4}{2^6} = \frac{5}{4} \times \frac{81}{80} \quad \frac{2^5}{3^3} = \frac{6}{5} : \frac{81}{80} \quad \frac{3^3}{2^4} = \frac{5}{3} \times \frac{81}{80} \quad \frac{3^5}{2^7} = \frac{15}{8} \times \frac{81}{80}$$

A third view of the subject, according to which either of the two systems may be adopted indifferently, because they differ only by a "comma" (an interval represented by 81 : 80), may be at once dismissed, since, as a matter of fact, the ear is capable of appreciating intervals much smaller than the comma.

As regards the scheme (2), it seems impossible not to admit that a major third is most harmonious when the resultant tone is exactly the double octave below the fundamental note, *i.e.* when the interval is represented by the ration 5 : 4. Any deviation from this proportion produces unpleasant beats. This argument seems decisive in favour of (2).

On the other hand, an ear which hears successively the notes emitted by an entire string, and by $\frac{4}{5}$ths of its length, will pronounce the third so formed to be too low. And, in fact, stringed instruments for concerted music are tuned by perfect fifths. Thus the intervals given by the violoncello, alto, and violins, &c., in a concerto would be *c*, *g*, *d'*, *d*, *e''*.

But this involves a third (*c*, *e*) defined by the ratio $\frac{3^4}{2^6}$; for *e* must be the double octave below *e''*, and the interval (*c*, *e''*) is by hypothesis $\left(\frac{3}{2}\right)^4$; hence

$$(c, e) = \frac{1}{2^2}\left(\frac{3}{4}\right)^4 = \frac{3^4}{2^6}$$

which is the ratio for a major third according to scheme (1).

Thus, experiment apparently gives contradictory results.

The authors of the paper then proceed to describe a series of experiments made with the voice, violoncello, violin, organ-pipes, and monochord, all of which lead to conclusions reconciling this apparent contradiction, viz. :—

That musical intervals do not belong to any single scheme, but that the ear is capable of distinguishing between the intervals of the two schemes in question, and requires,

(*a*) When notes are heard in succession, forming what is called melody, that the intervals should belong to a series of fifths, in accordance with scheme (1) ;

(*b*) When notes are heard simultaneously, forming chords, or harmony, that the intervals should be adjusted according to scheme (1).

The details of the experiments, which are well worth study, would be too long to give in *extenso*; but the subjoined table will enable the reader to form a judgment of the results.

Notes produced by	Major Third		Fifth	
	Harmony	Melody.	Harmony	Melody.
Voice	—	1.260	—	1.497
Violoncello	1.251	1.266	1.499	1.508
Violin	1.249	1.264	1.504	1.504
Organ pipes	1.252	1.267	1.493	1.497
Monochord	—	1.271	—	1.500
Mean of observation	1.251	1.266	1.499	1.501
Calculation	$\frac{5}{4}$ = 1.250	$\frac{81}{64}$ = 1.2656	$\frac{3}{2}$ = 1.500	1.500

The direct experiments were made with these two intervals only ; but the same conclusions are shown to apply to the other intervals.

The authors then proceed to inquire whether there is any reason for limiting the prime numbers which enter into the ratios of the harmonic intervals to those (1, 2, 3, 5) actually occurring in scheme (1). An answer to this question is found in the chord of the dominant seventh, usually defined as the common chord with the addition of a minor third (*e. g.* Do, Mi, Sol, Si♭). The ratios of these intervals, according to scheme (2) are

$$1 : \frac{5}{4} : \frac{3}{2} : \frac{3}{2} \times \frac{6}{5} = 20 : 25 : 30 : 36$$

and the simplest whole numbers near to these are 4 : 5 : 6 : 7 ; and these, it is argued, are in fact an improvement on the former. For the ear, which alone can decide the question, will choose those notes which will form a chord devoid of beats, and whose difference tones do not introduce any notes foreign to the chord itself. Now

$$\begin{gathered} 7-6=1,\ 6-5=1,\ 5-4=1 \\ 7-5=2\ \ 6-4=2 \\ 7-4=3 \end{gathered}$$

So that from the chord 4 : 5 : 6 : 7, we obtain the group of difference tones 1, 2, 3, all of which belong to the natural series terminating with the chord itself. While from the chord 20 : 25 : 30 : 36 = 4 : 5 : 6 : 7·142. we at once derive an inharmonious difference tone. This *a priori* conclusion may be verified on the violin, by first tuning the two upper strings in unison ; then by shortening one of them so as to form a minor third (6 : 5) with a difference tone 1; and finally, shortening the other until the difference tone 1 is again heard. This will, of course, give an interval (7 : 6) perfectly agreeable when sounded simultaneously, but not so when sounded in succession. In the same way the ear might be called upon to decide whether the numbers 11, 13, &c., are or are not admissible in harmony.

I trust that this very brief abstract may induce some of your readers to examine the paper itself.

W. SPOTTISWOODE

ON THE GREAT MOVEMENTS OF THE ATMOSPHERE *

MY original paper was based on the mean monthly pressures calculated for 516 places, and on the mean monthly direction of the wind calculated for 203 places over the whole surface of the earth. From these mean pressures the monthly isobars were drawn for every tenth of an inch, a pressure of thirty inches and upwards being represented on the charts by red-coloured isobars, and pressures of 29·9 inches and less by isobars coloured blue. Thus the distribution of the mass of the earth's atmosphere from month to month was graphically represented, the red lines showing where there was an excess, and the blue lines where there was a deficiency. The general results are that, in each hemisphere, pressures are highest in winter, and lowest in summer; that in winter the highest pressures are grouped over the continents, and in summer the lowest, and that in winter very low pressures prevail in the northern parts of the Atlantic and Pacific oceans respectively. The position of the isobars is wholly determined by the relative distribution of land and water.

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* This paper, presented to the recent meeting of the "British Association" at Liverpool, is a brief *résumé* of a paper, and the discussion which followed, "On the Mean Pressure of the Atmosphere, and the Prevailing Winds over the Globe for the Months and for the Year," originally read before the Royal Society of Edinburgh, and published in the Transactions of that Society in the beginning of December last.

As regards wind, those places were selected which are favourably situated for observing its direction. In calculating the average direction, the element of force was wholly excluded. The years were, so far as possible, the same as those for which the mean pressure had been calculated; but where this was not possible, care was taken to see that a good average was in every case obtained. The mean direction of the wind for each month is shown in the charts by arrows.

Thus two quite distinct facts were exhibited on the charts, viz. lines showing the mean pressure of the atmosphere, and arrows showing the prevailing winds at the earth's surface, each being independently arrived at by the summing and averaging of observed facts. What relation is there between these two classes of facts?

1. *As regards regions of Low Pressures:*

In every case where such occur at any season, it is universally seen that the relations of the winds to the isobaric lines is exactly the same that is illustrated in every storm of wind when the winds and pressures are set down in synophic charts.—The relation is this; the wind neither blows round the centre of least pressure in circles, nor as tangents to the concentric isobaric curves, nor does it blow directly towards that centre, but it takes an intermediate direction, approaching however more nearly to the direction and course of the circular curves than of the radii to the centre. The angle is not a right angle but from about 60° to 80°. Thus the whole system of winds seem to blow vorticosely in upon the spaces of low pressure. This is the relation known as *Buy's Ballot Law of the Winds.*

2. *As regards regions of High Pressures:*

In all cases where such occur, the winds are seen to flow out in every direction. In these cases also, the behaviour of the winds differs in no respect from what occurs on particular days on which the isobaric lines present the same conditions of pressure. The winds flow out of these spaces of high pressure in courses exactly the opposite to what takes place when they flow in upon spaces of low pressure, and hence such meteorological phenomena have been called by Mr. Francis Galton, "Anticyclones." This is also, it will be seen, in strict accordance with Buy's Ballot Law. Hence then this broad result is arrived at, viz., that the prevailing winds over the globe at all seasons obey Buy's Ballot Law, with reference to the distribution of atmospheric pressure.

The *outflow* of the air from a region of high pressure, and the *inflow* upon a region of high pressure is reducible to the single principle of gravitation, and so marked is this, that if there be any other force or forces which put the winds in motion, they must be altogether insignificant as compared with gravitation.

The annual averages of the 115 places distributed over the north temperate zone were minutely examined with the view of ascertaining how far the commonly-alleged prevalence of equatorial and polar currents is borne out by observation. The result of the analysis is this:—There are two maximum directions of prevailing winds at the stations of which the S.W. and N.E. at Greenwich may be taken as an example. The chief prevailing winds of the north temperate zone blow from some point from S.S.W. to W. (the true equatorial direction) at 41 per cent. of the stations, leaving 59 per cent. at which they are from other points of the compass. And the secondary prevailing winds come from some point from N.N.E. to E. (the true polar direction) at 34 per cent. of the stations or only a third of the whole. Hence whilst the largest percentage of prevailing winds are in the direction in which truly equatorial and polar winds should blow, the percentages from the direction are so large as to preclude the supposition of a general flow of the surface winds of the temperate zone towards and from the polar regions. The truth is, it is not the poles of the earth, but the regions of high and low pressure which must be regarded as the true poles of the winds towards which and from which the great movements of the atmosphere proceed; and owing to the unequal distribution of land and water, the poles of pressure and movements of the atmosphere are, as in the case of the poles of temperature, very far from being coincident with the North Pole.

The causes which bring about an unequal distribution of the mass of the earth's atmosphere are chiefly two—viz., the temperature principally, and secondarily the moisture of the atmosphere, their relations to the geographical distribution of land and water. The charts show that where there is an excess of temperature, as in the interior of Asia in summer, and where there is a relative excess of moisture, as in the belt of calms and in the north of the Atlantic in winter, and where there is an excess of cold, as in the interior of Asia in winter, and where there is a deficiency of moisture—there atmospheric pressure is high. Hence where pressures are low there the air is specifically light, and where pressures are high there the air is heavy relatively to that of surrounding regions.

Further, since vast columns of air are poured by the prevailing winds into those regions where pressures are low without increasing that pressure, we must suppose that there is an outflow from these regions through upper currents, and this inference is all the more inevitable, seeing that the specifically light air resting over these regions supplies the conditions of an ascending current. Again, since vast volumes of air are driven off from the regions of high pressure without diminishing the pressure, it must be inferred that the high pressure is maintained by accessions poured in upon it by the upper currents, and this inference is the more certain because in such regions the air is relatively heavier than in surrounding regions, thus supplying the conditions of a descending current.

It is evident that the currents from the regions of low pressure will continue to ascend as long as their pressure is less than that in regions surrounding them *at the same height*, but that as soon as they reach a height where the pressure is less, towards and over that region will they flow as upper currents of the atmosphere. The courses of these upper currents will be directed towards those regions where the air is coldest and driest near the surface of the earth, because being thereby densest, the great mass of the air will condense in the lower beds, thus leaving less air or a diminished pressure in the upper regions. Thus the high winter pressure of Asia will be maintained by air being poured upon it by upper currents from the regions of low pressure in the north of the Atlantic, the north of the Pacific, and the Equatorial regions of the south.

From these considerations the following *Theory of the Movements of the Earth's Atmosphere* necessarily follows. The winds on the surface of the earth are known from the isobaric lines, the direction being from regions of high towards regions of low pressures, subject to the changes produced in the direction of the currents by the earth's rotation; and the upper currents of the atmosphere may be inferred from the isobaric lines taken reversely together with the isothermal lines taken directly. In other words, the regions of lowest pressure, by giving the ascending currents, point out the sources or fountains whence the upper currents flow; and the isothermal lines, by showing where, on account of the low temperature, the greatest portion of the air is condensed in the lower beds and so diminishing the pressure in the upper beds, point out the regions towards and over which the upper currents diffuse themselves.

In this discussion of the prevailing winds and mean atmospheric pressure over the globe, there is contained the first approximate answer to the two questions:—

1. What are the motions of the earth's atmosphere?
2. What are the causes of these motions?

It has been shown by Dr. Balfour Stewart* that these questions form the first two stages in the development of meteorology. Considering the importance of the subjects of this discussion which enter deeply into Physical Geography, Geology, the Science of Navigation, and the General Physics of the globe, it is to be hoped that observation will be made and turned to account in rectifying the isobaric lines and filling up the blanks of the winds over the ocean, and portions of Africa and South America, data from these regions being in this as well as every similar discussion to a large extent wanting.

ALEX. BUCHAN

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, November 15.—Prof. W. H. Flower F.R.S., V.P., in the chair. An eighth letter was read from Mr. W. H. Hudson, containing further observations on the Ornithology of Buenos Ayres.—Mr. Sclater exhibited a specimen of the new Australian Mudfish, recently described by Mr. Krefft in the Society's "Proceedings" as *Ceratodus forsteri*. This specimen had been obtained from the Mary River, Queensland, and forwarded to Mr. Sclater by Mr. E. P. Ramsay.—Dr. J. Murie read a memoir on the form and structure of the Manatee (*Manatus americanus*), as deduced from a fresh specimen of this animal forwarded to this Society in a living state by Mr. G. W. Latimer, of Porto Rico, in April 1866, but which had unfortunately died just before reaching Southampton.—A communication was read from Mr. Morton Allport, relating to the progress of the experiments for introducing Salmon and Trout into Tasmania.—Professor Flower read a memoir on the anatomy of the Panda (*Ailurus fulgens*), as deduced from a specimen of this animal which had been presented to the Society by Dr. Simpson, in May 1869, and had lived for some time in the Society's Gardens. After an elaborate examination of every part of this animal, Professor Flower came to the conclusion that it belonged to the Arctoidean group of the Carnivores, and was most nearly allied to the Raccoons and other members of the family *Procyonidæ*. Mr. Bartlett read some notes chiefly on the habits of the same animal, as observed when living in the Society's Gardens.—A communication was read from Dr. J. E. Gray, containing the outlines of a new arrangement of the Delphinoid Whales.—A communication was read from Mr. W. Harper Pease, containing remarks on the Mollusks of the genus *Triphoris* and descriptions of some new species.—Mr. A. G. Butler communicated a notice of some abnormalities observed in the neuration of the hind wings in *Acræa andromacha*.—A paper was read by Dr. A. Günther containing descriptions of two new species of Lizards of the genera *Eumeces* and *Calotes*, proposed to be called *E. brachydactylus* (from Pegu) and *C. jerdoni* (from the Khasya hills).—A paper was read by Messrs. Sclater and Salvin on the recent collections of Venezuelan birds made by Mr. A. Goering in the vicinity of Merida. The present collection was stated to embrace examples of 105 species, nine of which were considered to be new to Science. Amongst the latter were two new Parrots, proposed to be called *Urochroma dilectissima* and *Conurus rhodocephalus*.—A communication was read from Mr. H. Adams, containing descriptions of 27 new species of Shells collected by Mr. R. McAndrew in the Red Sea. A second communication from Mr. Adams contained descriptions of a new genus and four new species of Shells from Borneo and other localities.

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* See NATURE of December 2, 1869, p. 129.

Anthropological Society of London, November 15.—Dr. Charnock, V.P., in the chair; H. R. Adam, and David Kinloch, were elected fellows. Dr. R. H. Bakewell read a paper on "The Condition of the Blood-corpuscles in certain Races." The author's researches had been undertaken at the request of Dr. Barnard Davis, F.R.S. While investigating the pathology of malarious fevers, the writer made numerous microscopical examinations of the blood, both of the sick and the healthy, in Trinidad. On comparison of the various races of English, French, Portuguese, Italians, Germans, Indians, Chinese, Africans (both indigenous and of West Indian birth), and Creoles of various breeds, he found that, besides the differences produced by disease, there were well-marked differences between the various races. The corpuscles of the flesh-eating Mussulman and the vegetable-feeding Hindoo were contrasted, and it was found that the Hindoo's blood contained a much larger number of white corpuscles, the red corpuscles also differing in form from those of the Mussulman. The author coined the word "nummulation" for the phenomenon observed in the aggregation of the corpuscles like *rouleaux* of coin. He gave the results of the examination of the blood of about a hundred different persons.—Mr. C. Staniland Wake, Director A.S.L., then read a paper on "Tribal Affinities among the Aborigines of Australia." After tracing the distribution of the several forms of native habitations, canoes, and weapons, and also of certain burial customs and initiatory rites, and referring to the apparently universal practice of cannibalism and blood-revenge, the writer stated, firstly, that the phenomena presented by the generality of the western natives are, on the whole, of a more simple character than those exhibited by the other aborigines agreeing with the milder disposition they apparently possess; secondly, that the southern and eastern natives agree generally in their customs with the aborigines of the western part of the continent, but that they present certain peculiarities which seem to suggest an internal influence. This influence must have proceeded from the north, and there the very customs of phenomena which constitute the differentia between the natives of the west and those of the east are met with. Comparing the distribution of initiatory rites with that of other customs, it would seem that scarifying the flesh and nose-perforation are, like the use of the semi-circular hut, primitive customs at one time common to all the aborigines; that the custom of removing a front tooth is almost limited to the canoe-making peoples of the north, south, or east, who also possess in common certain funeral rites unknown to the western natives; whilst circumcision is limited to the northern and southern tribes and to some intermediate ones. The final conclusion of the paper was, that the western natives represent the most primitive and simple form of the Australian stock, the natives of the south and east having been intermixed with fresh comers from the north, who introduced new customs. This southward movement was in two directions—one across the continent to the head of the Great Bight, thence spreading east and west along the coast; the other along the north and eastern shores, and gradually spreading over the eastern portion of the continent.—Dr. Robert Peel contributed a communication on Australian Aborigines and Half-Castes, and exhibited skulls (which he had presented to the Society's Museum) in illustration.

Chemical Society, November 17.—Prof. Williamson, F.R.S., President, in the chair. The following papers were read:—"Mineralogical Notices," by Prof. Maskelyne and Dr. Flight. The contents of this communication were—1. On the formation of basic cupric sulphate. In 1867 M. Pisani described a mineral which he supposed to be the Woodwardite of Mr. Church. The substance, however, is not the latter mineral. It had previously been examined in the Laboratory of the British Museum, and the results sufficiently tallied with those of M. Pisani to identify the mineral. The only interest this mineral offers is in the light it seems to throw on the possible modes of the formation of native basic cupric sulphates. The actions of solutions of magnesium or calcium sulphate on malachite may terminate in the production of langite. An experiment in the laboratory showed that an insoluble cupric sulphate and acid magnesium carbonate were actually formed. 2. Opal from Waddela Plain, Abyssinia. Mr. Markham presented to the British Museum some remarkable specimens of green opal from the above locality. Its analysis showed it to consist of about 92 per cent. of silica, 6 per cent. of water, and the remainder was iron, manganese, calcium, and magnesium. 3. Francolite, Cornwall. Its analytical numbers point to the formula

$$\left.\begin{matrix} 5\,[3\,\mathrm{Ca},\ 2\,(\mathrm{P\,O_4})] \\ \mathrm{Ca\,CO_3} \end{matrix}\right\} + 2\,\mathrm{Ca\,F_2}$$

It is, in fact, a fluor-apatite, in which one equivalent in every six of the calcium phosphate is replaced by carbonate. The crystallography of this mineral seems also to point to its not being ordinary apatite, and in fact to its not being the same mineral as the original francolite from Wheal Franco. 4. Epidote and serpentine, Iona. A pebble in which a green mineral traverses bright red felspar and quartz in veins was sent by the Duke of Argyll to the British Museum. Its analysis leads to the view that it consists of a lime epidote with some 23 per cent. of quartz, the former mineral having the following constitution:

$$3\,[2\,\mathrm{Ca\,O},\ \mathrm{Si\,O_2}] + 2\,[2\,(\tfrac{3}{5}\mathrm{Al}\ \ \tfrac{2}{5}\,\mathrm{Fe})_2\,\mathrm{O_3},\ 3\,\mathrm{Si\,O_2}]$$

Two specimens of serpentine from the same locality gave the general formula:

$$3\,\mathrm{R''\,O},\ 2\,\mathrm{Si\,O_2},\ 2\,\mathrm{H_2\,O},$$

where R represents in one case Mg, with a little Fe and Ca; in the other Mg, with nearly one-fifth of its equivalent of an equal mixture of Fe and Mg. 5. Vivianite. Two kinds of this mineral were found in an unknown Cornish locality. The one is of a pale, bluish tint, the other of a brownish colour. Both proved to be octahydrated ferrous orthophosphate, $3\,\mathrm{Fe\,O},\ \mathrm{P_2\,O_5},\ 8\,\mathrm{H_2\,O}$, and the difference in the colours can only be ascribed to some minute difference in the degree of oxidation of the iron. 6. Cronstedtite. The analysis of this mineral presented considerable difficulties, inasmuch as it was extremely difficult to free it from the substances with which it is found associated. The cronstedtite in question possesses an unusual interest from a crystallographical point of view, being one of the best defined types of hemimorphism. 7. Pholerite. This mineral, derived from India, is of a pale flesh white, penetrated in several places by patches and veins of a black mineral. A new name was proposed for this mineral, but the analysis shows it to be nothing but pholerite. Mr. Church observed that it was a matter of congratulation to have those beautiful specimens which are stored in our magnificent national collections investigated in so excellent a manner as the contents of the paper just read have shown.—The next communication was a note by Mr. Chapman "On the Oxides of Nitrogen." In a paper read at the last meeting of the Chemical Society, Mr. Chapman mentioned that he had quantitatively estimated nitric oxide by converting it into nitric acid, and determining the latter by the production and weighing of the baryta salt. Objections were then raised as to the possibility of the completeness of such a conversion. Mr. Chapman now endeavoured to show by referring to well-known chemical facts that whether N_2O_5, N_2O_4, or N_2O_3 be formed when NO is left with excess of oxygen over water, the final result must be the transformation into nitric acid. Mr. Harcourt reasserted that on his passing nitric oxide into oxygen he obtained as result nitric peroxide; when reversing this order and passing oxygen into nitric oxide, a mixture of N_2O_4 and N_2O_3 seems to be formed. Mr. Chapman replied that the different results obtained by Mr. Harcourt and by himself were in all probability due to differences of the temperatures at which the respective experiments had been executed. Prof. Williamson took occasion of this repeated mentioning of nitric peroxide to remark that this compound may be viewed as $\left.\begin{matrix}NO\\NO_2\end{matrix}\right\}O$, *i.e.*, as water in which the one hydrogen was replaced by NO, the other by NO_2.

Linnean Society, November 17.—Dr. J. D. Hooker, V.P., in the chair. Notes on the *Passifloreæ*, by Dr. M. T. Masters. The paper treated of the morphology of the whole order, including the organography of all the genera; the minute anatomy, development, mode of fertilisation, and the movements which take place in the stamens and pistils of *Passiflora*; the affinities of the order, together with an inquiry into the value of characters, and the mode of estimating them; a complete list of the genera and species, with detailed descriptions of all the species which are not either American or African, and which will be published elsewhere; and, lastly, an account of the geographical distribution. (1.) Organography.—The tendrils, as shown by their development, minute anatomy and position, and the fact that they occasionally bear flowers, are merely modified flower-stalks. The leaf, whether single or divided, always commences in development at the apex, and proceeds from above downwards. There are only three bracts instead of five, as there would be in a perfectly symmetrical arrangement; their position, as well as that of the other parts of the flower, relative to the axis has been somewhat incorrectly described by Payer, Griffith, and Schleiden. Jussieu and St. Hilaire held that there is no true corolla in the passion-flowers, but two calycine whorls, because both organs fall at the same time. But the mode of development and the internal structure clearly demonstrate that the inner whorl is a true corolla; the calyx is quincuncial, while the petals appear simultaneously. The flower-tube is, according to Bentham, composed of a union of the calyx and corolla; Dr. Masters, on the other hand, believes it to be an expansion of the axis. Its development is comparatively late. The form of the corona was traced from its simplest form in *Turnera* to its most complicated arrangement in some Modeccas and Passifloras, in all cases it is a mere projection from the flower-tube, and is of late development, and morphologically of little importance, though essential to the individual life of the plant. The inner portion of the tube is a glandular secreting surface. Each separate thread of the corona has its own distinct vascular bundle. The stamens, unlike those of *Cucurbitaceæ*, are not perigynous, but truly hypogynous. The gynophore becomes gradually developed, raising the stamens with it. The anthers are invariably two-celled. The pistil is singularly uniform, one-celled, made up of three united carpels, with three parietal placentæ and three stigmas. (2.) Mode of Fertilisation.—The arrangement of the sexual organs favours cross-fertilisation. The anthers, originally introrse, become, when fully developed, distinctly extrorse, and it is thus rendered difficult for the pollen to fall on the stigmas of the same flower; it falls on to the rays of the corona, on which insects alight in search of the honey concealed at the base of the tube, and carry the pollen away to other flowers. Some species are more easily fertilised by pollen belonging to a different species than by their own; hence hybridisation abounds. (3.) Affinities.—Dr. Masters connects the *Passifloreæ* rather with the *Turneraceæ*, *Samydaceæ*, *Violaceæ*, and *Sauvagesieæ* than with the *Cucurbitaceæ*, chiefly on account of their truly hypogynous stamens. (4.) Geographical Distribution.—The order is essentially tropical, occurring between 30° N. and 30° S. latitude. Taking the genus *Passiflora* as the type, it is almost exclusively American, and chiefly Brazilian; a few outliers of the true passion-flowers occur in Madagascar and Mauritius, the latter probably introduced, and in North America there is a very remarkable form, the *P. incarnata*, or original passion-flower of the Jesuits; it is an annual, and apparently an alien, but remarkably uniform and invariable; its representative in Brazil is *P. edulis*, a shrub, and an extremely variable plant. On the western side of the Andes you get entirely different forms, especially the *Tacsonias*, which have generally a much longer flower-tube. In India there are a few species, not belonging to the American section of the order, but to the *Polyanthea*, which are found also in Ceylon and the Indian Archipelago, and one outlying species in an island near Hong Kong. In Australia and the Pacific Islands occurs another perfectly distinct group; in New Zealand a perfectly distinct form is met with; and, again, another in Africa, where no true passion-flower is indigenous, except the one in Madagascar; the species here, few in number, belonging to six or eight distinct genera.—"On the White-beaked Bottle-nosed Whale," by Dr. Jas. Murie. The paper gave an account of several anatomical points in the structure of this whale, which had not been clearly described before. The little bag or sac connected with the double articulation between the lower jaw and the rest of the skull, which is found in all Mammals except the Cetacea, is however, present in the fœtus of the latter.

Mathematical Society, Nov. 10.—Prof. Cayley, President, in the chair. The following gentlemen were elected to form the council for the Session 1870–1:—President, W. Spottiswoode, F.R.S.; Vice-Presidents, Professors Cayley, Henrici, and H. J. S. Smith; Treasurer, Dr. Hirst; Hon. Secs., M. Jenkins and R. Tucker; other Members, W. K. Clifford, T. Cotterill, M. W. Crofton, *C. W. Merrifield, *J. F. Moulton, J. Stirling, Archibald Smith, Prof. Sylvester, J. J. Walker. Mr. J. J. Hamblin Smith, M.A., Caius College, Cambridge, was proposed for election. The new president having taken the chair, called on Prof. Cayley to read his paper, of which an abstract is given below—"Sketch of Recent Researches upon Quartic and Quintic Surfaces." The classification of quartic surfaces is, even as to its highest divisions, incomplete; and it is by no means easy to make it at once exhaustive and precise; an examination of all the *prima facie* possible cases would include forms which do not really exist. Premising that the expression "singular" means double or cuspidal, or refers to a higher singularity; "nodal" in general double (including the signification cuspidal), we may provisionally arrange the non-scrolar quartic surfaces as follows—1, without a nodal curve; 2, with a nodal line; 3, with a nodal conic, or line-pair; 4, with three nodal lines (not in the same plane) meeting in a point); 5, the quartic scrolls, omitting altogether the torse and cones. For the scrolls the division into twelve species appears to be complete (Memoirs by Cayley (3) (4), Cremona, and Schwarz). As regards non-scrolar surfaces, we have—1. Without a singular curve. The surface may be without a cnicnode (conical point), or it may have any number of cnicnodes up to 16 (Cayley, (5); Kummer (1) (2),). It may be remarked that the wave-surface, or generally the surface obtained by the homographic deformation of the wave-surface, called by Cayley (1) (2) the "tetrahedroid," is a special form of surface with 16 nodes. 2. Quartic surface with nodal line. Considered incidentally in *Clebsch* (2) (3). 3. Quartic surfaces with nodal conic. Such a surface may be without cnicnodes, or it may have one, two, three, or four cnicnodes. The cases other than that of three cnicnodes are mentioned, *Kummer* (1); but the question is examined and the remaining case of three cnicnodes established, *Cayley* (6). The general case of the nodal conic without cnicnodes is elaborately considered, *Clebsch* (1). See also *Geiser*; the several cases of one, two, three, and four cnicnodes are considered, *Korndorfer*. In the case where the nodal conic is the circle at infinity the surfaces have been termed "anallagmatic" (perhaps "bicircular" would be a more convenient name), and a great deal has been written on these surfaces by Moutard, Clifford, and others. The theory of the quartic surfaces with a cuspidal conic has been hardly at all considered, briefly referred to in *Cayley* (6), also in *Cayley* (8). "I do not know that anything has been done in regard to the quartic surfaces where the nodal conic becomes a line-pair, that is, where we have two intersecting nodal lines." 4. Quartic surface with three nodal lines (not in the same plane) meeting in a point. This is in fact *Steiner's* quartic surface, and it has been the subject of numerous investigations. The author then proceeded to speak of the paper, *Kummer* (1), which discusses the cases in which a quartic surface has upon it a system of conics; or what is the same thing, in what cases there is a system of planes each intersecting the surface in two conics. It is, in the first place, remarked that there is no proper quartic surface cut by every plane in a pair of conics or even a proper quartic surface cut in a pair of conics by every plane through a fixed point. The cases considered are :—(I.) where the planes are non-tangent planes; (II) where they are single tangent planes; and (III.) where they are double tangent planes (*Steiner's* surface, where every tangent plane meets the surface in a pair of conics, comes under II.). It was then pointed out that several of the papers by *Clebsch* and others, refer in their titles to the "Abbildung" of a surface, viz. they show that a (1,1) correspondence exists between the points of a surface and the points of a plane. For surfaces of the higher orders it is only certain surfaces which admit of an "Abbildung," or (1,1) correspondence of the points thereof with the points of a plane; viz. a surface, n order that it may thus correspond with the plane, (or say in order that it may be unicursal), must have a sufficient singularity n the way of a nodal or cuspidal curve. In the memoir *Clebsch* 3), after explaining the above method of the transformation of a cubic surface by means of two of the lines thereof, the author goes on to notice that the like method is applicable to certain quartic and quintic surfaces, viz. (1) quartic surfaces with a nodal conic; (2) quartic surfaces with a nodal line; (3) quintic surfaces having a nodal skew cubic (the nodal skew cubic may break up into a conic and line which meets it, or into three ines, two of them not meeting each other, but each met by the third line; and the like theory applies to these quintic urfaces). The memoir by *Korndorfer* relates to the "abbildung" of a quartic surface having a nodal conic, and one, two, three, or four cnicnodes. *Clebsch* (4) relates to the "abbildung" of a quartic croll. "As regards quintic surfaces (not being scrolls), we have, so far as I am aware, only the paper *Clebsch* (3) relating to quintic urfaces with a nodel Skew Cubic; the paper *Clebsch* (5), which elates to the 'abbildung' of a quintic surface having a nodal quadriquadric. It only remains to speak of *Schwarz's* memoirs on Quintic Scrolls: it is to be remarked that the theory of scrolls s allied more closely with that of plane curves than with that of urfaces, viz. considering any plane section of the scroll the lines f the scroll have in general a (1,1) correspondence with the points of the plane section, and the scrolls of any given order are properly rranged according to the deficiency of the plane section. This is what is done in the memoirs by *Cremona*, and this is the principle f classification in *Schwarz's* memoir. A model of Steimer's surface was exhibited, and many of its properties pointed out. The following list of memoirs will indicate the sources whence the ketch was principally drawn. Memoirs by the author: (1) Sur a Surface des Ondes. Liouville, tom. xi. 1846. (2) Sur un cas articulier de la Surface du quatrième ordre avec seize points singuliers. Crelle, tom. lxv. pp. 284—290. (3.) Second memoir on Skew Surfaces otherwise Scrolls. Phil. Trans. vol. liv. (1864), pp. 559—577. (4.) Third memoir. Phil. Trans. vol. clix. (1869), pp. 111—126. (5.) A memoir on Quartic urfaces. Proc. of London Math. Society (1870), vol. iii. pp. 19—69. (6.) On the Quartic Surfaces $(\ast \;\ast, U, V, W)^2=0$, Quarterly Journal of Math. tom. x. (1868), pp. 24—34. (7) Do. om. xi. pp. 15—25 (1870). (8.) Memoir on Cubic Surfaces. hil. Trans. vol. clix. (1869), pp. 231—326. *Cremona:* Sulle uperficie gobbe di quarto grado. Mem di Bologna, tom. iii. (1868). *Schwarz:* Ueber die geradlinigen Flächen inften Grades. Crelle, tom. lxvii. (1867), pp. 23—57. *Clebsch* 1) Ueber die Flächen vierter Ordnung welche eine Doppelcurve weiten Grades besitzen. Crelle, tom. lxix. (2.) Intorno lla rappresentazione di superficie algebriche sopra un piano. tti di R. Ist. Lomb. (1868), 13 pages. (3.) Ueber die bbildung algebraischer Flachen insbesondere der vierten und funften Ordnung. Ann. Clebsch und Neumann, vol. i. 868), pp. 253—316. (4.) Ueber die ebene Abbildung der eradlinigen Flachen vierter Ordnung welche eine Doppelcurve dritten Grades besitzen. Ann. Clebsch und Neumann (1870), pp. 445—466. (5.) Ueber die Abbildung einer Classe von Flachen, Ordnung. Gott. abh. tom. xv. 64 pages. *Geisser:* Ueber die Flachen vierten Grades welche eine Doppelcurve weiten rades haben. Crelle, tom. lxx. (1868), pp. 249—257. *Kornorfer:* Ueber die ebene Abbildung Clebsch und Neumann, tom. ii. *Kummer:* (1) Ueber die Flächen vierten Grades auf welchen Schaaren von Kegelschnitten liegen. Berl. Monatsber. Jul. 863. Crelle, tom. lxiv. (1864), pp. 66—76. (2) Ueber die gebräischen Strahlensysteme ins besondere uber die der ersten und zweiten Ordnung. Berl. abh. (1866), pp. 1—120. (3.) (Surfaces of the 4th order with sixteen conical points). Berl. Monatsber. (1864), pp. 246—260, and 495—499.

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* These gentlemen were not on the Council for the Session 1869–70.

Meteorological Society, November 16.—Mr. C. V. Walker, President, in the chair. Mr. J. H. Gilbert, Mr. C. R. Marten, Mr. F. E. Sawyer, and Mr. T. H. Wilson were elected Fellows. A paper was read by Mr. G. Dines, "On Evaporation and Evaporation Gauges, with some remarks upon the Formation of Dew," in which, after referring to Dr. Dalton's investigations, he explained the experiments in which he has been engaged during the past eighteen months with gauges of different sizes, the experiments being made sometimes with water of ordinary temperature, and at other times with heated water and also with water artificially cooled, in the open air and in a closed room. Mr. Dines then gave the results of these experiments, one of which confirmed the statement of Mr. Glaisher in 1847, viz. that "the formation of dew was found to depend solely on the temperature of the bodies upon which it was deposited, and that it never appeared upon them till their temperature had descended below that of the dew-point in their locality." Finally, he stated that the conclusions which he had arrived at were, 1st. The greatest cause of evaporation is the movement of the air. 2nd. Whatever tends to increase the temperature of the air increases evaporation, and *vice versâ*; and 3rd, That which tends to lessen the temperature of the dew-point, increases evaporation, and *vice versâ*. Mr. Glaisher then made a communication respecting the November meteors, giving the results of the watches which were kept during the nights of the 12th-13th, 13th-14th, and 14th-15th. On the latter evening fifty-three meteors were observed. He also made some remarks on the great magnetic disturbances which occurred during the brilliant display of Aurora Borealis on the nights of October 24th and 25th. The President then adjourned the meeting till January 18, 1871.

MANCHESTER

Literary and Philosophical Society, October 4.—Rev. Wm. Gaskell, Vice-president, in the chair. Mr. Boyd Dawkins gave a short account of the work done in the Victoria Cave, near Settle, since the last notice brought before the Society. The two layers containing traces of man were separated at the entrance by a talus of fallen stones, seven feet thick, that gradually coalesced as the excavation passed into the cave, and at last became so confused together as not to be easily distinguished at a few feet from the entrance. The remains of a gigantic bear which had been eaten may probably be assigned to the lower horizon, which furnished flint-flakes, and a bone harpoon in form resembling that used by the natives of Nootka Sound; the upper or Romano-Celtic stratum continued to supply evidence of the comparatively late date of its accumulation in barbarous imitations of coins of Tetricus (A.D. 267-273.) A portion of the ivory handle of a Roman sword and a coin of Trajan have also been found, along with large quantities of the bones of animals that had been used as food. Several spurs of cocks proved that the inhabitants ate the domestic fowl, which was probably imported into this country either directly or indirectly by the Romans. The most striking object, however, is a beautiful sigmoid fibula made of bronze, and ornamented with a beautiful pattern in red, yellow, green, and blue enamel. It is an admirable example of the art of enamelling ("Britannicum opus"?) which the Celtic inhabitants of Britain probably taught their Roman conquerors.

October 18.—Mr. E. W. Binney, President, in the chair. Prof. Balfour Stewart, F.R.S., exhibited a series of sun-spot curves projected from results obtained by himself and Mr. De La Rue, from observations of Schwabe, Carrington, and the Kew series of photographs of the sun. These extend over a term of about forty years, and exhibit a principal and secondary maximum and minimum in each solar spot period of eleven years, thus corresponding with the light curves of R Sagittæ observed by Mr. Baxendell, and β Lyræ by Prof. Argelander. Hence it may possibly be that notwithstanding the darkening of the sun's surface during the maximum spot period, the total light and heat emitted by the sun at this period is really greater than at the times of minimum spot frequency.—Mr. Lockyer, F.R.S., gave an account of his recent spectroscopic investigations of the solar atmosphere, and pointed out that the conclusions arrived at by De La Rue, Stewart, and Loewy, confirmed the views to which he himself had been led by spectroscopic observations of the sun during the last two or three years. These tended to show that the absorbing atmosphere, termed the chromosphere, which he had proved to exist round the sun's body, had gradually diminished in thickness since the last solar spot minimum in 1867.—Mr. Boyd Dawkins, F.R.S., gave a short account of the examination of Offa's Dyke made in the autumn by Col. Lane Fox and himself. The portion examined extended from Cherbury in the south to the abrupt range of limestone hills to the north of Llanamynech. At Nantcribba Hall, near Forden, the dyke passes nearly due north between the road to Montgomery and the abrupt boss of volcanic trap which looks at a short distance like a ruined castle, and which has been encircled by a very broad and deep moat. There can be no doubt but that this was a point of observation, and as it is but some twenty yards on the English side of the dyke, it was most probably one of the permanent positions occupied by the English followers of the Mercian King From this point the dyke gradually swerves to the east from the road between Montgomery and Buttington, and makes directly over the low slopes of the hills, in some places being nearly ploughed down, and in others, and especially in the small valleys, being of considerable height and resembling a railway embankment, until it reaches the higher ground of Fron. Thence it runs through Pentre and gradually approaches the road, and finally dies away in the alluvium of the Severn, nearly a quarter of a mile to the south of Buttington Church. The commanding camp to the south of this portion of the line is Caer Digol, or the Beacon Ring, on the top of the Long Mountain. The morass, which in Offa's time must have extended between the main ditch and the Severn, prevented the necessity of any bank being made between Buttington and the Cefn. Where, however, the open country demands a defence to the north of Cefn, an embankment makes straight for the Greenstone ridge of the Garreg, and is very plainly seen close to the farm-house of that name, near the Trewern Gate. Here we lost our clue, and it is very likely that the steep ridges of Moel y Golfa, and the marvellously strong camps of the Breiddan and Middleton Hills, formed a sufficiently strong barrier without any dyke being raised. We picked it up again, however, on the western or Welsh side of the Severn, from which it runs, as shown in the Ordnance map, due north to the four crosses, where it joins the Oswestry road, and where it is cut across by the new railway. There it makes straight for the fortified hill of Llanamynech, its line coinciding with the high road. On reaching the summit of Llanamynech it takes the western or Welsh side of the two large camps, and passes down into the valley to the south of Whitehaven, which was the limit of our expedition. The results of our examination are the direct proof that the dyke was made for military purposes, and that it took the line which was best adapted for repelling the incursions of the Welsh. Throughout the district which was examined the embankment faces Wales, and was therefore made to defend the country within it from the Welsh. Dr. Wright's view, therefore, that it was a mere geographical boundary to prevent the Welsh from stealing the cattle of the Mercians cannot be maintained, although it may perhaps receive some confirmation from the nursery legend of Taffy. The camps in the neighbourhood of the dyke are probably older than Offa's time. The bronze spears found in Llanamynech imply that the camp is not later than the bronze age, while the Roman coins in that of the Breiddan point to its occupation by the Romans.

November 1.—Rev. William Gaskell, Vice-president, in the chair. Mr. William H. Johnson, Mr. Walter Morris, and Professor Balfour Stewart were elected ordinary members of the Society. Dr. Joule exhibited a series of curves obtained by Dr. Stewart from the self-recording instruments at the Kew Observatory, showing a large amount of disturbance of the magnetic declination and horizontal force during the progress of the aurora of the 25th October. He also showed a curve of the changes which took place in the magnetic dip as observed by himself at Broughton. The most remarkable variation occurred during the interval from $6^h\ 15^m$ to $6^h\ 23^m$ G.M.T., when the dip increased from 69° 8′ to 70° 30′.—"Notes on Glacier Moraines in Cumberland and Westmoreland," by Mr. Brockbank, F.G.S. The author referred to the proceedings of the Geological Society of London for 1840–1, which contain notices of the evidences of glaciers having existed in Great Britain, by Professor Agassiz, Dr. Buckland, and others, and which point out (1) "Moraine-like Masses of Drift," which occur near the junction of the Eamont and Lowther with the Eden, near Penrith; (2) The "large and lofty insulated piles of gravel in the valley of the Kent near Kendal, and the smaller moraines and their detritus which nearly fill the valley from thence to Morecambe Bay;" (3) "Similar mounds near Shap," and (4) the "Gravel mounds near Milnthorpe and thence to Lancaster." Of these the author considered the Kentmere Group, near Kendal, as most nearly fulfilling the conditions required in true glacier moraines, and that in the other cases it admitted of doubt whether they were really due entirely to glacial action. The districts more particularly the subjects of the author's notes are (1) the valleys of Eskdale and the Duddon (which were not visited by Dr. Buckland, but in which he supposed moraines to exist, from the appearances of the valleys as delineated in Fryer's map of Cumberland); (2) the valleys eastwards from Bowfell; and (3) the district of Shap Fells

DIARY

THURSDAY, November 24.

London Institution, at 7 30.—On the Precious Metals and their Distribution: Prof Morris

Royal Society, at 8.30—Note on the Pendulum Observations in India Col. J T Walker, F.R.S.—The Theory of Resonance: Hon J W. Strutt.

Society of Antiquaries, at 8 30—Romano-Celtic Sword exhibited by Lord Wharncliffe: Mr A. W. Franks.—Wall Decorations of the Roman Period in Sectile Work, especially in Glass. Mr A Nesbitt.

SUNDAY, November 27

Sunday Lecture Society, at 3 30—On the Antiquity of Man · Dr Cobbold

MONDAY, November 28.

London Institution, at 4—On Chemical Action: Prof. Odling

Institute of Actuaries, at 7.—On Legislation in reference to Life Insurance and Life Insurance Companies. Mr. T. B Sprague, M.A

TUESDAY, November 29.

Geographical Society, at 8 30

Manchester Literary and Philosophical Society, at 7.

WEDNESDAY, November 30.

Society of Arts, at 8.—On Peat and its Profitable Utilisation · Robert M Alloway

THURSDAY, December 1.

Chemical Society, at 8.—On some Derivatives of Anthracene. Mr. W. H Perkin

Linnean Society, at 8

London Institution, at 7 30.—On Gems and Precious Stones: Prof. Morris

Society of Antiquaries, at 8.30

TUESDAY, December 6.

Anthropological Society, at 8.—On the Races inhabiting the British Isles · Mr A L Lewis—On Archaic Structures of Cornwall and Devon: Mr A L Lewis—On Forms of Ancient Interment in Antrim. Dr Sinclair Holden

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Errata.—Page 28, second column, line 10, for "cumulus" read "nimbus" Page 54, first column, line 25 from bottom, for "Mr. Care" read "Mr. Earl"

THURSDAY, DECEMBER 1, 1870

MEDICAL SCHOOLS IN ENGLAND AND GERMANY.

III.

THERE is something elevating in the thought that the hospital, while it provides care for the sick, at the same time makes them useful for the purposes of instruction in the service of mankind. Still it is painful, on the other hand, to think that the patient enjoys this attention only in order to be made as profitable as possible to others; that he is given over to a crowd of curious seekers for knowledge, who feel his painful spots, percuss and auscultate his weakened body, and, in short, by proceedings of various kinds, disturb the rest he so heartily longs for. Such evils are not indeed of any account, so long as the number of students does not exceed a certain limit. Experience teaches that a patient is pleased to see a certain number of doctors about him; he soon gives them his confidence, he looks upon them as his friends, and willingly allows himself to be examined, partly with the idea that it will be better for him if the examination is several times repeated, partly in acknowledgment of kind attentions which are shown him. The difficulty arises when the number of students is so large that the patient can no longer feel at home with them. Apart from the fact that the disturbance of the patient increases with the increase of the crowd about him, it is also of the utmost importance to consider that the greater number of students must remain total strangers to him, that they are for him only intruders, who are learning from his body without offering him anything in return.

The situation becomes, however, actually distressing in the neighbourhood of a distinguished teacher in one of the large schools. An inquisitive throng crowds about the sick bed, utterly regardless of the patient's comfort. The hospital becomes a market, the sick merchandise.

Inasmuch as it is important for us to give the relations of hospital and school a basis as humane as possible, we must inquire what those large schools have to offer n the way of medical instruction, and whether we may take the advantages as a set-off to the evils.

As far as the art of medicine is concerned, we can maintain, without fear of opposition, that, *cæteris paribus*, clinical medicine will be taught with less success, the greater the number of students who crowd about the bed. The technical part of medicine should be taught orally. The student should be drilled in it as in a trade. In general, however, and to a certain point, the capability of the person so drilled increases in proportion to he time which the teacher devotes to him personally, and it must therefore be in inverse proportion to the number of pupils whom a teacher has to instruct.

Experience, in fact, teaches us that with large numbers of pupils the standard of individual capability is apt to be xtremely low. In Vienna, where about 300 students are nstructed in clinical medicine around each bed, the mount of drill which each receives comes so near zero that for practical life it is scarcely to be taken into account. The students discover this as soon as they have left their studies behind them and begin to look forward to practice. They distribute themselves then among the hospitals where there are no students' tasks, and there, with great loss of time, and under discouragements of many sorts, they are instructed in subjects which, according to their diplomas, they should have learned long before. Between such extreme unsuitableness and a really healthy state of instruction, there is, however, a long road leading us through many schools of various grades.

Instruction in medical science is good only where it happens to be endurable by the patient — that is, in smaller schools, where a few students only collect about his bed. Medical instruction has still another side. The student must not only learn the handicraft, he must also, through well-instructed teachers, be made familiar with the delicate processes which require delicate instruments, and experiments, and methodical thought for their elucidation; for it is only by intercourse with thinking men that he can learn how careful observation and earnest thought on the phenomena with which his whole life will be occupied, can be made available for practical ends.

In this connection, also, the question of the value of instruction in hospital wards must be answered. In point of fact the *practising physician* has always hitherto been regarded as the high school of medical science. Famous systems of medicine—that is, theories upon the nature of disease—took their start in hospital wards, and were taught at the bedside by distinguished masters.

In this country, however, a total revolution has taken place, speculations upon systems of medicine are supplanted by exact medical science, and this, for the moment, has fled from the ward and taken up its abode in the laboratory—here, at the present time, the most distinguished medical men and working pupils of superior gifts attach themselves to the laboratory, and remain there to study. Hence the phenomenon that, in Germany, in the department of pure clinical medicine, there is a want of worthy representatives. We are not to conclude on this account that our generation is inferior; it is only that the distribution of its scholars is different.

The modern students of clinical medicine seek to found their reputation by work in the laboratory, and they are unable to stand competition with those workers who are at home in it. First, because in general their ranks are not recruited by thinkers of the first order; secondly, because their profitable business coming in the way hinders their thorough education. They leave, therefore, the firm ground which the morphology of disease affords, and trust themselves to the weak ship of experimentation, which they are not able to steer.

Even if the results of such investigations are taught at the bedside, the highest school of medical science is not to be sought there. The patient ought not to be disturbed, and the drill of the student neglected, for the sake of a science which is, or ought to be, better studied in the laboratory. It is the morphology of disease which belongs directly to the sick bed. One must have seen distinguished teachers of this kind, one must have observed how they bring to light latent indications of disease, in order to concede that to such men, even a little misusage of the sick might be allowed in the interests of education. This bringing to light and grouping of phenomena for the purposes of diagnosis, is nowadays, indeed, not called science. It is an intellectual act, such as everyone performs when he defines a plant, a mineral, or a commodity. The combination of the symptoms and definition of the disease is, in fact, an important element of the physician's art, and must be included in every course of instruction. Still "Eines schickt sich nicht für Alle."

When Cuvier determined to what primeval animal a bone found buried in the earth must have belonged, it was only a definition; yet no one will hesitate to rank it amongst the noblest exercises of the human intellect. So, too, the diagnoses of distinguished masters may be ranked with the greatest achievements of medical investigators. To forbid such masters to teach at the bedside because many students crowd about them and disturb the patient, would be absurd. The instruction of such masters is, however, fit only for connoisseurs. For the uncultivated taste of the beginner it is unsuited. If it is besides so diluted as, with a large number of students, must invariably be the case, according to an unalterable law, it entirely loses its value. If, then, the founders of hospitals or their administrators would diminish to a certain definite and very low standard the number of students who, at one time, should be allowed to enjoy the privilege of instruction in a hospital ward, they would certainly do no injury to the interests of education. On the contrary, if liberal in other respects, they would, by such limitation, further these interests.

In what shall this liberality consist? Simply in this, that all available space in the hospital shall be open for the purposes of instruction, and that as many teachers shall be admitted as is consistent with a proper classification of the patients. We say *admitted*, and this is really the fittest expression, for there is not the smallest doubt that always and everywhere the ablest men would be eager to accept the place of teacher. The intellectual stimulus which an able physician derives from association with a school and intercourse with young men, and the moral support which a situation as instructor gives him in regard to his patients, are sufficient compensations for the required expenditure of time.

The physicians and surgeons who at present have large amounts of material at their disposal in large hospitals, would certainly not be willing to support such a system of division. They would complain that they are deprived of the possibility of attaining great skill. There is some truth in this objection. It is a benefit to mankind when a surgeon, by extensive practice, attains superior skill in the performance of certain operations. But the advantage which a few derive from his technical skill is counterbalanced by the loss which the rest sustain from the superficial manner in which they are passed over with a mere glance. In such a hospital more things are overlooked than one would imagine; and the pupils profit far less from the skill of the teacher than they lose from the superficiality of the teaching. It is, moreover, untrue that skill is likely to suffer from a further division of material. We have examples of great operators who never occupied a position in a hospital. Nor can it be doubted that the greater the number of those to whom the opportunity is given of proving their skill, the greater will be the number of skilful surgeons.

In the plan here proposed, many readers may see only what is already introduced in England, that is, small schools for the special study of medicine. The writer of this article is, however, far from speaking in behalf of schools of this kind. Such schools are fit only for the production of craftsmen; and medical men, though they must be craftsmen, must not be mere craftsmen. Such small special schools can serve only as preparatory appendages to the larger educational bodies, that is to say, to the Universities, whose function it is to foster the sciences for their own sakes.

Every medical student ought to obtain his education in general physical science and special medical science at the University; and while doing so he must acquire the art of medicine in a hospital, just as the young botanist must study in the field at the same time that he attends lectures.

The small Universities in Germany answer both purposes to an approximate degree. There one finds excellent schools with so few students that they can obtain both the higher scientific education and drill at once. To approach towards such circumstances, without giving up the great advantages which large schools offer for the development of science, is the end towards which we must strive.

S. STRICKER

POLARISATION OF THE CORONA

As this forms one of the most important questions to be settled during the coming Eclipse, it becomes desirable to reconsider the observations already made on this subject. Arago first suggested that the polariscope should be used on the corona, but apparently did no anticipate any decided results. The principal observation since made are the following:—

1842.—Arago at Perpignan. Used a polariscope *lunulis*, that is, a double-image prism and crystal. H found the two images of complementary tints, the colou extending over the sky around the corona, the coron itself, and even over the disc of the moon.

1842.—Mauvais at Perpignan. Used a Savart's polariscope. He saw the bands very distinctly on the corona and faintly on the moon itself. Their maximum of inten sity corresponded with the horizontal position of the band Evidently he should have found another maximum whe the bands were vertical.

Both this and the preceding observation show th existence of atmospheric polarisation extending even ove the disc of the moon. Its plane must have been the sam throughout, or Arago would have seen the different par varying in tint. The maximum noticed by Mauvais show that the plane must have been either vertical or horizonta that is, not oblique.

1851.—Abbadie at Trocdeseckseverk. Inserted a pla of quartz between the object glass and eye-piece of h telescope, and applied a double refracting prism to tl eye-piece as an analysis. He found the light of the coro strongly polarised, but saw no traces of colour on t moon. He was, however, troubled by clouds.

1851.—Dunkin at Christiania. Found no traces polarisation, but was troubled by clouds.

1851.—Carrington at Lilla-Idel. Used a Nicol's pris but found no polarisation.

1858.—Liais at Paranagua. Instrument used, a Savart's polariscope. He found the plane normal to the limb of the sun, and the intensity small, but greater than that of the moon.

He also remarks that the neutral point of sky polarisation was in the neighbourhood of the sun, a statement difficult to comprehend, as the neutral points are commonly defined by their distance from the sun.

1860.—Secchi on Mont St. Michel. Used an Arago's polariscope, and found that the images were not of equal colour, and that one was elongated in one direction, the other in a direction perpendicular to it. This last appearance was probably imaginary, as the crystal in the polariscope would prevent the extinction of any polarised rays.

1860.—Pragmowski at Briviesca. Used a plate of right and left-handed quartz at the common focus of the object glass and eye-piece, and a Nicol's prism in the eye-piece. This combination should give two semicircles of complementary tints when the plane of polarisation is oblique to the line of junction of the quartz. Using a power of 22, placing the line of junction vertical, and bisecting the sun, he found the top and bottom alone of uniform tint, the two semicircles being very strongly coloured, one red, the other green. He thence inferred a radial polarisation. In reality, in this case, he should have found the sides alike, as well as the top and bottom, only faint yellow instead of purple, and the colours most strongly marked at angles of 45°.

1868.—Campbell used a Savart's polariscope and found the bands strongly marked, having a maximum at 140° from the vertex.

1868.—Winter used a similar instrument, and, as a result, found the polarisation very strong, especially close to the sun.

1869.—Pickering at Mount Pleasant. The writer used an Arago's polariscope, and found the sky polarised close to the corona, the plane being the same on all sides of the sun.

1869.—Smith, at Eden Ridge, records a similar result obtained by a member of his party.

We see, therefore, that the results are very variable, the polarisation of the corona, if any, is obscured by that of the sky, probably due to secondary reflection. It is therefore very desirable to use some means of neutralising this effect. One remedy is to place a double-image prism in front of the telescope, which thus superposes two images of the sky polarised at right angles. For observations on the sky no telescope should be used, or the light will be too much enfeebled. A Savart's polariscope is the most delicate instrument, but such a one as the Arago is more useful to determine just what portion of the light is polarised. The Nicol's prism and the double-image prism give such indefinite results, that little could be expected of them, and they have been tried by several observers without success. The best instrument to measure the intensity of the polarisation is the polarimeter, consisting of several glass plates, which can be set at an angle in front of a Savart, and the point of disappearance of the bands recorded. The absence of polarisation of the protuberances has been observed by Abbadie, Pragmowski, and others, and seems so well determined, their further examination is unnecessary.

EDWARD C. PICKERING

THE RESOURCES OF LA PLATA

The Mineral and other Resources of the Argentine Republic. Published by special authority of the National Government by Major I. Rickard, F.G.S. (London: Longmans and Co., 1870.)

MAJOR RICKARD has executed in a very creditable manner the task which the National Government deputed him to perform, and his volume will give its readers considerable insight into the vast material resources of the Argentine Republic. Hitherto La Plata, in spite of its name, has been regarded rather as a field for agricultural enterprise than as a source of mineral wealth; and the stories which were once current of mountains rich in precious metals have been forgotten in the details given by our countrymen of their successful farming in the pampas of the south. Various causes have combined to divert attention from the mineral riches of the country. The tedious contest with Lopez, only just concluded, and the turbulent character of the gauchos and Indian tribes, have checked the growth of confidence in the minds of emigrants or capitalists; and though the vigorous administration of President Sarmiento has already done much to remove these obstacles, some time must still elapse before investors will be persuaded that "the great Republic of the South" is likely to realise all the favourable vaticinations of which Major Rickard makes it the subject. What, however, has most retarded the progress of mining, and, indeed, of all industrial enterprise, in the Argentine Republic, has been the deficient population and the want of means of intercommunication and transport. Buenos Ayres and the other riverine provinces, where grazing is a pleasant and profitable pursuit, absorb nearly all the working power of the Republic, and at the present time not more than 2,687 persons are engaged upon any form of mining industry. If the reader will cast his eye over a map of the country (the absence of which in Major Rickard's book is a serious defect), he will see what a mere scratching of the soil can be effected by such a handful of men. Hence it is that very trifling results have hitherto been obtained from the few mines yet in operation, and that the processes for reducing the ore have remained defective and costly. The supplies drawn from the Argentine Republic produce no appreciable effect upon the metal markets of the world, and in popular estimation its exports solely consist of tallow, wool, and hides.

President Sarmiento, a man of no common discernment, is convinced that the substantial wealth of his country is to be found in its mineral resources, and, moreover, that they will provide the surest means for promoting rapid and extensive immigration. In their development is bound up the extension of commerce and the progress of agriculture. Had it not been for the discovery of gold, California might still have remained a vast cattle range to this day; and what is there to prevent La Plata, which can boast of the riches of Copiapo, Potosi, and Famatina, from rivalling her neighbour in wealth, population, and national importance?

The Argentine Republic is divided into fourteen provinces, and extends southwards from the Tropic of Capricorn to the 40th parallel of latitude. Roughly speaking, the characteristics of the country admit of a two-fold division; the northern and eastern provinces being metalliferous, and the southern and littoral provinces agricultural. The latter term must, however, be accepted with some qualification, inasmuch as the cereals hitherto produced have been very scanty, and to this day the Republic is an importer of flour. In truth, the natural pasture is so abundant, and alfalfa or lucerne thrives so luxuriantly, that stock-farming is practised almost to the exclusion of all other branches of agriculture.

Major Rickard remarks that mining in La Plata is at once in its infancy and in its old age. An instance of this anomalous state of things may be seen in Mendoza, where the ancient silver mines, El Rosario and San Rumaldo, which were discovered as early as 1638, are again in active operation. The old miners dealt merely or chiefly with what Spaniards term "warm metals" (*metales calidos*), that is, those which could be reduced directly by means of mercury, and this, therefore, left for a later generation the "cold metal" (*metal frio*) which required for its reduction the process of smelting. Silver mines are not by any means confined to the province of Mendoza. In San Juan (where civil war and revolution have long been fatal barriers to all industrial progress) the district of Tontal, on the slopes of the great Andine range, is peculiarly rich in argentiferous lodes; ordinary samples from the Mine Señor containing not less than 160 ounces to the ton, and first class samples yielding 400 ounces. But, in the opinion of Major Rickard, the silver mines of Famatina, in the more northerly province of La Rioja, are the richest in the whole country. That the difficulties in working them are formidable may be gathered from the fact that some are situated 13,000 feet above the sea-level, and that the whole district is deficient in fuel of any sort, and exposed during three months of the year to a rainfall so heavy as to compel the miners to suspend their labours. In the province of Catamarca copper is the predominant metal, and in union with it an appreciable percentage of gold and silver has been found. During the year 1868 the Restauradora mine produced 2,639 tons of ore, containing by assay 506 tons of fine copper; but it must be remembered that the prevailing systems of smelting are by no means perfect.

More than one auriferous district exists within the limits of the Republic, and those which are respectively named Gualilan and Guachi (from *Gua*, which in the Huerpe tongue signifies gold) have for many years enjoyed considerable celebrity. Both of them largely enriched their first workers, and there can be little doubt that thousands of tons of ore still exist in the old workings which have been abandoned, partly from natural difficulties, but principally from the want of skilled labour.

As to the other productions of the Republic, it is impossible in this brief notice of a copious volume to do more than mention them by name. Mendoza and San Juan possess silver-lead mines of considerable extent; and in the former province petroleum springs have been recently discovered. In Santiago del Estero large tracts of land are covered with indigenous indigo; rice and tobacco are cultivated in Tucuman, and in the most northerly provinces of Salta and Jujuy are thriving plantations of coffee.

After making every allowance for Major Rickard's natural enthusiasm, it must be admitted that the country whose resources he has so minutely described, offers many and great inducements to the British emigrant to give it a fair trial. President Sarmiento desires especially to attract a further immigration of our fellow countrymen, for he infers from the success they have already achieved in the cultivation of the Pampas, that their energy and enterprise will be invaluable in developing the mineral resources of the Republic, and that Anglo-Saxon coolness and perseverance will form a favourable counterpoise to the opposite characteristics of the Hispano-American race.

C. J. ROBINSON

OUR BOOK SHELF

An Elementary Course of Hydrostatics and Sound. By Richard Wormell, M.A., B.Sc. Fcap. 8vo, pp. viii and 146. (London: Groombridge and Sons, 1870.)

THIS little book is "designed for the use of schools, colleges, and candidates for University and other examinations." In such a work it would of course be out of the question to look for novelty of matter: by the nature of the case, to praise the author's originality would be to cast a doubt on his accuracy; and, while inaccuracy would be inexcusable, no merit can be claimed for its opposite. Hence, in trying to form an estimate of a book like this, we are inevitably led to consider whether the subjects treated are arranged in a simple and natural order; whether the exposition of principles is clear and logical, the really fundamental matters being kept constantly and prominently before the student's mind, and special consequences and applications grouped about them in such a manner as to show distinctly their mutual connection and dependence: whether, in short, the book is scientific in treatment as well as in subject. We are sorry to say that, in these respects, our judgment of the work before us is by no means favourable. We should expect a student, instead of acquiring from it ideas which are capable of growth and expansion within his own mind, and being led towards the conception of the organic connection of all scientific truth, to conclude that science—or at least hydrostatics and acoustics—consists of a series of propositions which it is his duty to "get up" and write out on the first opportunity in answer to examination-questions. The least satisfactory parts of the book are the explanatory and descriptive portions, and especially the twenty-two pages at the end devoted to sound. The author says in the preface that "the whole contains all that is required on these subjects [hydrostatics and sound] for the B.A. and B.Sc. degrees of the University of London." If this is true as regards the latter subject, it is more to the discredit of the University than to the credit of his book.

Studien über das Central Nerven-System der Wirbelthiere. Von Dr. Ludwig Stieda, Prosector in Dorpat. (London: Williams & Norgate.)

DR. LUDWIG STIEDA is already well-known for his admirable papers on the central nervous system of osseous fishes, birds, and some mammals. The present work embraces a description of the central nervous system of the frog, rabbit, dog, cat, mole, and mouse; an account of the course of the fibres in the spinal cord of Vertebrata generally; a comparison of the brain of the various classes of Vertebrata with that of man, and finally, a comparison of the cerebral with the spinal nerves. Of the description of the brain and spinal cord of the several mammals mentioned above we need say nothing here, except to remark that the account is full and carefully drawn up; the minutest structure of the several parts being given as well as their coarser anatomical features. In regard to the brain of the frog, the parts of which have received such different names, Dr. Stieda gives the following description of the organ as it appears when viewed from the upper surface. The cerebrum presents the following parts in successive order:—1, the medulla oblongata; 2, the cerebellum; 3, the lobus opticus, with its median furrow; 4, the lobus ventriculi tertii (thalami optici of authors); 5, the lobi hemisphærici, each of which terminates anteriorly in a knob constituting the tuberculum olfactorium. On the under surface of the brain there appear successively from before backwards:—(1) the bases of the lobi hemisphærici; (2) the chiasma of the optic nerves, which last proceed from (3) the lobus opticus, and between which is situated (4) the hypophysis cerebri, and behind this (5) the base of the medulla oblongata. M. Stieda then gives a full description of these parts, and of the various cerebral nerves in the frog. To this succeeds a very good general view or *résumé* of the anatomy of the brain in mammals. We may draw attention to some remarks made in the section where a comparison is made between the brain of man and that of the several classes of Vertebrata. It may be premised that little difficulty is experienced in discovering the homologous parts of the central nervous system of man and the more highly organised mammals. In the birds, however, there are several parts that are difficult to decipher; whilst in Amphibia, and still more in fishes, the nature of the several parts has given rise to much discrepancy of opinion between different observers. Dr. Stieda refers to his former work for the brain of fishes. In regard to Amphibia and reptiles, he considers that the lobi hemisphærici, or anterior lobes, being hollow, and containing a ventricle, are clearly the analogues of the cerebral hemispheres of man. The azygous portion of the central cavity, between the posterior parts of the hemispherical lobes (or ventriculus communis) in the frog, is the indication of the primordial single cavity of the first cerebral vesicle, and consequently establishes the transitional stage between the osseous fishes and the higher Vertebrata. The succeeding segment constituting the lobus ventriculi tertii, (or Zwischenhirn) corresponds in its upper part to the thalami optici; in its lower to the tuber cinereum and lamina cinerea. The third segment, or lobus opticus, agrees exactly with that of fishes, both in its external and internal relations, whilst reptiles exhibit the intermediate type between fishes and birds. Of the nature of the cerebellum there can be no doubt. In regard to birds, he observes, that the great club-like segment of the cerebrum of birds corresponds to the hemisphere of man, the bodies enclosed in them to the corpora striata, the radiated septum to the septum pellucidum. He considers the existence of parts analogous to the corpus callosum and fornix of man to be doubtful. The succeeding segment corresponds to the optic thalami; the large spheroidal body of the lobus opticus to the corpora quadrigemina. Two plates accompany the treatise, which are devoted to the histology of the parts described.

H. P.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Difficulties of Natural Selection

As Mr Bennett complains that I have charged him with errors he has not committed (which I should much regret to have done), I must ask permission to justify my statements by a reference to his own words.

1. Mr. Bennett says that he is unable to discover where he has led his readers to understand that there is only one completely mimicking species of *Leptalis*. I will therefore show him where he has done so. In the third column of his article (p. 31) he says: "Another South American genus of Lepidoptera, the *Leptalis*, belongs structurally to an entirely different class, the *Pieridæ*, and the majority of its species differ correspondingly from the *Heliconidæ* in their size, shape, colour, and manner of flying, being nearly pure white. There is, however, *one particular species* of *Leptalis*, which departs widely in external facies from *all its allies*, and so closely resembles a species of *Ithomia* as to deceive," &c. &c. Then comes the argument and the mathematical calculations always referring to "the *Leptalis*," and it is at the end of this, at the bottom of the next column, that we have the following passage (of which Mr. Bennett in his reply has only quoted a line and a half): "For supposing the chance is reduced from one in ten million to one in ten thousand, and it is said that the world has existed quite long enough to give a fair chance of this having occurred once, it is not a solitary instance that we have. Mr. Bates states that in a comparatively small area several distinct instances of such perfect mimicry occur, Mr. Wallace has a store in the Malay Archipelago, Mr. Trimen records several of wonderful completeness in South Africa," &c. Now, as there is not a word here about other species of *Leptalis*, but only about other cases of mimicry, as *Leptalis* is unknown in Africa or the East, as mimicry occurs in other genera and families of Lepidoptera, and other orders of insects, and as Mr. Bennett has himself stated, that the "*one particular species of Leptalis* departs widely in external facies from *all* its allies," I think it will be admitted that I was justified in asserting that Mr. Bennett's readers would be "led to understand," that there was only one species of completely mimicking *Leptalis*. If I was not so justified I confess my ignorance of the English language, and beg Mr. Bennett's pardon.

2. I leave your readers to judge for themselves whether the fact of a *Leptalis* having twenty offspring does or does not affect the mathematical argument as set forth by Mr. Bennett, but when, in answer to my statement, that the right variation has, by the hypothesis, a greater chance of surviving than the rest, he asks: "By what hypothesis? The hypothesis that these small variations are useful to the individual, the very hypothesis against which I am contending as unproved,"—I must protest against his denying his own words. For, at p. 31, col. 1, he says: "The next step in *my argument* is, that the smallest change in the direction of the *Ithomia* which we can conceive, on any hypothesis, to be beneficial to the *Leptalis* is, at the very lowest, one-fiftieth of the change required to produce perfect resemblance;" and six lines farther on, "For the sake of argument, however, *I will suppose* that a change to the extent of one-fiftieth *is beneficial*," and then comes the calculation. Again, I must acknowledge my ignorance of the meaning of words if Mr Bennett does not here directly contradict himself. I never said the hypothesis was proved, but only that Mr. Bennett's argument, founded on it, was unsound, and for the sake of the argument he had admitted the hypothesis.

Mr. Bennett goes on to say: "The new factor, of which I take no account, is, again, entirely dependent on the admission of the natural selectionist premiss." This new factor is the principle of *heredity*. As he acknowledges that he takes no account of it, we must presume that he denies its existence; and as the whole of Mr. Darwin's theories and my own fall to the ground without it, he might have spared himself the trouble of his "mathematical demonstration."

3. I do not consider, as Mr. Bennett seems to do, that the distinction between "protective resemblance" and "mimicry" is a subtle one. Anyone who reads his paragraph on this subject (p. 32, col. 2) will, I think, be under the impression, as I was, that he alluded to mimicry, or mimetism, properly so called, as being strongly developed in birds. It seems, however, that he means only protective resemblance; but this, I believe, to be equally common among the very lowest forms of life. Transparency, for example, is a great protection to aquatic animals, and it is very prevalent in low organisms. Fishes are all, or almost all, protectively coloured, by the back being dark and the belly light, so that, whether looked at from above on the dark background, or from below on the light one, they are equally difficult to see. In many fishes, too, we have a specific protective resemblance as perfect as in any birds (see "Contributions to the Theory of Natural Selection," p. 55), and this is as much opposed to Mr. Bennett's theory as the absence of true mimicry in birds and mammals.

4. Mr. Bennett says, I have "brought no evidence to show that *extremely small variations* afford any immunity from the attacks of enemies,"—but this was quite unnecessary, because I show that the variations which continually occur in insects are by no means "extremely small." He also says that I "give no explanation of the tendency of the *Leptalis*, referred to by Mr. Bates, to produce naturally varieties of a nature to resemble *Ithomiæ*." But Mr. Bates introduces this remark with—"It would seem as if;" and though I think that the fact may be so,

and that it is not difficult to explain, yet I do not feel bound to explain every supposed fact as if it were a well-established one. As to the "parallelism of the development of protective resemblance and of instinct in the animal world," which I am also asked to explain, I deny that it has been proved to exist.

In conclusion, I will observe that the theory of Natural Selection, and its subordinate theory, Mimicry—have now been so fully developed by Mr. Darwin, Mr. Bates, Mr. Trimen, and myself, that I conceive it to be a full and sufficient answer to any opponent if we can show that his particular objections are unsound. This, I believe, I have done in the case of Mr. Bennett, although I am sorry to find that he cannot see it, and it is therefore unnecessary to go fully into the collateral points on which he has touched, and which have already been sufficiently explained by Mr. Darwin or myself.

Alfred R. Wallace

I am forcibly reminded of Pope's lines,

> A little knowledge is a dangerous thing;
> Drink deep, or taste not, the Pierian spring,

by the argument used by Mr. Bennett in the P.S. to his letter in Nature, of the 24th November, in which he says, after quoting a passage from a paper by Mr. Jenner Weir: "Here at least it would seem as if *imperfect* mimicry was anything but beneficial to the individual; how can the principle of natural selection account for its propagation in these instances?" He considers that a little mimicry is a dangerous thing. I would rather agree with Lord Brougham in his remark on the above lines, that as a little knowledge is better than great ignorance, so a little mimicry is better than great dissemblance.

But the case referred to by Mr. Jenner Weir is plain, and the argument, instead of being against the theory of natural selection, is really in its favour.

Some of the larvæ in question, for some reason of which we are unaware, are not so palatable to birds, and they, therefore, are not eaten by them to the same extent. These larvæ have not so much need of the aid of protective resemblance, and indeed their hair, spines, and gay colouring are advantageous to them instead of a drawback. The smooth-skinned larvæ require the aid of protective resemblance for their preservation, but no one would for a moment expect that because an insect has a protective resemblance to the place on which it rests, that every individual is to escape destruction by its enemies.

Mr. Bennett again asks for an explanation of the tendency of the South American *Leptalidæ* to resemble *Ithomiæ*. I think the reason is clear. Mr Bates, in his paper, read before the Linnean Society in 1862 (Trans., vol. 23), states that the *Leptalidæ* are exceedingly rare compared to the *Heliconidæ*, and that the proportion is about 1 to 1,000, and also that none of the *Leptalidæ* are found in any other locality than those of the *Heliconidæ* they mimic. From this I should judge that the *Leptalidæ* cannot make head against their enemies, and require the assistance of mimicking some better protected species to be able to maintain itself.

November 25 S. N. Carvalho, Jun.

Professor Huxley has referred Mr. Bennett to the highest authority for an answer to his reasoning on a difficulty in the theory of natural selection. Meanwhile, Mr. Wallace has replied on his own account. Upon the biological question I do not presume to touch, but I wish to say a word upon the mathematical one, especially as I cannot think Mr. Wallace has really met this part of the argument.

Mr. Bennett's argument is shortly this. A modification must be advantageous before natural selection can take hold of it. In order to be advantageous, it must not be too small; it must be so great as to be attainable only in the course of many generations, during which, in the absence of natural selection, we must see whether chance will carry us over the ground. As an extreme concession, he supposes that an advantageous *amount* of change might be accumulated in twenty steps; and, assuming that the required *direction* of change is only one out of twenty directions equally probable, he easily shows it to be violently improbable that a stationary population of one million should produce a single instance of even ten such steps in successive generations.

But why is it necessary to suppose the steps made in successive generations? Provided that the required number are made within reasonable time, it may surely be immaterial what intervals of merely unprogressive variation may elapse between them. In 200 generations, the first, fifteenth, fiftieth, for instance, and seventeen more, might make steps in the right direction, and all the rest might make steps in some or all of the other nineteen possible directions. Ten would in fact be the most probable number of steps in the right direction, and it would be about an even chance that there were ten at least.

However, as soon as we suppose steps in other directions, we must allow for the possibility of steps which shall actually reverse such progress as might be made in the right direction. If one change out of twenty equally likely is in the right direction, there will be on an average one in the opposite direction, and eighteen in indifferent directions. If we assumed that, in 200 generations, 180 were neutral, while twenty made steps forward or steps backward, these twenty *might* be all forward, and the chance that they were so would be one in 2^{20}, or one in little more than a million. Generally, the number of neutral steps would be a little more or a little less than 180, and if we allow for this the resulting chance will be considerably increased. Several instances would probably be produced by a population of a million; and I presume it is easy to allow much more than 200 generations of butterflies.

Nov. 23 C. J. Monro

Dr. Nicholson's "Zoology"

I notice in Nature for Oct. 20, a review by Mr. E. Ray Lankester, of a Manual of Zoology recently published by me, and I crave a small portion of your space to say a few words thereon. Upon Mr. Lankester's zoological strictures on my work I will not enter, partly because the public verdict on the merits of my work has already been very emphatically and decisively expressed; partly because the sins laid to my charge are chiefly of *omission* and not of *commission*, and are, therefore, more or less inevitable in a work of such limited compass; and partly because it must be patent to everyone how much more admirably the work, unfortunately left to me, would have been discharged by Mr. Lankester himself.

In the matter of *Greek*, however, Mr. Lankester really must excuse me if I decline to bow to his superior knowledge. I am well aware that he probably entertains a fresher recollection of his school days than I can boast of, and I might, therefore, without shame, have pleaded guilty to some obliviousness of Greek roots. Mr. Lankester, however, has been singularly unlucky in the point of attack chosen by him. He takes upon himself to condemn the whole of the glossary to my work, because he finds the *twelfth* word of the same ("actinomeres") derived from the Greek word *aktin*, and he is good enough to add the information that "there is no such Greek word as *aktin*." Now, any decent lexicon would have informed Mr. Lankester that *aktin* is not only good Greek, but that it is the original form of the word, and that *aktis* was employed for the first time by Pindar, not, therefore, till about 450 B.C.

In conclusion, if I may be permitted to make a suggestion, I would recommend Mr. Lankester, in his capacity as critic and appraiser of the work of other men, not to judge in future of the value of a haystack by the first straw that he may happen to pull out of it; or, if he must do this, to be very sure before giving his opinion to the public, that it *is* a straw that he has succeeded in laying hold of.

Newhaven, Edinburgh H. Alleyne Nicholson

Dr. Nicholson's extraordinary assertions as to the supposed word "aktin" really demand no serious discussion, which, indeed, would be out of place in Nature. A reference to Liddell and Scott's Lexicon will conclusively demonstrate to any person interested in the matter that he is entirely wrong. The following additional blunders in Dr. Nicholson's glossary will enable your readers more fully to judge of his accuracy, and it will require considerable boldness to attempt to justify them by reference to imaginary archaic forms:—1. In several places we find Dr. Nicholson giving "poda" as the Greek for "feet," a gross grammatical fault. 2. "Pseud*os*" is given as the adjective corresponding to the English word "false." 3. "Enchuma" is said to be a Greek word meaning "tissue." It has not this meaning. Dr. Nicholson's mistake arises from ignorance of the origin of the signification of the word "parenchyma." 4. "Laima" is given in several places in the glossary for "throat," in place of "laimos."

It is improbable that these are anything but a fraction of Dr. Nicholson's etymological misrepresentations. Mistakes in the glossary of a zoological work are not of very great importance, and would not in this case have demanded notice had they not been fair samples of the general character of the book in which they occur.

I much regret that the fact of the writer's name being appended to the notice of Dr. Nicholson's work should have led him into the region of personalities, whither I do not intend to follow him.

E. RAY LANKESTER

Glass Floats off the Isle of Lewis

IT would be of great importance if the *fact* could be ascertained whether the floats are from the Norwegian or from the Canadian fisheries. Your note of November 10 says, "They are hermetically sealed, and have certain characters, such as IV. or VI., impressed on the sealed part." Doubtless your columns are read in Norway as well as in Canada, and possibly a correspondent, from these characters or from other evidence, may claim the floats for one or for the other side of the Atlantic. In favour of the *west* side, but with the utmost deference to the opinion of Mr. Gwyn Jeffreys, I suggest that a north-*east* wind is an unlikely conveyance to "the *west* side of the Island of Lewis," or to "the *western* coast of Shetland."

A writer in the *Athenæum* of this week (Nov. 19, p. 659) thinks that these "net floats" are carried to Nova Zembla, and "still farther to the north and east" by the Gulf Stream. Dr. Carpenter supposes a constant warm surface current from each tropic to each pole, and a constant cold current below from each pole to each tropic, caused as we cause the currents of water to warm our houses. Suppose this grand theory to be true. The surface current should warm east coasts as well as west coasts. The same parallel touches England, Newfoundland, and Vancouver's Island. The climate of England and of Vancouver's Island on west coasts is "insular." The climate of the *island* of Newfoundland on an east coast is "excessive." This difference of climate in islands, on the same parallel, at the same levels, results from currents of *air*, not from currents of *water*—namely, from the prevalence of south-west winds in the north temperate regions. In winter with a south-west wind we hunt, with a north-east wind we skate.

GEORGE GREENWOOD, Colonel

Brookwood Park, November 19.

ENGLISH GOVERNMENT ECLIPSE EXPEDITION

THESE arrangements and instructions are not yet finally completed, but it is thought that the latter may be useful to members of other Expeditions, though they are not yet by any means complete.

A. SPANISH AND ALGERIAN PARTY.

NOTE.—*This party leaves Portsmouth in the "Urgent," on the 6th proximo. Observers to be on board by* 5 P.M. *on the 5th.*

1. *Cadiz Detachment.*—In charge, the Rev. S. J. Perry. Spectroscope, the Rev. S. J. Perry and assistant (Mr. Hostage), Mr. Abbay; Polariscope, Mr. Moulton, Mr. Hudson: Sketches of Corona, Mr. Naftel, Mr. Smyth, Mr Penrose, Mr. Collins; Time and General Observations, Captain Toynbee.

2. *Gibraltar Detachment.*—In charge, Captain Parsons. Spectroscope, Mr. Carpmael, Mr. Gordon; Polariscope, Mr. Lewis, Mr. Ladd; Photography, Mr. Buckingham, and assistant (Mr. Spiller); Sketches of Corona, Mr. Hunter, two Oxford men; Saturn in the Corona, Mr. Talmage, Mr Maclear; Chemical Intensity, Mr. Thorpe.

3. *Oran Detachment*—In charge, Mr. Huggins. Mr. Huggins, Admiral Ommanney, Rev. F. Howlett, Mr. Carpenter, Mr. Crookes, Captain Noble, Dr. Gladstone, Prof. Tyndall.

B. SICILIAN PARTY.

NOTE.—*This Party leaves London, overland, at* 8.45 P.M. *from Charing Cross, on the 7th proximo.*

In charge, Mr. Lockyer. Spectroscope, Mr. Lockyer and assistant (Mrs. Lockyer), Prof. Roscoe and assistant (Mrs. Bowen), Mr. Seabroke and assistant (Mr. Burton), Mr. Pedlar; Polariscope, Mr. Raynard, Mr. Griffith, Mr. Clifford; Sketches of Corona, Mr. Brett, Mr. Darwin; Photography, Mr. Brothers, Herr Vogel, Mr. Harris; Time and General Observations, Mr. Vignoles, Sen., Mr. Vignolles, Jun.

INSTRUCTIONS TO OBSERVERS.

Instructions for the Polariscopic Observations of the Corona, including Beams and Streamers.—It is recommended that the polariscopic examination of the Corona be carried on as follows:—

1. To examine a detached and selected part of the Corona about 6′ from the limb of the sun, and say about 8′ in diameter. 2. A field extending from the limb of the sun outwards should be examined either with a Nicol's prism, or a double image-prism. 3. The light of the streamers at some distance from the sun should be examined with a Nicol and a crystal. 4. The polarisation of the Corona should be examined in such a manner as to eliminate atmospheric polarisation. *Note.*—The most suitable instruments for ascertaining the plane of polarisation, and the proportion of polarised to unpolarised light are (1) a double-image prism; (2) Savart's polariscope; (3) a plate of quartz, consisting of two compensation wedges turned through an angle of 180°; (4) a plate of arragonite, or calc spar, cut perpendicular to an optic axis, and affixed to an analysing prism; (5) a polarimeter, consisting of four plates of glass, movable on an axis perpendicular to the plane of polarisation.

First Observation.—The object of this observation is to observe the polarisation (if any) of the Corona without having the observer's attention distracted by the chromosphere. A Savart's polariscope is recommended by preference. The Nicol's prism of the polariscope should be set beforehand with its principal plane (or plane of symmetry) radial, *i.e.* perpendicular to the sun's limb, and the observer must note whether bands are visible, and if so, whether they are black-centred or white-centred. Should the bands be feeble, it will be well to rotate the polariscope, prism and plates of course moving together, and quickly restore it to its primitive azimuth, after having noted the estimated azimuth of the Nicol when the bands are strongest and black-centred. Should no bands, or only dilute bands be seen, it may be that the Corona, though polarised, is overpowered by other light; and the observer will move the telescope from the sun, radially if it may be, if in any other direction rotating the polariscope so as to render its principal plane radial in the new position. He will then note whether, though the light becomes feebler, the bands become less dilute. Should, on the other hand, the bands be strong, the observer, after having satisfied himself as to the plane of polarisation, will endeavour to make out whether by means of the polarisation of its light the Corona can be detected superposed on the chromosphere. He will, therefore, move the telescope *towards* the sun, keeping the principal plane of the polariscope radial, and note how near the sun the bands can still be seen. To provide for the contingency of this observation, it will be well to point in the first instance to the side of the sun which will be first uncovered. If time permit he should try whether there is any sensible quantity of polarised light on the dark disk of the moon, rotating the analyser and determinating the plane of polarisation.

Second Observation.—The special object of this observation is to differentiate, if possible, between the Corona, on the one hand, and the Chromosphere, or whatever else may be self-luminous (be it even a portion of the Corona itself), on the other. This will be possible if the light of the Corona be strongly polarised, so as to admit of comparative extinction by an analyser. The observer should turn the analyser so as to extinguish, as far as may be, the light of the Corona in the neighbourhood of a radius depending on the angular position of the analyser. He should notice the form, colour, and general appearance of any residual luminosity other than the well-known protuberances; should contrast the appearance, especially as to colour, with that seen when the light is analysed so as to retain light from the same region polarised in the perpendicular direction, and should ask himself whether the luminosity is such as could be accounted for by the superior brightness near the sun of the unanalysed light, even though it were to suffer the same proportionate loss by analysis as the Corona at a greater distance. Of the instruments named the Nicol's prism is better adapted for a general survey, not requiring any limitation of field; the double-image prism is better adapted for a comparison of the oppositely polarised images, since the observer can compare them *directly*, not one with *his memory* of the other. The double-image prism will require a diaphragm with a long and moderately broad aperture in the focus of the eye-piece, rotating with the prism, and in the observation the length should be placed in a radial direction.

Third Observation.—The observation of the streamers as to polarisation might throw much light on their nature, and the observer who undertakes this observation, by means of a Savart's polariscope, or else of a Nicol's prism capped with a plate of calc spar or arragonite, will be in a condition to determine with advantage the plan of polarisation, if there be polarisation, of the Corona generally. But the streamers must be his first object.

Fourth Observation.—It has been supposed by some that the polarisation observed in the Corona was really due to the *secondary* illumination of the intervening portion of the Earth's atmosphere, in other words, to the illumination produced by reflection from clouds, &c., towards the horizon. This effect may be eliminated by using a Savart's polariscope, or, better, a polariscope with quartz wedges, and turning the instrument till the bands (if any) seen on the moon's disc disappear. The corona can then be scrutinised as to polarisation, and the polarisation examined in different azimuths of the Nicol's prism relative to the radius drawn from the sun's centre, by pointing the telescope instead of rotating the analyser. In this observation the observer has the choice of two rectangular azimuths of the polariscope, for each of which the bands (if any) on the moon disappear, and if no bands be seen on the moon he is free to scrutinise the polarisation of the Corona, by turning the polariscope.

General Remarks.—The object-glasses of all telescopes intended to be used in polariscopic observations should be examined before departure as to their freedom from defects of annealing. All polariscopes including a Nicol's prism, or tourmaline, should be marked, so that the principal plane may be readily known *by feeling*, as sight-marks might fail for want of light. Double-image prisms should have one side of the aperture in the diaphragm marked so as to distinguish the two images.

Instructions for the Spectroscopic Observation of the Chromosphere.

Note: The objects to be obtained are:—1. To determine the actual height of the chromosphere as seen with an eclipsed sun; that is, when the atmospheric illumination, the effect of which is doubtless only partially got rid of by the Janssen-Lockyer method, is removed. If the method were totally effective, the C line, the line of high temperature, should hardly increase in height; but there can be little doubt that the method is not totally effective, so the increase in height should be carefully noted. 2. To determine if there exists cooler hydrogen above and around the vividly incandescent layers and prominences. To do this the band of the spectrum just above the stratum which gives the hydrogen lines before totality and during totality, should be carefully examined, to notice (*a*) if any traces of the hydrogen spectrum exist above the region which before totality gave the hydrogen lines, and (*b*) what lines extend outside the hydrogen spectrum and whether they also exist with it in the lower strata. 3. To test the American observations of last year as to the existence of a line at 1474 in the corona spectrum, by seeing if it be visible above the region which gives the hydrogen spectrum. 4 To determine whether any other gases or vapours are ordinarily mixed up with hydrogen, but remain invisible with the uneclipsed sun in consequence of the absence of saliently brilliant lines in their spectra. The observations should be conducted as follows:—1. Work with a horizontal slit, or a slit in a parallel of declination, according to the instrument used, whether altazimuth or equatoreal. 2. See that the spectroscope telescope works easily, so that sweeping along the spectrum is easy. 3. Find before totality an average plain-topped region of the chromosphere, where some motion on either side does not brighten, or thicken, or lengthen the lines near the part covered at the commencement of totality. 4. Observe this before and during part of totality, the telescope being driven by the clock if an equatoreal is used. 5. Just before totality sweep from red to violet; note the lines and their lengths; mark the 1474 line and the lines between D and E carefully. 6. Immediately after the commencement of totality sweep back; note new lines—their heights; especially the comparative heights of C, near D, F, and near G, with these former heights; and especially whether band over *b*, and the thickness of F. Note also the lines between D and 1474. 7. Just before the end of totality unclamp and bring back the slit to the *following* limb of the Moon; and note the extinction of the spectrum by the reappearance of the Sun, if possible by a rapid sweep; if this is not possible, then watch the behaviour of F; sweep back again to see if there be any variations from (5) in the new region now observed. 8. Carefully note position angle where slit cuts limb. 9. Record the impressions of facts, and facts not already noted, AS SOON AS POSSIBLE.

Instructions for the Spectroscopic Observation of the Corona

Note.—*The word Corona is here used for convenience, to include all the light above the prominences. It therefore includes that part of the chromosphere which is generally veiled to us when observations are made by the Janssen-Lockyer method.*

The principal object to be obtained is:—To determine whether it is possible to differentiate the outer layers of irregular outline and streamers from a stratum say some 5′ or 6′ high round the sun, which may possibly be the limit of the gaseous envelopes above the photosphere. To attack this question, we require a long slit, a large aperture, and long collimator, and small dispersive power. The slit must be adjusted for a faint cloud before totality, and on no account is it to be touched before observations of a similar cloud can be made after totality, by the heads of the party. The most important observation to make is, whether there are any dark lines in the spectrum at any distance from the sun; and if so, at what distance? Next, whether there are any bright lines; if any, their positions must be noted, especially if the lines recorded by the American observers are again visible. The observations should be conducted as follows:—1. Arrange the instrument so that the image of the following limb of the moon, at the point of its first contact, will fall on the left-hand side of the slit, placed nearly horizontally. 2. See if Corona is visible before totality, and note its spectrum with utmost care, moving the slit in azimuth, so that perhaps, at the instant during totality, while possibly with a long slit, the spectrum of the sun or prominences on the preceding limb is visible in the same field of view.

Some Particulars to be Especially Noticed by those Observers who make Drawings of the Corona

1. Its extent, and the boundary-line *if any*; if no definite boundary, this *should be stated*. 2. Whether

there is any change, all changes must be most carefully shown in any manner the artist may prefer. 3. Especially note all long streamers. 4. All tints and change of tint, and whether the colour is distributed in patches or in layers concentric with the moon, or in connection with the prominences. 5. Whether it consists of a level patch of luminous haze or radiating beams of light, or of bundles of hyperbolic rays. 6. If of radiating or hyperbolic beams, whether they are evenly distributed all round, or in groups only. 7. Whether the dark intervals between such radiating beams are constant or fluctuating. 8. Whether it is concentric with the moon. 9. Whether it is equally intense all round the moon. 10. Whether the outer border exhibits any coruscations, or whether its definition is permanent and equally pronounced all round. 11. Whether the light of the Corona is more intense or less so in the immediate neighbourhood of the prominences. 12. How much darker the moon's disc is than the sky.

ENERGY, AND PROF. BAIN'S LOGIC

[Extract from Prof. Tait's Opening Address to the University of Edinburgh, Nov. 2, 1870]

THE so-called *Laws of Motion* first explicitly stated, as we now employ them, by Newton in the Principia, are partly due to Galileo, partly to his immediate successors. Like all great physical discoveries, they were more or less clearly seen by many philosophers about the time in which Newton threw them into the simple, and yet comprehensive, form in which we now use them. As ordinarily understood, they embrace the results of observation and experiment as to the action of force on matter. The first tells us how matter behaves when not acted on by force, and therefore shows us how to *detect* the action of a force. The second tells us how to measure the force by its effects, and how to calculate the action of a force or forces acting on a mere *particle* of matter. The third, as directly interpreted, shows how to apply the other two to the motion of masses or of groups of particles. With these alone we have the foundation of an enormous portion of the science of Dynamics, and we require merely a sufficiently powerful mathematical process to enable us to develop to their utmost the calculations necessary for the determination of equilibrium or motion of any set of masses whatever, so long as the motion is visible, or capable of being rendered visible by a microscope.

But we require something more before we can extend mathematical calculations—which, be it ever remembered, are necessary in physics solely on account of the imperfections of our intellect; merely saving us an intolerable amount of thought which would otherwise be wasted on petty details—something more, I say, is required before we can apply our mathematics to Heat, Electricity, Chemical Action, &c., &c.

Curiously enough, that something was foreseen and provided for by the keen intellect of Newton. He gave it in the form of a *second* mode of interpreting his third law, quite distinct from the ordinary one, which is the well-known assertion that "Action and Reaction are equal and opposite." Instead of using the terms Action and Reaction in the sense of mere pressures or tensions, he shows that the law will equally hold if they stand for what are now called rates of *spending* or of *receiving* energy; or, in more familiar language, rates of doing work. So that whenever there is transference of energy from one body to another, the one gains *exactly* as much as the other loses. This is at present the grandest physical law known. That we may understand it better, let us take first a simple physical fact, but one of a somewhat analogous nature. It is a comparatively recent discovery that *matter is indestructible*, yet so important that without it we may be certain that chemistry could never have become a science. If a chemist were not assured by experiment that no quantity of matter, however small, is ever put out of existence, submit it to what ordeals he may, what confidence could he have in the results of an analysis? Or again, where would his science be if new matter could suddenly make its appearance? The balance is his most important instrument, but without the confidence (derived from experiment) that matter cannot change in quantity, its indications would be of no value to him.

So it is, but in a more extended sphere, with the Natural Philosopher, and it is a source of legitimate pride to us, that as Newton first hinted at this grand modern generalisation, and first gave the mathematical method naturally fitted for its development, so it is to this country again, and mainly to Dr. Joule of Manchester, that we owe the proof (which must, of course, be *experimental* to be valid) that energy is, like matter, *indestructible*. It is, therefore, in the usual sense of the word, as REAL as matter. In fact, the physical phenomena of the Universe (excluding in the meantime, on account of our utter ignorance, some of those connected with life) depend upon matter and energy alone. Different combinations of matter constitute the subject of our chemistry; different groupings of molecules as well as of masses, and different distribution of Energy, form the rest of our Natural Philosophy. Hence the overwhelming importance of this real *something*, Energy, in the whole of Physical Science.

I shall devote the rest of my time this morning to very elementary notions connected with energy and this grand law of Nature. But before I do so I have a few words to say about another work in which the principles of Natural Philosophy are discussed; a book infinitely more likely than that of Hegel (whose absurdities I have already pointed out to you) to fall into your hands. It is now not a dreamy and dogmatic German, evolving everything from himself, and railing at physical facts as well as at exquisite methods in mathematics, with whom we have to do—it is on the contrary, a hard-headed Scotsman, and a Professor in one of our Universities. We have here no evolution from consciousness to laugh at, no sneering at experimental science; we have to guard against dangerous misconceptions of the truths discovered by physicists; mistakes all the more dangerous that they are honestly held, and that they have been assigned a prominent place in a text-book which many of you may have at some time to read; and especially because, as students, you are peculiarly liable to be led away by *ex cathedrâ* statements. For obvious reasons I cannot take many examples now; in the more abstruse, the statement itself, and the exposition of its error, would be alike unintelligible to you; in the simpler ones you may be trusted to see the error for yourselves.

The first I quote is from what is called the Logic of Physics, and is, to a certain extent, personal. "*Volume* and *mass* rightly precede *density* in order of definition. Messrs. Thomson and Tait make *density* precede *mass*." And we do so, we think, very logically, because density is a *specific* property of matter, unalterable in general, except to a very small extent, by physical processes, while volume and mass are absolutely indefinite, depending as they do upon the quantity of matter spoken of.

Again, "In the transfer of force, *nothing is lost*. The mechanical momentum transmuted into heat is fully accounted for in the heat produced; by proper arrangements it could all be gained back." The last nine words, however they may be interpreted, are essentially false: in fact they contain an explicit denial of the second law of thermodynamics upon which Sir W. Thomson based his grand law of Dissipation of Energy, one of the most important of his many splendid contributions to physics, and one having the most direct bearing upon the future of the physical universe. The rest of the statement, as it stands, is also false. It may be made correct by writing one of the words, *work*, *power*, *potency*, or preferably *energy*, in place of *force*, and also in place of *momentum*. What would be thought of a man who should say—"I paid six weeks for it," meaning "pounds" by "weeks." For "momentum" cannot be transformed or "transmuted" at all, it remains for ever unchanged.

Again, when bodies impinge on one another, "the rise of temperature is exactly proportioned to the visible momentum destroyed." Let us put the correct word "energy" in the place of momentum, and we find that this asserts the startling physical fact that the specific heat of every body is the same at all temperatures. If we take the statement as it stands without correction, it is simply nonsense.

Again, "the foot pound, meaning the force expended in raising one pound weight one foot, which is the same as a (*sic*) momentum of one pound moving at eight feet per second."

Raising one pound weight one foot is a feat which, by proper combinations of machinery, may be effected by any given force whatever, be it the weight of a grain or of a million tons. But a "foot pound," and the "momentum of one pound moving at eight feet per second" cannot possibly be compared with each other, any more than a cubic yard can be expressed as a number of square yards, or the height of a mountain in acres, roods, and poles.

You will see that the error in the examples I have just chosen (excepting of course the fatal one about restoration of energy) is in great part due to the misuse of words. Yet it is from a treatise on Logic that I have quoted!

The essence of the lesson taught by all this is simply the conviction that scientific knowledge has reached such an immense development that no one man can now possibly master thoroughly more than one or two of its many branches. There can be no "Admirable Crichtons" in our days. The greatest logician the world has produced, or is likely to produce, for many a long day, the lamented George Boole, more than once expressed his regret that a systematic logical treatment of human knowledge, even in moderate compass, and going little farther than the elements in each branch, had become absolutely impossible as the work of one man—impossible, that is, for a man who revolted at the idea of publishing anything he knew to be defective.

MOUNTAIN CLIMBING

IN the number of NATURE for June 23, 1870, I described an ascent of Mount Etna which I made on March 4 of this year, with an excellent guide, Pietro Cravagna.

I now propose to make some remarks on a few points of interest with regard to mountain climbing. One of the most important of these is the alleged lowering of the internal temperature of the body under such conditions.

During two ascents of Mont Blanc made on the 17th and 26th of August, 1868, by Dr. Lortet, of Lyons, and Dr. Marcet, of London, and described by Dr. Lortet in the *Lyon Médical* of September 26, 1869, experiments made apparently with great precaution on Dr. Lortet himself with a registering maximum thermometer (of Walferdin), by which (between + 30° C. and + 40° C.) hundredths of a degree could be appreciated, showed that the internal temperature of the body under such conditions is lowered to a very remarkable extent.

I will quote Dr. Lortet's own words: "A jeun et exactement dans les mêmes conditions, *pendant la marche*, la décroissance de la température intérieure du corps est très-remarquable, *elle est à peu près proportionelle* à l'altitude à laquelle on se trouve."

In effect, from the table given in the paper referred to, I find that during the first ascents the internal temperature descended *gradually* from 36·3° C. (that *during exercise* at Chamounix, 1,050 metres above the sea) to 32° C. at the summit of Mont Blanc (4,810 metres above the sea); while during the second ascent the difference was from 35·3° C. to 31·8° C.

Dr. Lortet found that as soon as he stopped for a few minutes, the temperature of his body rose briskly to the normal standard, except on the summit itself, where "il a fallu près d'une demi-heure pour que le thermomètre atteignît sa hauteur habituelle."

During digestion, in spite of the exercise being taken, the temperature remains normal, or even rises; but this does not last long: "Une heure à peine après avoir mangé, le corps se refroidit de nouveau par les efforts." The descent of the temperature of the body under such conditions, then, amounts sometimes to more than 4° C.; and if we take the difference between the normal temperature of the body *at rest*, and that observed by these experimenters on the top of Mont Blanc, the difference amounts in one case to 5° C., "abaîssement énorme pour les mammifères dont la température était reputé constante!" as Dr. Lortet justly exclaims.

Now Mount Etna is particularly suitable for such experiments; one begins to walk either at Nicolosi, or preferably at the Casa del Bosco, and one has nothing to do but to go straight up; there is nothing in the way, it is simply a long "grind" of some five or six hours or more, according to the state of the snow. A series of misfortunes with my maximum thermometers prevented my repeating the above-described observations, and I have referred to them at such length in the hope that some one may be induced to take the excellent opportunity afforded by the expedition to Sicily of deciding so important a point.

The state of the circulation is hardly less important than that of the internal temperature.

Dr. Lortet found that the pulse increased in frequency from 64 per minute at Chamounix to 172 on the summit of Mont Blanc, and he was further enabled, by means of the sphygmograph, to make some observations as to the *state* of the pulse at various altitudes. In ascending Etna I made some comparative observations on the frequency of my guide's pulse as compared with my own, which show some points of interest.

At the Casa del Bosco my pulse was 68, my guide's 74; we had both *ridden* to that point, and the difference is probably an illustration of the established fact that the circulation of persons living in mountainous districts is quicker than that of those living in plains. On arriving at the summit of Etna after a ride of an hour and a quarter (from the Casa del Bosco), a rest of fifteen minutes, and a stiff walk of three and a quarter hours over dry, hard snow (an exceptionally-easy ascent),* my pulse was very irregular, and about 114 or 115 to the minute, while the guide's was only 89—that is to say, that mine had increased 46 beats in frequency, his only 15; or mine was about $\frac{2}{3}$rds, his only about $\frac{1}{5}$th, as fast again; showing the slight effect of such ascents on those who are used to them, and who live habitually in mountainous countries.

But this was still more forcibly illustrated by the state of our pulses after a very quick descent, a regular trot all the way (we had stayed two hours at the summit, and eaten a hearty breakfast); at the place where we left the mules, I found that while my pulse, after a minute or two's rest, was at 106 or 107, the guide's was at 99 or 100 mine being 8 beats *less* than it was on arriving at the top his 10 beats *more*; his circulation was *less* disturbed by walking fast up the mountain, carrying a load of provisions, than by running down it with nothing to carry ; mine, on the contrary, as one would expect, much more so by walking up than by running down. (Of course I could get nothing in the shape of sphygmograph at Catania.)

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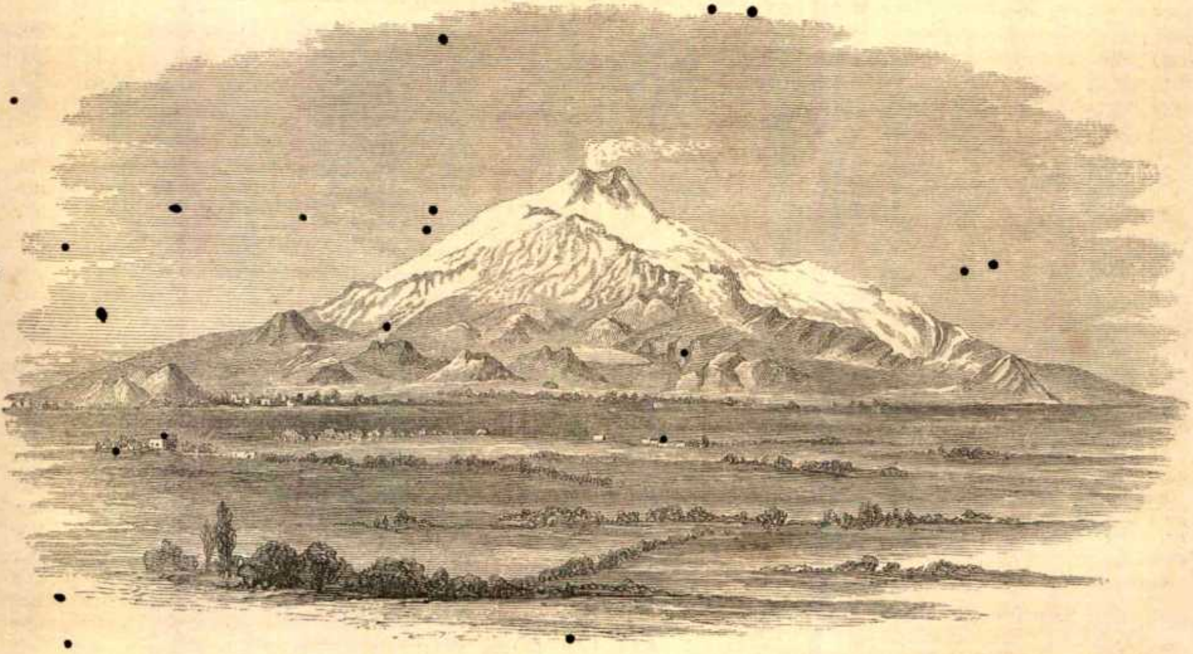
* Later on in the month of March, when much snow had recently fallen the Rev. A. G. Girdlestone and I, with two other friends, made an ascent: it took us nearly eight hours to walk from the Casa del Bosco to the Casa degli Inglesi, and we saw nothing but a very heavy snow-storm.

Dr. Lortet has recorded some interesting observations, made with the aid of the anapnograph of MM. Bergeon and Kastus, on the state of the respiration. It is much quickened, as is well known ; the expiration is much prolonged, its amplitude much lessened, the inspiration shortened and quickened ; much less air being inspired and expired than is normally the case.

These effects are partly due to the rarefaction of the air, less oxygen by weight being taken in at each inspiration, and partly to the excessive muscular exertion, which demands a corresponding increase in the animal heat, and so a corresponding increase in the amount of oxidation taking place in the blood ; this not being always obtainable and the exercise being continued all the same, the normal temperature of the body cannot be maintained, and so it falls, and one becomes miserably cold while walking, and has to stop to get warm again.

Now as to the amount of work done ; that is very much greater than is commonly supposed : leaving out of consideration the difficulties encountered in walking either over soft snow or over slippery places, especially when very steep, I find that in climbing Etna, starting as I did, on the occasion that I have already described, from some distance above the Casa del Bosco, I lifted about 150lb. to the vertical height of at least 5,700 ft., or about 380 tons one foot ; adding 10 foot-tons (little enough) for the horizontal distance traversed, we have 390 foot-tons as representing work done in 3¼ hours : now 390 foot-tons is about the work done by a person of the weight above mentioned in walking 22 miles on level ground ; that is to say that, without making any allowance for the increased difficulty of breathing due to the rarefaction of the air or for any of the consequences of this (increased action of heart, &c.), one has walked 22 miles in the time ordinarily taken to walk 13.



A VIEW OF ETNA

To put it in another way : 390 foot-tons is a hard day's work, as it is found that something over 300 foot-tons is the average day's work of strong labourers. One therefore does a hard day's work in 3¼ hours, and this after an ordinary day's work, *plus* a fatiguing ride of some four hours on a mule, over lava currents and cinders, in the middle of the night and without any chance of sleep.

Taking everything into consideration, it is difficult to believe that the fatigue is, as it is often stated, out of all proportion to the work done ; we must not only consider the amount of work, but *the time in which it is done*, and this is what I have especially wished to point out, as one can easily understand that the fatigue must increase very fast as the time in which the work is done decreases.

At the summit of Etna (the accompanying woodcut, from a rough sketch taken when a good deal of the snow had melted, can give but a feeble idea of the exquisite effect produced by the dazzling whiteness of the snow against the perfectly clear blue Sicilian sky) the range of temperature within a few feet of vertical distance is very remarkable. Just after sunrise I found that the temperature of the air at the height of four feet or so above the ground was — 2° C. ; on the ashes where the snow had melted it was + 9° C. ; just under the surface it was 20° C., and a few inches under it was higher than 36° C. (my minimum thermometer, the only one I had left, not allowing me to register a higher temperature than this).

This high temperature of the ashes only occurs where they are mixed with sulphur, which is continually undergoing oxidation ; the other parts of the cone and crater are in the winter covered with snow, and it is very strange to see snow a foot deep or more, quite close to ashes that really feel uncomfortably hot.

Those who wish to "get up" the history of Mount Etna and its structure, should refer to Sir Charles Lyell's "Principles of Geology," tenth edition, vol. ii. ; or for a much more detailed account of the eruptions, its present and past conditions, &c., to "La Vulcanologia dell' Etna," by Carlo Gemmellaro, published at Catania in 1858.

W. H. CORFIELD

NOTES

As will be seen from another column, the Eclipse Expedition is now fully organised, and all the parties will be on their way before our next number appears. The Organising Committee, who have done about three months' work in a fortnight, deserve all praise for their untiring efforts, and we may hope that they will be adequately rewarded by the results obtained. We may mention that on the representation of the Organising Committee, the Government have communicated with the French and German Governments with a view of securing the services of M. Janssen, who, if he can be got out of Paris, will accompany Mr. Lockyer to Sicily.

It is stated that Prof. H. J. S. Smith, F.R.S., has been appointed to succeed the late Dr. W. A. Miller as a member of the Royal Commission on Scientific Instruction and the Advancement of Science.

The annual election of the Council and officers of the Royal Society took place yesterday, when the following gentlemen were elected, viz.:—President: Gen. E. Sabine, K.C.B. Treasurer: William Spottiswoode, M.A. Secretaries: William Sharpey, M.D.; G. G. Stokes, M.A. Foreign Secretery: Professor W. H. Miller, M.A. Other Members of the Council: George Burrows, M.D.; Heinrich Debus, Ph.D.; P. M. Duncan, M.B.; Sir P. de M. Grey Egerton, Bart.; Prof. G. C. Foster, B.A.; Francis Galton; J. P. Gassiot; J. D. Hooker, C.B, M.D.; William Huggins; Prof. G. M. Humphry, M.D.; J. Gwyn Jeffreys; Sir J. Lubbock, Bart.; C. W. Siemens; Prof. A. J. S. Smith, M.A.; Prof. John Tyndall, LL.D.; and Prof. A. W. Williamson, M.D.

It is with very great regret that we have to announce the disablement of two of our most prominent scientific men. Sir R. I. Murchison has been stricken with an attack of paralysis, and Professor Balfour Stewart was among the sufferers by the collision on the London and North-Western Railway on Saturday evening last. In the latter case, though the injuries are severe, one thigh broken, and a great shock to the nervous system, we may hope that Professor Stewart will shortly be restored to his friends and to science. At the moment of going to press, we hear that Sir R. Murchison's state is considered somewhat improved, and that Sir W. Ferguson reports Prof. Stewart to be going on satisfactorily.

The Anniversary Session of the St. Andrews Medical Graduates Association will be held at the Freemasons' Tavern, Great Queen Street, on Friday and Saturday, December 2nd and 3rd. On Friday, at 7.30 P.M., Dr. Whitmore will read a paper "On the results of Sanitary Legislation on the Health of the Metropolis and our present urgent Sanitary requirements." On Saturday, at 5 P.M., the President, Dr. Richardson, F.R.S., will deliver the Anniversary Address "For the Future of Physic."

At the first ordinary meeting for the session of the Society of Arts, held on the 16th ult., the following silver medals were awarded:—To Mr. Thomas Dickins for his paper "On Silk Supply;" to Mr. James Collins for his paper "On Indiarubber, its History, Commerce, and Supply;" and to Mr. William Bridge Adams for his paper "On Tramways for Streets and Roads and their sequences;" and the Prince Consort's Prize of twenty-five guineas to Mr. Edward Turner Sim, who, at the Society's examination, had obtained during the last four years the largest number of first-class certificates.

Counts H. Wilcjek and G. Wurmbrand have been engaged, at the instance of the Viennese Anthropological Society, in an examination of the Austrian lakes, and have found remains of pile-dwellings in the Attersee. One of them belongs to the Stone period.

At the remote city of Indianapolis an Academy of Sciences has just been formed under the presidency of Professor E. T. Cox. The exclusive object of the association is the cultivation and imparting of knowledge of the natural and physical sciences. Though the number of members at present does not appear to be large, it will, no doubt, like most other things in the Far West, increase rapidly. We wish all success and a prosperous future to this the youngest society for the advancement of natural history.

The recently published volume of Bentham's "Flora Australiensis" (vol. v.) includes the natural orders from Myoporineæ to Proteaceæ.

Messrs. Macmillan will shortly publish, as one of their series of school class-books, Lessons in Elementary Physics, by Prof. Balfour Stewart.

We have just received the first part of the second edition of Schellen's Spectral-analyse. The preface states that it has been carefully revised, and considerable additions made, in accordance with the progress of science since the publication of the first edition, especially in the application of spectrum-analysis to the sun. It is copiously and beautifully illustrated.

The third section of "Husemann's Pflanzenstoffe in chemischer, physiologischer, pharmakologischer, und toxikologischer Hinsicht," does not, as was expected, complete the work. It is occupied with vegetable acids and neutral substances.

Mr. Matthew Williams, jun., has reprinted, from "Essays of the Birmingham Speculative Club," a paper on "The Relation of the Universities to Practical Life." The writer's aim is to assert the claims of experimental science to a higher place in the scheme of English University education. It is monstrous that such a plea should still be necessary. While it remains so, however, we welcome all who, like Mr. Williams, can break a lance in the cause of true progress in such a manner as to appeal even to the most "practical" sympathies. Why should not others in the great centres of English industry take up the subject? Agitation works wonders in the political world; who knows what it might not in time effect for science?

On the 7th of August the most severe shock of earthquake since April 1868, has been felt in the Hawaian Archipelago. The time was $4^h\ 10^m$ A.M., and the shock was felt generally in Maui, Molokai, and Hawaii, but not so much in Honolulu. The damage done was not so great, but it was considered remarkable that the shock lasted ten seconds—a long period for a single shock. The cause of its diminished violence is attributed to its being vibratory and extremely regular. In Molokai the earthquake was accompanied by a tremendous roar, and appeared to have a circular motion, so that no precise direction could be noticed. It is stated that the fissures on the Kau side of Maunaloa are constantly steaming, and that smoke is issuing from the crater of Makeaweoweo in great volume.

H.M.S. *Zealous*, bearing the broad pennant of Admiral Farquhar, having visited St. Charles, one of the Galapagos Islands, has found sufficient objects of interest to induce the Admiral to make another trip there in October, to examine the natural history of the place.

On Thursday, the 29th September, two violent shocks of earthquake occurred at Lima, in Peru, about 10 P.M., causing much alarm.

A passenger reports to the *Rangoon Times* a waterspout seen on the Irawaddy River on the 31st of August in the Chwaygeen Creek. He saw a dense mass of clouds, and then a whitish cloud, which resolved itself into a large waterspout. It soon partially dispersed, but formed again, and remained about ten minutes, sometimes straight, at others curved. The convex side was to the wind. The spout appeared to be rapidly skimming the surface of the river, the water rising like spray round it, and having a spiral appearance along the shaft. The phenomenon was not accompanied by noise or thunder and lightning.

WE have received the reports of the Mining Surveyors and Registrars, Victoria, for the quarter ending 30th June, 1870. In some instances steady improvement is reported, but there are also numerous complaints of a considerable falling off in the yield of gold. The total number of miners employed in the colony during the quarter was 60,267. Of these, 28,227 Europeans and 15,478 Chinese were employed as alluvial miners, 16,500 Europeans and 62 Chinese as quartz miners. In alluvial mining 399 steam-engines of 9,657 aggregate horse-power were used in winding, pumping, &c.; and in quartz mining 701 steam-engines, of 13,283 aggregate horse-power. The approximate value of the mining plant was 2,144,727*l.* 1,021½ square miles of auriferous ground were actually worked upon, and 2,782 distinct quartz reefs were actually proved to be auriferous. The Mining Surveyors and Registrars are unable to obtain perfectly full information respecting the quantities of quartz, &c., crushed or operated upon; but the following is a summary of the leading facts which they detail:—The total quantity of quartz crushed was 223,285 tons 14 cwt. 36,909 tons 7 cwt. of quartz tailings, cement, and mullock were crushed, and 868 tons 15 cwt. 2 qrs. of pyrites and blanketings were operated on. The average yield of gold per ton from quartz was 9 dwt. 11·38 gr.; from quartz tailings, cement, and mullock, 3 dwt. 0.39 gr.; from pyrites and blanketings, 2 oz. 6 dwt. 13·37 gr. The total yield of gold from quartz was 105,775 oz. 18 dwt. 19 gr.; from quartz tailings, cement, and mullock, 5,566 oz. 9 dwt. 1 gr.; from pyrites and blanketings, 2,022 oz. 7 dwt. 18 gr. Appended to the reports is a careful geological map of portion of the Durham Lead, with a paper on the subject by Mr. Reginald A. F. Murray. Mr. Murray describes the physical and geological aspects of the district traversed by the Durham Lead, discusses the question whether it is or is not the main outlet of the Golden Point Gutter, and gives a brief account of the claims lying within the portion of the lead immediately under consideration.

IF we may judge from its last annual report, the proceedings of the Bombay Geographical Society are not much enlivened by the efforts of its members. During the entire session not a single member had favoured the Society with any original communication. For some time the Society has been considering the propriety of getting compiled an Anglo-Vernacular Index of Indian Geographical names. The scheme would supply a want that has long been felt, but it does not appear to have received much encouragement from the authorities, to whom an appeal for aid in the matter was addressed. The "Transactions" of the Society for the year ending December 1869, are made up of some interesting notes on Annesley Bay, by Mr. Edwin Dawes, and a brief paper by Mr. J. U. Yajnik, on the Hot Springs of Lasúndrá, in the Kairá Zillá.

ANOTHER proof of the desirability of earnest experiments in the widespread cultivation of economic plants is shown in the success which has attended the introduction of tobacco in some of our own colonies. Samples of Latakia tobacco grown in Jamaica have been submitted for approval in London, and reported upon favourably; and from India we hear that the seeds of the best varieties are being distributed in the districts most suited to the cultivation of the plants. From Natal, a sample has just been received, and submitted to an eminent firm of tobacco brokers in the City, who report that it is a very near approach to what colonial tobacco should be. It is of good substance, and of a fair light brown colour, and would, if carefully packed, in all probability fetch a price of from 5*d.* to 5½*d.* per lb. in bond, and would meet with a ready sale to a rather large extent in the London market. The principal requirements of a good tobacco are brightness of colour and dryness of condition, the latter being most essential in consequence of the high rate of duty. Green and imperfect leaves should be excluded, and great care should be taken in packing, so as not to injure or crush the leaves. An important point in the curing of tobacco is to ensure its burning well, and holding fire. A good opportunity presents itself at the present time for the introduction into our markets of colonial tobacco, owing to the general scarcity both of the continental supplies and of other kinds usually substituted for the American product in the manufacture of cut tobaccos.

AS an instance of the rapidity with which introduced plants spread, when soil and climate are congenial to their habits, we may point to the *Euphorbia prostrata*, Ait, a little annual weed in Jamaica and Trinidad, which became introduced by chance about ten years since into a garden in Madeira, situated some 400 feet above the sea; from this spot it has rapidly spread down the steep road to the town; while up the other hills, separated by deep ravines from that down which it came, it has scarcely crawled at all, a downward course apparently being far easier for it than an upward one. It has, however, slowly crept up another hill at the rate of about ten feet a year. The seeds, which are described as ecarunculate, with sharply tetragonal palæ, transversely keeled, are well adapted for sticking to the clothes of travellers, and to be carried about, so that we might well expect the plant to crop up in all directions. Mr. Lowe says that it is now to be found everywhere in Funchal below 500 feet.

THE pods of the Ground Nut (*Arachis hypogæa*) are frequently to be seen in the windows of small shops in the poor neighbourhoods of our large towns, where they are chiefly purchased by children, and are known to them as "monkey nuts." Their chief use, however, is for the expression from the seeds of a light coloured bland oil, said to be extensively used for mixing with olive oil; and we have even heard of the seeds being parched and used as a substitute for coffee, but we now read that in America they are used for making chocolate (so called); for this purpose they are beaten up in a mortar and the mass compressed into cakes, and it is said to form a most agreeable chocolate without a particle of true cocoa. More than this, the Americans prepare the seeds as a dessert sweetmeat by parching them and beating them up with sugar.

THE disasters of a ship have made the Peruvians acquainted with the situation of a new guano island in the South seas. This is called "Baker's Island," in 12° N. lat. and 176° E. long. It has some smaller islands near, and is surrounded by coral reefs, on which thirty wrecks have been counted. The island has been taken possession of by a North American company, and is peopled by three Americans, of whom one is the governor, and about a hundred Kanakas. The cargo of the English barque *Bornea*, bound to London, consisted of a yellowish earth, which the Peruvians say has no smell of ammonia, but may sell at a profit to mix with Peruvian guano.

THE *Boston Journal of Chemistry* says that large factories have been established in New Orleans, Buffalo, and Brooklyn, for making grape sugar from corn. The latter is steeped in weak soda lye, for the purpose of softening the husk and gluten; and is then ground wet, and run through revolving sieves to separate impurities. It is afterwards made to flow through ways or troughs, in which the starch gradually settles as a white powder. The wash water is run into a large cistern, and allowed to ferment and produce a weak vinegar. The starch from the troughs is put wet into the mash-tub, and treated with water containing one per cent. of sulphuric acid, for eight hours. The acid is neutralised with chalk or carbonate of lime, and the liquid evaporated to get rid of the gypsum, and afterwards further evaporated in vacuum pans, and run into barrels ready for crystallisation.

MATERIAL changes are officially reported from the Bay of Talcahuano, in Chile, which was surveyed by Captain Fitzroy. The commander of the Chilean war steamer *Ancud* now reports that the water throughout the entire extent of the bay has increased from two to two-and-a-half fathoms. This represents great geological change. The rock Fraile, in the Gulf of Arauco, represented on Fitzroy's chart as a sunken rock, is now, however, a small island united to the mainland, opposite to the western bank of the river Tubul, and has rather the shape of a quadrangular pyramid.

THE *Times of India* states that the coal-beds discovered in Bellary are excellent in quality and abundant in quantity. Great anxiety is felt for Dr. Oldham, who said he would eat all the coal found in the Madras Presidency, for the doctor is a man of honour.

FROM a considerable number of observations on the temperatures of the two sides of the body, Mr. Blake draws the following conclusions, which are recorded in the *Medical Times and Gazette* of October 8, 1870:—1. That the temperature of the sides of the trunk under usual circumstances, *i.e.* in health and at rest in a temperate climate, is equal. 2. That under certain conditions the temperature of the left side of the trunk may exceed that of the right. 3. That that excess during exertion in a cool atmosphere averages half a degree F. 4. That that excess reaches its maximum of about one degree F. during exertion under a powerful sun.

THE GEOGRAPHICAL DISTRIBUTION OF DEER[*]

IN his excellent "Geographische Mittheilungen," Dr. Petermann has lately given us several *zoo-geographical* articles, as we may call them—such as those of Dr. Finsch on the distribution of Parrots, and of Freiherr von Heuglin on the Bird-fauna of North-eastern Africa. Both of these memoirs are the products of the highest authorities on the subjects to which they respectively relate, and deserve our warmest commendation. We cannot, however, say so much as to the merit of the paper upon the Geographical Distribution of Deer, which appears in a recent number of Dr. Petermann's journal. The authors of this memoir, which, if properly treated, is on a subject of very great interest, have, we fear, commenced to indulge in "generals" before having sufficiently got up their "particulars." In the first part of their essay they point out the present distribution of the different genera and species of *Cervidæ* over the world's surface, and endeavour to show how they have descended from a common ancestral form. This form they imagine must have been the *Moschidæ*, upon the ground that in order to obtain a deer with horns we must pre-suppose the existence of a deer without horns, and the *Moschidæ* answer this definition. Unfortunately, however, the authors have not yet discovered that their so-called group *Moschidæ* is composed of two forms of animal life that have very little to do with one another. It has been shown most conclusively by the researches of M. Alphonse Milne-Edwards[†] in Paris, and Prof. Flower in our own country,[‡] that the *Chevrotains* (*Tragulus* and *Hyomoschus*), one of the constituents of the *Moschidæ* of MM. Jaeger and Bessels, constitute a family *per se*, quite distinct from the rest of the ruminants, and connecting them with the pigs, and consequently quite distinct from the musk-deer (*Moschus*). In the same way our authors base certain arguments upon the fact of all the typical deer being spotted in the immature state. But, as Dr. Jaeger at least—having been, if we are not misinformed, custos of a zoological garden—ought to know, this is not quite the case, all the *Rusine* deer having their young spotless. Again, arguments are founded upon *Cervus pudu* of Chili living in the Cordilleras, and the other allied species with simple unbranched horns in the plains of South America. But exactly the contrary is the case. *Cervus pudu* is from the low maritime coast of Chili, and one, if not more, of the so-called "*Subulones*" (*C. rufus*) lives high in the Andes of Venezuela and New Granada. From these and other similar instances of erroneous statements which it would be easy to point out, it is, in fact, quite obvious that the authors of this essay have no very special acquaintance with the group upon the distribution of which they treat. We leave it to naturalists to decide whether, under these circumstances, the results arrived at are worthy of much attention. Their theory seems to be that the deer-family reached the New World by an Arctic continent which formerly connected northern Europe with eastern America, and which Dr. Jaeger, in a former paper, has proposed to call "*Arctis*." There are, however, if we are not mistaken, equally good grounds for believing that the numerous, undoubtedly Old-world forms in North America reached it by immigration from North-western Asia.

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[*] "Die Geographische Verbreitung der Hirsche mit bezug auf die Geschichte der Polar-länder." Von Gustav Jaeger und Emil Bessels. (Petermann's Geographische Mittheilungen, 1870.)

[†] "Recherches Anatomiques et Palæontologiques sur la famille des Chevrotains," Paris, 1864.

[‡] "Notes on the Visceral Anatomy of *Hyomoschus aquaticus*." Proc. Zool. Soc., 1867.

HENDERSON'S PATENT STEEL PROCESS

TWO articles written by Mr. W. Mattieu Williams, called "Papers on Iron and Steel—A Costly and Vexatious Fallacy," were published a short time since in NATURE. These papers are considered in this country to be the clearest and ablest that have ever appeared on this subject, setting forth the reason why all efforts heretofore made to produce steel from English cast-iron by partial decarbonisation have failed; that all manipulations have been directed to removing as much as possible the impurities contained in pig iron by oxidation. He was not aware that new agents have been used, combined with oxygen, and that patents had been granted therefor in England, Nos. 318, 1,051, A.D. 1870 (which were not then published) for combining fluorine with oxygen, and fluorine combined with titanic acid, or with titanium and oxygen.

The new patent process for the production of steel by the partial decarbonisation of cast-iron consists in the combined use of fluorspar or other fluorides and titanic acid, applied to cast-iron at the melting temperatures, preferably in reverberatory furnaces. Fluorine is given off from the fluorspar, and is a more powerful agent for the removal of silicon than oxygen, and removes it almost entirely from the cast-iron before the reactions with the carbon begin; the phosphorus and sulphur are next acted upon and removed in the order they are named by means of the combined action of fluorine and titanic acid or fluorine, titanium, and oxygen, and lastly the carbon is removed. The fluorine is derived from fluorspar combined with iron ores containing titanic acid in such wise as ensures simultaneous action of the fluorine, titanium, and oxygen upon the cast-iron; and by reason of the affinities of these substances for silicon, phosphorus, sulphur, manganese, arsenic and carbon, these substances are taken from the iron in the form of vapour and slag, leaving the purified metal in the condition to be hammered or rolled as merchantable steel.

English pig-iron may be made direct into steel by the new process; and with the large class of irons smelted from hæmatites and specular ores with good fuel, pure steel may be made that will be entirely desiliconised, dephosphorised, and desulphurised, and contain but the required amount of carbon to form steel of any particular degree of hardness, by arresting the decarbonisation of the metal whilst undergoing conversion.

Experiments were made in May last on a large scale at Messrs. Park, Brothers and Co.'s Steel Works, in Pittsburg, in boiling puddling furnaces fettled with such oxides, but without puddling or labour of stirring the iron during conversion; the only labour was that of "balling" and removing it from the furnace after the conversion was completed. The mode of application in the above-named establishment was 125lb. of titaniferous iron ore (similar to the Norwegian) and 50lb. of fluorspar, both in a powdered state, and then charging them evenly over the sole of the furnace. 475lb. of No. 3 charcoal pig-iron, similar in quality to English hæmatite of Barrow, was then charged upon them, and, when melted, was allowed to remain without stirring or puddling. As soon as the pig-iron melted, reactions began between the fluorspar and the titaniferous iron ore, and the silicon and phosphorus contained in the pig-iron. To ascertain exactly the conditions upon which the changes are made in the cast-iron whilst under treatment, samples were taken from the bath of liquid iron. The analysis of the first sample shows that the operation of the new process is entirely different from any other process, inasmuch as the silicon is entirely removed at the early stages of the process; and, with the silicon, phosphorus is also taken from the iron, and the carbon is changed from the graphitic to the combined form. Other samples were taken from the bath at intervals of ten minutes. The analysis of the first sample made it evident that most of the later ones were steel. This has since been confirmed by analysing a specimen taken thirty minutes later from the bath, and by the treatment of them as steel, it having been found that they possess the properties of steel, forging well, and tempering and hardening according to the various degrees of carbon contained in them. At the end of the operation, the charge became wrought-iron, by removal of *all* the carbon. This iron forges, welds, and is neither red-short nor cold-short.

The analyses of the first and fourth samples taken from the bath have been made by Mr. W. M. Habirshaw, analytical chemist, of 36, New Street, New York, and are annexed. Also, analyses of Messrs. Sanderson's and Krupp's cast-steel, and Hoop L Dannemura-Swedish bar iron are annexed for comparison, taken from Dr. Percy's "Iron Metallurgy."

First Sample. Refined Cast-iron, taken 40 minutes after fusion.		Fourth Sample. Steel, taken 30 minutes later.	
Carbon, combined	2·7144	Carbon, combined	0·2172
do graphite	traces	do graphite	none
Slags (silicates)	none	Slags (silicates)	none
Silicon	0·0046	Silicon	none
Phosphorus	0·0349	Phosphorus	none
Sulphur	0·1073	Sulphur	very minute trace
Titanium	trace	Titanium	none
Fluorine	none	Fluorine	none

The presence of sulphur in the refined cast-iron is mostly due to sulphurets mixed with the fluorspar, which was used in the condition in which it was taken from the mine, there being no convenience at hand for dressing it.

Later experiments with other kinds of pig-iron, with impure or fluorspar not dressed, show that at the stage of the process where it becomes refined cast-iron, the increase of sulphur from this cause amounts to 0·1051 per cent.; which with "dressed" or pure fluorspar, would be 0·0022 per cent. of sulphur for the refined cast-iron of the foregoing analysis, instead of 0·1073 per cent.

It will be evident to the practical metallurgist that the refined cast-iron, when treated with *pure* fluorspar, becomes steel of superior quality when decarbonised below 1·90 per cent of carbon.

Hoop L Bar-iron.		Sanderson's Cast-steel.		Krupp's Cast-steel.	
Carbon	0·087	Carbon	not deter.	Carbon	1·18
Silicon	0·115	Silicon	0·24	Silicon	0·33
Phosphorus	0·034	Phosphorus	0·02	Phosphorus	0·02
Sulphur	0·220	Sulphur	0·05	Sulphur	none
Manganese	none	Manganese	0·03	Manganese	trace
Arsenic	trace	Cobalt, nickel	none	Cobalt, nickel	0·12
Cobalt, nickel	none	Copper	none	Copper	0·30
Copper	none	Aluminium	0·12	Aluminium	none
Aluminium	none				

From the foregoing analysis it will be seen that the *refined cast-iron* of the patent process contains but $\frac{1}{26}$ part of the silicon, less than one half the sulphur, and about the same amount of phosphorus, as compared with Hoop L bar-iron. This is the most celebrated wrought-iron made in the world, and is used exclusively for making steel, and sells at 24*l.* per ton at Sheffield.

The analysis of the steel of the patent process shows, as compared with Sanderson's and Krupp's, that while the latter are alloyed with carbon, silicon, sulphur, phosphorus, &c., the *steel* of the new process is practically *pure* iron and carbon.

In the articles before referred to, written by Mr. Williams (and from his intimate practical and scientific knowledge no better authority can be had), he says:—"To make *perfect* steel they take out *all* these latter, and leave nothing but pure iron and carbon. Absolute perfection is not, of course, practically attainable in steel making, but it is approximated in the same degree as the purification of iron from everything except carbon is effected."

Persons wishing to satisfy themselves of the value of the process, by a test, should use the ordinary boiling puddling furnace, fettled with the purest red hæmatite or specular ores, containing the least silica, ground and applied wet, with the ordinary "bottom" made in the usual way: and when perfect steel is required, that contains neither silicon, phosphorus, nor sulphur, good pig-iron should be taken, such as Nos. 1 and 2 Barrow, or West Cumberland hæmatite; and treated with pure fluorspar, and titaniferous iron ore from the Bay of St. Paul, in Canada, containing over 43 per cent. of titanic acid, or from Norway, containing over 40 per cent. of titanic acid; using 48lb. of fluorspar to 118lb. of the ore. They should be ground to fine powder, and mixed, and used dry, and charged evenly over the sole or bottom of the furnace; and 475lb. of pig-iron should be charged upon them, and the furnace closed tight so as to exclude all air, for about 70 minutes. The fire should be kept to the highest temperature. After the metal has been in the furnace this period, samples should be taken from the bath at intervals of five minutes. It will be found that a little experience will soon determine the proper time to stop the process.

It is best *not* to allow the workman attending the furnace to stir or puddle the metal during the conversion, as the fluorspar and titaniferous ore become viscid by the heat of the furnace by the time the pig-iron melts, and, if left alone, will remain on the bottom of the furnace until decomposed by the reactions of the process, when they pass through the iron as vapour and slag, and purify it more effectively than can be done by stirring or puddling; and the slag serves to protect the surface of the metal from the effects of the sulphur in the fuel. The only labour that should be *allowed* is that of removing the steel from the furnace at the required stage of conversion.

The time in conversion of the steel, from the charging of the pig-iron, is one hour and 30 to 40 minutes; this time may be shortened 30 to 40 minutes, by previously refining the cast-iron of all its silicon and most of its phosphorus, by a shorter and more economical process (Patent, No. 1,051) with hæmatites and specular ores with fluorspar, which will become the subject of a future article.

JAMES HENDERSON

New York

SOCIETIES AND ACADEMIES

LONDON

Geological Society, November 9.—Mr. Joseph Prestwich, F.R.S., President, in the chair. Lieutenant Reginald Clare Hart, R.E., Brompton Barracks, Chatham; Lieutenant James Frederick Lewis, R.E., Brompton Barracks, Chatham; and Mr. M. F. Maury, Jun., 1300 Main Street, Richmond, Virginia, U.S., were elected Fellows of the Society.—(1.) "On the Carboniferous Flora of Bear Island (lat. 74° 30′ N.)," by Professor Oswald Heer, F.M.G.S. The author described the sequence of the strata supposed to belong to the Carboniferous and Devonian series in Bear Island, and indicated that the plant-bearing beds occurred immediately below those which, from their fossil contents, were to be referred to the mountain limestone. He enumerated eighteen species of plants, and stated that these indicated a close approximation of the flora to those of Tallowbridge and Kiltorkan in Ireland, the greywacke of the Vosges and the southern Black Forest, and the *Verneuilii*-shales of Aix and St. John's, New Brunswick. These concordant floras he considered to mark a peculiar set of beds, which he proposed to denominate the "Ursa-stage." The author remarked that the flora of Bear Island has nothing to do with any Devonian flora, and that consequently it and the other floras, which he regards as contemporaneous, must be referred to the Lower Carboniferous. Hence he argued that the line of separation between the Carboniferous and Devonian formations must be drawn below the yellow sandstones. The presence of fishes of Old Red Sandstone type in the overlying slates he regarded as furnishing no argument to invalidate this conclusion. The sandstones of Parry Island and Melville Island are also regarded by the author as belonging to the "Ursa-stage," which, by these additions, presents us with a flora of seventy-seven species of plants. The author remarked upon the singularity of plants of the same species having lived in regions so widely separated as to give them a range of 26½° of latitude, and indicated the relations of such a luxuriant and abundant vegetation in high northern latitudes to necessary changes in climate and in the distribution of land and water.—Sir Charles Lyell remarked that the Yellow Sandstones of Dura Den in Fife, and of the county of Cork in Ireland, contain *Glyptolepis* and *Asterolepis*, genera of fish exclusively Devonian, or belonging to the middle parts of the Old Red Sandstone—also the genus *Coccosteus*, which is abundantly represented in the Middle Old Red Sandstone, and sparingly, or only by one species, in the Carboniferous formation. The evidence derived from these fishes inclined him to the belief that the Yellow Sandstone, whether in Ireland or Fife, should be referred to the Upper Devonian, and not to the Lower Carboniferous, as Sir Richard Griffiths contended, and as Heer now thinks. As to the argument founded on the plants, he considered it an important and truly wonderful announcement, that many well-known Carboniferous species are common to Bear Island (in lat. 74° 30′ N.), in the Arctic regions and to Ireland and other parts of Europe (26° of latitude farther south). But fossil plants are supposed to have a wider range in space and time than fossil-fish; and we know that the cryptogamic flora of the ancient coal is remarkable for the wide horizontal spread of the same species, extending from North America to Europe, so that we need not be surprised if many species should extend vertically from the Devonian into the Carboniferous strata. Mr. Carruthers remarked on the bearing of the paper on the Kiltorkan beds, and considered that Dr. Heer had completely established the correlation of the deposits. He differed, however, as to the numerical proportions of the species. He could not recognise *Cyclostigma* as a genus, but considered it founded on insufficient grounds, in which view Prof. Haughton now agreed. It was, in fact, founded on fragments of the bark of *Lepidodendron Griffithsii*, Brongniart, to which species the *Lepidodendron* indicated by Prof. Heer as *L. veltheimianum* really belonged. Other detached portions of this same plant had been described by various authors under no less than seven different specific names, and referred to nearly an equal number of distinct genera, and Prof. Heer had reckoned these as species in his comparison of the Bear Island and Irish floras. Prof. Heer had been led, chiefly by the erroneous determination of the Kiltorkan *Lepidodendron* by the Irish palæontologists, to refer these beds to the Carboniferous rather than to the Devonian formation, the Kiltorkan fossil having been established as a very distinct species by Brongniart and Schimper. Mr. Carruthers considered that both the Irish and Bear Island deposits belonged to the Devonian. Mr. Boyd Dawkins pointed out that the proximity of land was exhibited by the presence of terrestrial plants in the deposits, and believed that this might have much to do with the difference in the proportion in the beds. As the marine fauna decayed more rapidly than the terrestrial, it was preferable for classificatory purposes. He mentioned forms of vegetable life which had been recently discovered in America in beds of Cretaceous age. He did not believe that corals could have existed in those high latitudes under anything approaching to the present condition. Prof. Nordenskjold had failed to discover any traces of glacial action in these beds; and the question arose whether there had been any change in the position of the Pole or in the radiated heat of the sun.—(2.) "On the Evidence afforded by the Detrital beds without and within the North-eastern part of the Valley of the Weald as to the mode and date of the Denudation of that Valley." By Mr. S. V. Wood, jun., F.G.S. The author commenced by discussing the various hypotheses that have been proposed to explain the denudation of the Weald Valley. In his opinion the upheaval of the district took place in Post-glacial times, and subsequently to the deposition of the gravels of the Thames Valley, of East Essex, and of the Canterbury heights; and the denudation was effected chiefly by tidal erosion during gradual upheaval in an inlet of the sea, aided by the action of fresh water flowing into this inlet from the north by streams draining the land which now constitutes the counties of Middlesex and Essex. The chief evidence in favour of his views is as follows:—1. The absence from the glacial beds of Essex of any débris representing a considerable denudation of the Weald during the glacial period, and the probability that the Wealden area was beneath the sea during the deposition of the Boulder Clay. 2. The comparative absence of Lower Cretaceous or Hastings-sand materials from the Post-glacial gravel-sheets outside the north of the Weald. 3. The impossibility of reconciling the presence of Tertiary pebbles in certain Weald-gravels with an origin by means of streams flowing in the direction of the present rivers. 4. The antagonism between the character of the major valley of the Weald and that of any excavation producible by the agency of rivers. 5. The persistence of the old coast contour with the river-drainage entering it from the north. 6. The existence of a cause, in the shape of an isthmus at Dover, sufficient to induce a strong tidal scour. Mr. Godwin-Austen thought that the author had done his theory injustice in presenting only a portion of the Wealden area for consideration. He remarked that phenomena similar to those of the Weald were to be found in various parts of Western Europe. He was glad to find that Mr. Searles Wood did not regard the escarpment as representing marine cliffs; but he did not attach sufficient weight to the absence of any material of marine origin at their base, so that there was no evidence of the presence of the sea within the Wealden area. He differed wholly from the author as to the age of the gravels, for beneath the gravels were silty beds containing elephant remains. These gravels he was inclined to refer to a glacial period, as they contain blocks such as could only have been transported by the agency of ice. The elephants found in the valley of the Wey are of the species (*E. primigenius*) which also occurs in the Selsea beds; and he believed both to be of glacial age. As to the theory of the denudation of the Weald, he professed himself a convert to the views of Messrs. Foster and Topley, and cited what was now going on in Heligoland in illustration of atmospheric denudation.—Mr. Whitaker observed that the present absence of gravels along parts of the valley of the Thames affords no proof of their not having formerly existed. He pointed out the soft and friable nature of most of the rocks of the Wealden, which would account for their absence in the gravels. The only really hard rock was the Chert of the Lower Greensand, which was abundant in the gravels of West Kent. Angular flints occurred at the base of the chalk escarpment wherever it had been carried back by denudation. The major valley of the Weald had been spoken of, but he denied that any such valley existed; it was merely a series of numerous small valleys. He could not conceive the rivers flowing against the dip of the strata, as supposed by Mr. Wood. He did not agree in the view of the denudation of the Weald being such an enormous affair, but thought that it might be due to comparatively small causes.—The President pointed out that beyond Southend there was a section precisely similar to that of Grays. It was a mistake to suppose pebbles from the Wealden area did not occur in the Thames gravels.

He thought that much of the denudation of the Wealden area might have taken place before the glacial period. The presence of Tertiary pebbles in the Wealden area might readily be accounted for by their presence at the edge of the escarpment. Mr. Searles V. Wood, jun., in reply, justified himself for having limited his observations to the northern part of the Weald, as it was there only that it could be brought into juxtaposition with the glacial beds. He maintained that, under certain circumstances, no beaches or marine beds were formed at the base of sea-cliffs. He pointed out that in Post-glacial gravels large blocks of rock were frequently found, and protested against limiting all ice-transport to the glacial period. He could not recognise the Selsea beds, with 150 living species, some of southern character, and none extinct, as glacial. He did not acknowledge the alleged softness of the Wealden rocks—The Earl of Enniskillen sent for exhibition a fragment of Lias Limestone from Lyme Regis perforated by *Pholades*.

Entomological Society, November 21.—Mr. Alfred R. Wallace, F.Z.S., &c., President, in the chair. Exhibitions of *Lepidoptera* were made by Mr. Bond; of *Coleoptera* by Mr. Albert Muller and Prof. Westwood; and Mr. F. Smith exhibited *Phora florea*, a Dipterous parasite in the nest of the wasp. The following paper was read:—"Descriptions of some new diurnal *Lepidoptera*, chiefly Hesperiidæ," by Mr. A. G Butler.

Ethnological Society, November 22—Prof. Huxley, President, in the chair. Mr George Macleay was announced as a new member. Mr Edgar Layard made some remarks upon a collection of stone implements which he has recently brought from the Cape of Good Hope. Some polished celts from the Naga Hills, between Assam and Burmah, were exhibited, and Lieut. Barrow's notes upon them were read.—A paper was then communicated by Dr. Bleek "On the Concord, the Origin of Pronouns, and the Formation of Genders or Classes of Nouns." The author believes that the classes or genders in the sex-denoting languages originally depended, not upon the meaning of the nouns, but upon their representative particles, which, in these languages, were primiarily at the end of the nouns. These genders were, from an originally large number, gradually reduced, until in the Aryan languages they were mainly two—one with the representative element, U, which is called the *masculine* class, and the other with the representative element, A T I, which is named the *feminine* class. The *neuter* appears to be a later development, into which, however, an original common plural gender, with the termination, A N I, may have been incorporated. To these endings the case-terminations were affixed, and through pressure of the latter the original marks of gender have frequently been obscured. The concord was at first due to the presence of these representative elements of the nouns in their pronominal character. Mr. Hyde Clarke, in eulogising this paper, said that he had by independent investigation arrived at some of the results detailed by the author. The speaker insisted upon the necessity of extending our philological studies beyond the Indo-European languages.

MANCHESTER

Literary and Philosophical Society, November 15.—Mr. E. W. Binney, president, in the chair. "On the Temperature Equilibrium of an Enclosure containing a Body in Visible Motion," by Prof. Balfour Stewart, LL.D., F.R.S. It has been established that in an enclosure containing bodies which are all at the same temperature, and at rest, the same amount of heat enters any surface forming part of the walls of the enclosure as leaves it in the same time, so that the body, of which this is the surface, neither gains nor loses heat. It is also known that if we take, not the outer surface of such a body, but any plane passing through its substance, say for instance one parallel to its outer surface, then, as much heat passes across this plane going into the body, as passes across it going out of the body in the opposite direction; and further, this equilibrium of heat is known to hold separately for every one of the individual rays of which the whole heterogeneous radiation is composed. The effect of the motion of a body in altering the wave-length of the radiated light is also well known. In consequence of this, if a cosmical mass, such as a star or nebula, should be formed of incandescent hydrogen, and be at the same time rapidly approaching the earth, the light which strikes the earth will not be the double line D, but a line more refrangible than it, and therefore this light will be able to pass through a mass of ignited sodium vapour at the earth's surface without suffering absorption, while, however, the light emanating from the sodium vapour will still be the double line D. In such a case, even if the star and the terrestrial sodium vapour should both be of the same temperature, yet the light radiated by the latter will not be the same in quality as that absorbed. This instance would appear to show that the equilibrium which holds in an enclosure of uniform temperature when all the substances are at rest does not hold when some of these are in visible motion, and that if in that enclosure there be a body moving towards or from the surface of the enclosure, the heat which enters the surface from the moving body will not be the same as that which the surface gives out. Suppose for instance that the walls of the enclosure are made of glass, and that the temperature of the whole enclosure including that of the moving body is 0° C., then, were the whole at rest, the heat which strikes the glass surface will all be absorbed at a very short distance below the surface, and in like manner the heat radiated by the glass will all emanate from a short distance below the surface. But let us now suppose, to take an extreme case, that the moving body is approaching one of the glass surfaces so rapidly that the heat which it emits has been so much increased in refrangibility as to enter the boundary of the visible spectrum. Then, while the heat radiated by the glass will still continue to proceed from a very short distance beneath the surface, the heat absorbed by the glass from the moving body will be able to penetrate to a very considerable depth beneath the surface of the glass. The outer layer of glass will thus lose, while the inner layer will gain heat. Now, it is possible to conceive an enclosure with a fixed diaphragm, and containing a revolving body, so arranged that the heat which leaves it in the direction of a certain part of the enclosing surface, shall always be given out by that part of the revolving body which is moving towards the surface; while, on the other hand, the heat given out by the revolving body to another surface, shall be given out when the revolving body is moving from that surface. There will thus be a want of temperature equilibrium among the various layers, those near the surface being somewhat different in temperature from those beneath. But when we have a temperature difference of this kind, have we not acquired the power of converting heat into work? It would thus appear at first sight that the mere presence of a moving body has given us the power of obtaining work from an enclosure all of whose particles were originally at the same temperature. This appears however to be opposed to the theory of the dissipation of energy, and in consequence we are induced to think there must be some error in the assumption. Now, does not the unwarranted part of the hypothesis consist in our supposing that the revolving system can continue to revolve without losing part of its visible motion? When two moving bodies approach or recede from each other, is it not possible that each loses a small part of its visible energy, while at the same time there is a surface disturbance produced in both? It might be said that, believing in a medium pervading all space, we were prepared for a stoppage of motion of this nature, and that there is therefore nothing gained by the supposition which has been made; but it might be replied that by looking at the problem in the above light, we appear to connect this stoppage of motion with other facts, besides being made aware of a source of surface disturbance when cosmical bodies approach or recede from each other.—Postscript added 19th November.—If we imagine a stoppage of the motion of cosmical bodies of the nature above described, then if the two approaching bodies be exactly equal and similar, either extremity of the medium between them will be similarly affected by the motion derived from the approaching bodies; but if these bodies are unequal, the two extremities of the medium will be dissimilarly affected.

Microscopical and Natural History Section, October 10—Mr. Joseph Baxendell, President of the Section, in the chair. Mr. Joseph Sidebotham read the following paper:—"On the Variations of *Abraxas grossulariata*." The variations in animals and plants are of great interest, and each contribution to the store of facts accumulated relative to these variations, their causes and limits, is of value in determining the identity and limits of species, in whatever way we interpret the word *species*. *Abraxas grossulariata* is probably one of the most variable insects we possess in this country in colour and markings, and it would be quite pardonable in any one not well acquainted with it, were he to split it up into four or five species, but although it varies in colour and markings in such a great degree, all these varieties are joined together by gradual steps, and yet no step is found to join it to the next species on our list, *Abraxas ulmata*. The larvæ of this species will feed upon the leaves of most trees and shrubs, and are therefore easily experimented upon, as to whether the changes in food influence the colour or markings. So far as my own experiments, and I believe those of others are concerned, no difference whatever can be detected from the varieties of food, except in size. That long-continued changes of food through many generations might have a perceptible effect, is however more than probable. The type form of this moth is too well known to require description I will therefore exhibit a drawer of specimens, having the type form in the centre, the various forms radiating from it in steps, in one line ending in white, another in black, another in which the white ground runs gradually into brown, and various other marked varieties. We may divide these into the following seven groups :—1 Variation. White, or the spots very few and distant : this leads up to the type form. 2. Spots joined together, forming curves and lines. 3. A variety of intermediate spots and patches. 4. The spots at the border becoming lines, and running towards the base of wings 5. Spots confluent, forming solid black patches over nearly the whole of wings. 6. The spots having the type form, but the white ground tinged with a smoky brown or drab colour, sometimes suffusing the whole of the wings. 7. Spots of the type form, but the ground of wings bright yellow From various experiments with many thousands of larvæ of this species, I have come to the conclusion that these variations are in a great measure hereditary, that one brood of eggs will produce moths of forms in a great measure identical, if the parents be of the ordinary type; if the eggs be the produce of moths of extreme colouring, varying much from the type, then, although the bulk of moths will be marked dark or light as the parents, there will be others of the ordinary type, and also some of the very opposite character of marking, precisely as in many florists' flowers the seeds from those varying most from the original form are known to produce the most marked and opposite varieties. These experiments can only produce approximate results, unless a great number of years could be devoted to them, and in this and many others of our most variable species, it is almost impossible to rear them in confinement beyond the second generation.

November 7.—Mr. Joseph Sidebotham exhibited a series of specimens of *Limobius dissimilis*, from Llandudno, on which the markings were very distinct and perfect, he discovered the species in considerable numbers beneath the flowers of *Geranium sanguineum*.—Mr. Spencer H. Bickham, jun., reported occurrence of *Myosurus minimus*, L., in plenty at Vale Royal, near Northwich, which species, he believed, had never previously been noticed in the neighbourhood. — Mr. Bickham then exhibited a series of specimens of *Polygonum minus*, Huds, collected at Mere and the surrounding district; he stated that he had searched for *P mite*, Schrank, but without success, and believed with Mr. Hunt, that luxuriant specimens of *P. minus* had been mistaken for it : on the other hand he called attention to the fact that in 1859 Mr. John Hardy, to whom Mr. Bailey had previously alluded, distributed specimens of *P. mite* from Mere, through the Thirsk Exchange Club, and on this authority Mr. J. G. Baker, the Curator, remarked in the report, "new to the Mersey Province." It seems doubtful also whether *Alopecurus fulvus*, reported from the same locality, has not been erroneously recorded, peculiar states of *A. geniculatus* having been mistaken for it. As, however, it was found in considerable quantity at Oakmere in 1868, it appears probable that it may occur elsewhere in Cheshire.

LEEDS

Field Naturalists' Club (Young Men's Christian Association), October 24.—The first meeting of the winter session took place this evening, Mr. Coates in the chair. In entomology, Mr. Liversedge exhibited specimens of *Latyrus ægeria*, *Argynnis selene*, *Anthrocharis cardamines*, and *Pamphila sylvanus*, all collected in this neighbourhood.—Mr. Turner exhibited a variety of insects taken near Selby, including *Cerura vinula*, *Triphæna fimbria*, *Argynnis paphia*, and *Saturnia carpini*. In oology, Mr. Coates brought the nest and egg of the ring ousel found at Ilkley.—Mr. Beevers and Mr. Taylor were the principal exhibitors in the conchological branch, Mr. Beevers exhibiting *Unio pictorum* from Went Vale, *Cyclostoma elegans*, Thorparch, and *Limnæa palustris* var. *corvus*, Knaresbro.—Mr. Taylor exhibited *Limnæa glabra*, *Helix virgata* var. *submaritima*, *Planorbis corneus* var. *albina*, and a small collection from Wisconsin, U.S.

November 7.—Mr. W. Coates in the chair. Mr. Taylor read a short paper describing a conchological visit to Boston Spa during the present month. Amongst the specimens taken were *Cyclostoma elegans*, *Helix lapicida*, *H. cantiana*, and *Pupa marginata*, specimens of which species, and a number of others, were exhibited.—Mr. Wood brought for exhibition a fine collection of shells, illustrating the Pontefract district.—Mr Roebuck exhibited several species of shells taken in the neighbourhood of Harrogate —Mr. Scholefield exhibited the American mosquito and a fine specimen of *Bombyx cynthia* —Mr. Denny brought for inspection a quantity of wheat infested by the wheat weevil, and a specimen of the death's-head moth, *A. atropos.*—Mr. Liversedge exhibited a number of insects taken in the immediate neighbourhood, including *Lasiocampa quercus*, *Smerinthus popule*, *Nemeobius lucina*, and *Lycæna alsus*. The next meeting was to be held November 21st, when a paper was to be read by Mr. Acomb, "On geology as a study."

NORWICH

Norfolk and Norwich Naturalists' Society, October 25. —The President, the Rev. J. Crompton, in the chair. A most elaborate and interesting paper was read by Mr. F. Kitton, "On Diatomacea and the lower forms of vegetable life as revealed by the microscope" The lecture, for such it may more properly be termed, was illustrated by diagrams, showing some of the most familiar as well as most peculiar forms of Desmids and Diatoms ; and at the close Mr. Kitton exhibited a series of very beautiful photomicrographs, of similar objects, executed by Dr. Maddox. The Chairman, in offering to Mr. Kitton the thanks of the Society, and especially of the members present, for the time and labour he had devoted to their instruction, alluded in complimentary terms to the high reputation he had already attained throughout the scientific world, by his persevering researches in this particular branch of natural history; his skill as a microscopist being equalled only by the extreme accuracy of his descriptions of the most intricate and minute organisms. At the request of the meeting Mr. Kitton consented to his paper being published *in extenso* in the Transactions of the Society.

EDINBURGH

Royal Physical Society, November 23.—Prof. Duns in the chair. The retiring president, Professor Duns, delivered an address, in which he referred to the early history and past achievements of the Society. A hundred years ago, eighteen students of nature banded themselves together for mutual profit in the pursuit of natural science, under the name of the Physical Society. Here is the first list of the ordinary members, Session 1770-1771 :—William St. Clair, M.D. ; David Young, M.D. ; Thomas Melville, Thomas Smith, James French, James Wood, Robert Stewart, Alexander Muir, James Dick, Henry W. Tytler, Malcolm Macqueen, Arthur Taafe, Daniel Gibb, Thomas Thorburn, James Webster, George Home, William Manuel, and William Keir. The names deserve to be brought out of the mists of 1770, and set before the Society in the light of 1870. The period was one well fitted to quicken young and ardent students, and to lead them to long to win their spurs in work closely kindred to that in which others were distinguishing themselves. Eight years previously, Black had made public his theory of Latent Heat, and two years before he had been inducted to the Chemistry Chair in Edinburgh. The influences of the day were bearing in on Hutton's mind, in which "The Theory of the Earth" was shaping itself into compactness and symmetry. Ray's *Synopsis*, Willoughby's *Ornithologia*, Lister's *Mollusca*, and Ellis's *Corallines*, were before the public. But these dealt with British forms. Scotland was still in the rear Nothing had been done to purpose for Scottish forms, except in the *Scotia Illustrata* of Sibbald, most valuable at the time, no doubt, but also most suggestive of how much still remained to be accomplished. It was in such circumstances the Physical Society began, and more than ten years elapsed before the foundation of the Royal Society of Edinburgh. In 1788 the Physical obtained a Royal Charter, and assumed the name it now bears. Its meetings were held for many years in the Royal Physical Society Hall, Richmond Court, a building which stood on a site now occupied by a chapel. From the outset its influence over working naturalists was great and beneficial, and it ultimately absorbed other kindred associations, which had been at different times set up in Edinburgh. The Chirurgo-Medical, its senior by a few years, joined it in 1788, the Hibernian Medical, in 1799, the Chemical, in 1803; the Natural History, in 1812;

the Didactic, in 18[illegible]3; and the Wernerian, in 1858. Between 1771 and 1788 many well-known names occur among its list of members—Benjamin Bell, Professors Alexander Munro, J. Hope, Joseph Black, Francis Home, James Gregory, Alexander Hamilton, and W. Hamilton (Glasgow). In 1802, Dr. Barclay and Charles Bell; in 1814, David Brewster. By its union with the Natural History Society it enrolled among its members the botanists, James Edward Smith and Robert Brown, and another, great in almost every department of science, literature, and law, Henry Brougham. Brown's papers on the "Botany of Angus," and on the "Sexes of Plants," are models in this department, and Brougham's on "Thunder" and "Combustion" will well repay a careful perusal. One other notice: in 1828 the Plinian approached the Royal Physical with proposals to unite, and both societies appointed influential committees, with powers to form a union. After much consultation, they reported "that the union of the societies had been admirably accomplished" But the Plinians, after the union was consummated, rued the act, and refused to associate with their lawful head. The Plinian lived on for a season in cold estrangement, and gradually passed into the dark On the list of ordinary members of the Plinian is the name "Charles Darwin, Shrewsbury, Nov. 26, 1826." The history of the Royal Physical Society is substantially that of Scottish zoology. The latter could not be written without the former. I have only to choose these names from the list of our presidents to make good this remark:—Robert Knox, Captain Thomas Brown, Edward Forbes, Robert K. Greville, James Y. Simpson, John Coldstream, George Wilson, John Goodsir, Alexander Bryson, William Dick, Hugh Miller, Sir John Graham Dalyell, and John Fleming. In these men was embodied the great characteristic of our society. They were all practical naturalists. In November, 1849, Professor Fleming delivered the opening address, in which he urged the expediency of steps being taken by the society to bring before the Government and country the great want of a general national museum for the native products of Scotland, and to bring together the other collections in Edinburgh under one roof. Steps were soon taken in these directions by public bodies and by influential individuals. It again fell to Fleming to give the opening address, in 1855, and he could say—"The gratifying intelligence at last reached us that the Board of Trade had resolved to institute an industrial museum for Scotland in Edinburgh." The Society might claim the merit of one of the first agitations for this great national institution. Dr. Duns passed a high eulogium on the researches of Dr. Strethill Wright on the *Cœlenterata* and *Protozoa*, referring to the sensation produced by the deep-sea dredging report, intimating the growth of chalk in one of the dredged localities. But honour to whom honour. In 1861, this note occurs in Dr. Wright's address to this Society. Referring to the oolite and the chalk, he says: "Similar deposits are now in process of formation over vast areas of sea bottom, especially in the Atlantic, Mediterranean, and Australian seas." Since the Society last met it had lost one of its most distinguished members, Sir James Young Simpson. Dr. Simpson was born at Bathgate, Linlithgowshire, on the 7th June, 1811. He sprang from a family long resident in the district, comfortable in worldly circumstances, and noted for their strong mental powers and outstanding individualities. After being educated at the parish school, where for several years he had for a companion the late Prof. John Reid, of St. Andrews, Simpson entered the Arts course of the University of Edinburgh. He commeneed his purely medical studies in 1827, and graduated as M D in 1832. Immediately after graduation he was elected President of the Medical Society In 1833 he petitioned for a seat in this Society, recommended by Edward Forbes. From 1832 to the beginning of 1836, he acted as assistant to Prof. John Thomson, who occupied the Chair of General Pathology in the University. In Session 1839-40 he gave a course of lectures on midwifery, and in 1840 he was elected by the Town Council to the Chair of Midwifery, vacant by the resignation and subsequent demise of Dr Hamilton. Dr. Simpson died on the evening of the 6th May, 1870. Dr. Duns concluded by some apposite observations on the motives that should incite to natural history studies, and the methods by which they should be pursued.

Botanical Society, July 14.—Sir Walter Elliot, President, in the chair.—1 "On Kashmir Morels." By Mr. M. C. Cooke, India Museum, London. The author remarked that it has long been known that truffles and morels are found in N. W. India andKash mir, but no attempt has hitherto been made to determine the species. Some years ago, application was made to the Agricultural and Horticultural Society of the Punjab, and to other sources, for specimens, but without any result. He had, however, lately received, through Dr. J. L. Stewart, a string of dried morels, said to be the morels of Kashmir, and sent by Mr. Baden Powell, of Lahore. This string contains two distinct species, both of them small, and neither of them the *Morchella esculenta* of European markets. The author gave some account of the history of morels as far as known, and concluded by giving scientific descriptions of the supposed two new species from Kashmir. 2. "On the Characters of the Flowers of *Silene maritima* and *Silene inflata*, as regards their Stamens and Pistils." By. Dr. F. Buchanan White. The author had examined 72 plants and 201 flowers of *Silene maritima*; of these, 39 plants were perfectly hermaphrodite, 11 had the stamens abortive, 10 the styles abortive, 11 the styles partly abortive, and 1 with the stamens partly abortive. Of the 201 flowers examined, 122 had three styles and three-celled ovary; 68 had four styles and four-celled ovary; and 11 had five styles and five-celled ovary. 3. "Notes of a Botanical Excursion to the neighbourhood of Perth." By Mr. John Sadler. 4. "Results Obtained from the Cutting and Transplanting of a Plaited Hornbeam Hedge." By Mr. M'Nab. 5 "On the Guachamacan, a poisonous plant growing in the Llanos (plains) of Venezuela." By M. A. Ernst, Caracas.

GLASGOW

Geological Society, November 3.—Mr. John Young, Vice-president, in the chair. Mr. James Thomson, F.G.S., submitted to the Society some remains of fish and molluscan life, which he had recently discovered in the neighbouring coal-fields, and which were new at least to the west of Scotland. These were *Acanthoides Wardii*, from Airdrie; *Athyris pisum*, from Brockley; and *Anomia corrugata*, from Dalry. He pointed out the characteristics of these species, and described the relative position of the beds in which their remains had been found. 1. The *Acanthoides* was a well-preserved specimen, showing the dorsal and anal spines in their natural position This was of some importance, as these spines had frequently been found singly, and could not be referred to any known genus; but this discovery enabled palæontologists to name and classify these ichthyodorulites. This species had also been discovered in the Staffordshire coal-field by Mr. John Ward, and named by Sir Philip Egerton, F.R.S., after its discoverer. It also occurs in the Edinburgh coal-field. The specimen before them had been found near Airdrie, in the upper members of the Clyde coal-measures. 2. *Athyris*. This little fossil occurs at Brockley, Lesmahagow, and Roughwood, Ayrshire. From the resemblance to *Terebratula Saculus*, it had often been mistaken for that shell; but when placed under the microscope the structural characters indicated that it could not be referred to that genus. It had been submitted to Mr. Thomas Davidson, F.R.S., who named it *A. pisum*, from its pea-like form. 3. *Anomia corrugata*. This is the first well-authenticated specimen of Anomia that has been recorded from the Scottish mountain limestone. It is found in a band of shale which underlies the "Linn" limestone, near Dalry—Mr. D. C. Glen, C.E., gave some notes on the boulder-clay laid open in the excavation now going on for a new dock at Cartsdyke, near Greenock, and referred to the abundance of arctic marine shells and other organisms found embedded in it. The shell-bed seems to occur in a hollow of the boulder-clay, which has been exposed to view by a deep cutting running parallel to the river, or east and west. On the northern side of this cutting, nearest the river, the bed is several feet in thickness; but on the other side it thins out, and finally disappears as we recede from the shore. In the other direction, from east to west, it is seen to abut suddenly against the boulder-clay, and thus occupies a hollow of no great extent, in which, however, an immense number and variety of marine organisms are crowded together, forming one of the richest beds of such clay yet discovered on our western coast. At the same time there was reason to doubt whether the deposit is now found in its natural position, or has not been dug out from some neighbouring part of the shore, and laid down to improve and level the ground, many years ago, in forming the policies where the excavation is being made. On this point, however, he would not express a decided opinion, and other members who had visited the spot were not unanimous regarding it.

DUBLIN

Royal Irish Academy, November 14.—The Rev. Professor Jellett in the chair. The Rev. Maxwell Close read a paper "On M. Delaunay's Views relating to the condition of the Interior of the Earth." The paper was referred to the Council for publication.—Mr. Samuel Ferguson, LL.D., read portions of a paper "On the difficulties attendant on the Transcription of Ogham Legends, and the means of avoiding them." He presented the Academy with a series of casts of Ogham Legends, and pointed out the advantages of them to students of the subject. Dr Stokes and Professor Ingram congratulated the Academy on this important addition to its collection, and Dr. Ferguson was invited to consider the expediency of issuing engravings of the casts. The reading of the remainder of the paper was postponed to a future meeting.—At a meeting of the Council of the Academy on the 9th inst., it was resolved to recommend to the Academy that Her Majesty's Government be memorialised to use their good offices in order to prevent, as far as possible, any injury during the present siege to the collections in Paris, which are universally acknowledged to be of inestimable value to science, literature, and art. In pursuance of this resolution the following memorial to the Government was adopted on the motion of Dr. Ingram, seconded by Professor Hennessy:—"We, the president and members of the Royal Irish Academy, desire to call the earnest attention of Her Majesty's Government to the irreparable loss which would be sustained by the whole civilised world if the inestimable scientific, literary, and other collections of Paris should be destroyed or seriously injured during the siege. That city contains galleries stored with treasures of art, libraries rich in every species of literary monument, and scientific museums which are amongst the foremost in their several kinds. These collections represent the accumulated labours of many generations, and are, in truth, the property not of France only but of the whole civilised world. Many of the objects contained in them, if once allowed to perish, no subsequent exertion could ever replace. The fate of the library at Strasburg shows that these priceless collections are in real and imminent peril from the operations of the war. It is not for us to pronounce any opinion on the merits of the present lamentable struggle, or on the conduct of either of the contending parties; but as members of a body having for its object the cultivation of science, literature, and archæology, we protest, in the name of the intellectual interests of humanity, against the destruction of these collections; and we respectfully call upon Her Majesty's Government to use their utmost efforts for their preservation, by impressing on the belligerents the duty of taking every possible precaution for their protection from the dangers to which they are likely to be exposed"

Royal Geological Society of Ireland.—W. Stokes, F.R.S., in the chair The Rev Prof. Haughton, F.R S., read a paper "On the amount of horizontal thrust produced by the secular cooling of the earth, and its effect in producing continents and seas." In the discussion which followed the reading of this paper, Professor Hull, Rev. Maxwell Close, and Mr. William Ogilby, took part. Professor Macalister, hon. sec., exhibited a collection of volcanic rocks and of fossils from South Italy, presented by Prof. Guiscardi and Mr. R. Mallet, also a collection of fossil Devonian Plants from Nova Scotia, presented by Principal Dawson.

BERLIN

Royal Prussian Academy of Sciences, July 14.—Dr. A. W. Hofmann read a memoir on the Aromatic Cyanates, containing investigations on derivatives of the phenyle, tolyle, xylyle, and naphtyle series.

July 25—M. Kummer read a paper on the Algebraic Systems of the third order—Prof. W. Peters read descriptions of New Species of Shrews from the British Museum The species were *Crocidura retusa*, from Ceylon, *C. fœtida* and *C. doriæ*, from Borneo, *C. monticola*, from Java, *C. microtis*, from Hong Kong, and *C. gracilipes*, from Madagascar, and belonging to the subgenus *Pachyura*, *C. waldemaris*, from Bengal, *C. ceylanica*, and *C. media*, from Ceylon, *C. sumatrana*, from Sumatra, *C. fuscipes*, from Singapore and Java, and *C. luzoniensis*, from Manilla.—Dr. Hofmann read an account of various investigations relating to the action of cyanogen upon aniline and triphenylguanidine, to a new class of cyanic ethers, to a new mode of formation of the isonitriles, to tests for cyanuric acid and chloroform, to the diagnosis of primary, secondary, and tertiary amines, to the knowledge of phenylxanthogenamide, to the action of acetic acid upon phenylsenfol, to the history of the ethylene bases, to the knowledge of aldehyde-green, and to the molecular volumes of chinone.

PHILADELPHIA

American Philosophical Society, Oct. 21.—Prof. Cope read a paper "On the Osteology of *Megaptera bellicosa*." He stated that this species of whale was one of the few whalebone whales of economic value found within the tropics, being the object of pursuit in the Caribbean Sea. Having received a skeleton from the island of St Bartholomew, West Indies, he presented a detailed account of its structure. He pointed out important points by which it differed from the known species of *Megaptera*, among others in the form of the mandible and of the nasal bones.—Dr. George Emerson read a paper on the part taken many years ago by the American Philosophical Society and Franklin Institute of Philadelphia in establishing stations for meteorological observations in Pennsylvania, detailing the arrangements adopted by them for procuring a full series of observations at fifty-two points in the State.

BOOKS RECEIVED

ENGLISH—Odd Showers, or an Explanation of the Rain · Carriber (Kerby)—Our Feathered Companions. Rev. T Jackson (Partridge).

FOREIGN—(Through Williams and Norgate)—Steinkohlentheer: A. Pubertz.—Beitrage zur Histologie des Gehör-organes Dr. Rudinger.—Die Kleinschmetterlinge der Umgegend Munchens A Hartmann—Biologische Briefe von Dr. G Jager—Die Praxis der Naturgeschichte: P. L Martin.—Geometrie der raümlichen Erzeugnisse ein-zwei-deutiger Gebilde Dr E. Weyr—Die Geometrie und die Geometer vor Euklides C A Bretschneider—Die Pflanzenstoffe, 3^{te} Lieferung Husemann—Elemente der Mineralogie: C F Naumann—Beitrage zur Biologie der Pflanzen, 1^{te} Lieferung: Dr F. Cohn—Die Spectralanalyse: Dr Schellen, 2^{te} Auflage.

DIARY

THURSDAY, DECEMBER 1.

ROYAL SOCIETY, at 4.—Anniversary Meeting.
LONDON INSTITUTION, at 7 30—On Gems and Precious Stones: Prof. Morris
LINNEAN SOCIETY, at 8
CHEMICAL SOCIETY, at 8—On some Derivatives of Anthracene: Mr. W H Perkin
SOCIETY OF ANTIQUARIES, at 8.30—Faliscan Inscription Padre Garucci

SUNDAY, DECEMBER 4.

SUNDAY LECTURE SOCIETY, at 3.30—On the Telescope and its Discoveries: Mr. R. A Proctor.

MONDAY, DECEMBER 5.

ROYAL INSTITUTION, at 2—General Monthly Meeting.
LONDON INSTITUTION, at 4—On Chemical Action: Prof. Odling.

TUESDAY, DECEMBER 6

ANTHROPOLOGICAL SOCIETY, at 8—On the Races inhabiting the British Isles Mr A L Lewis—On Archaic Structures of Cornwall and Devon. Mr A L Lewis—On Forms of Ancient Interment in Antrim · Dr Sinclair Holden

WEDNESDAY, DECEMBER 7

SOCIETY OF ARTS, at 8.—On the American System of Associated Dairies, and its bearing on Co-operative Farming. H M Jenkins.
GEOLOGICAL SOCIETY, at 8—On Fossils from Cradock, Cape of Good Hope Dr George Gray—On some points in South-African Geology, Part 2: Mr G W Stow—On the Geology of Natal Mr C L Griesbach.—On the Diamond-districts of the Cape of Good Hope Mr G Gilfillan.

THURSDAY, DECEMBER 8

LONDON MATHEMATICAL SOCIETY, at 8—Further Remarks on Quartic Surfaces. Prof Cayley—On the Polar Correlation of two Planes, and its Connection with their Quadric Correspondence: Dr. Hirst—On Systems of Tangents to Plane Cubic and Quartic Curves Mr L J. Walker—On the Order and Singularities of the Parallel of an Algebraical Curve Mr. S Roberts
SOCIETY OF ANTIQUARIES, at 8 30
LONDON INSTITUTION, at 7 30.—On Count Rumford and his Philosophical Work. Mr. W. Mattieu Williams

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THURSDAY, DECEMBER 8, 1870

THE ECLIPSE EXPEDITION

BEFORE this reaches the hands of our readers, both sections of the English Government Eclipse Expedition will be on their way, the one to Spain and Algiers, the other to Sicily. The article in our last number will have given a general idea of the work to be done, and we think it will be admitted that seldom has so much work been laid out to be accomplished in a brief two minutes. To choose the right men for so important an investigation in a scientific point of view has been no easy task; but the list may now be looked on with satisfaction as comprising men of known ability, and of tried powers of observation in the various departments of Science concerned. Astronomy, chemistry, spectroscopy, photography, pure physics, are all worthily represented; and from our Paris intelligence this week it will be seen that there is good hope of M. Janssen being able to leave Paris to join in the Expedition.

We can now only wish for both parties that the elements will be propitious for the work they have undertaken. The time during which the observations can be made is so short that the most careful arrangements will be necessary to utilise the observing powers of every member of the party. Each will have his work definitely laid out for him. On the performance of the assigned duty without regard to other phenomena which come within the sphere of another man's work, will much of the success of the Expedition depend.

But scarcely less important than the arrangements at the moment of the eclipse, have been those of the Organising Committee, which had the charge of the preparations for the Expedition. The unfortunate delay which took place in ascertaining the intentions of the Government, threw on this Committee, after that intention was known, an amount of work compressed into the space of a few weeks, which ought to have extended over as many months. It was only in the first week in November that a definite assurance was received that an application for money and ships for the purposes of the expedition would be likely to be successful. The work to be accomplished by the Committee between that time and the first week in December, was such as those experienced in such matters might well shrink from; but, thanks to one or two individuals who had the advance of science at heart before anything else, the work has been done, and, what is more, has been well done. To Prof. Stokes in particular the thanks of the scientific world are due, for the untiring assiduity with which he has laboured to bring the affair to a successful issue.

If there is one cause for regret in the programme of arrangements, it is the absence of any one name among the observers who are going out, who can be said to directly represent the Government. It is a Government expedition, undertaken with the assistance of public money and ships belonging to the nation; and it would have been right and fitting to have seen at the head of it one of the Government astronomers, rather than that all the labour of the organisation and all the credit of the observations, should they be successful, should fall to the lot of private persons This expedition will, indeed, form a conclusive argument against those who have held that if Government hold out a helping hand to Science, this will act as a bar to all private enterprise. Had the Government held back altogether from offering their assistance, no English expedition would have been organised; individual astronomers who felt sufficient enthusiasm to give up their time, and spend their money in furthering the ends of Science, would have been compelled to avail themselves of the generous and munificent offers of assistance from the American Government. Need we say in what light this would have been regarded by contemporary science and by future historians? Government having once stepped forward, and assumed its rightful position, a stimulus was thereby given to private enterprise; every individual concerned felt that not only the interests of Science, but the honour of his country was at stake, in doing his part towards ensuring a successful result; and probably never has an expedition been better organised, and started under happier auspices, notwithstanding what has been said to the contrary in one of the daily papers, which has evidently been misled by those who have a purpose to serve in abusing the Committee.

Now that the Government has put its hand to the work, we are bound to say it has done so in no grudging spirit. More has already been done than the promoters of the Expedition were at first given to expect. Not only has the *Urgent* been placed at their disposal, to carry the Spanish and Algerian party from Portsmouth to their destination, but a despatch-boat, the *Psyche*, is told off for the Sicilian expedition. All the foreign Governments concerned appear determined to emulate this good will; the arrangements of that of Spain we have already published. There is reason to hope that the necessary apparatus will pass through every custom-house, duty free, without the slightest impediment. It is hoped that the Sicily party may combine with that sent by the American Government, and may do their work and publish their results in concert.

Our readers need hardly be reminded of the special object which it is hoped will be accomplished by the present expedition: the settling for ever of the vexed questions concerning the luminous appearance visible in total eclipses, known as the Corona, both as to its actual locality and its constitution, which still have to be settled, notwithstanding some hard writing to the contrary. In all these observations the utmost nicety of observation will be required, and some ingenious and novel contrivances will be employed for the determination.

We shall take the earliest opportunity of placing the results before our readers. We have taken means to have a report sent to us by telegraph from every station, and shall hope to be able to summarise them in our issue of the 29th inst. These early reports will be the more valuable, as up to the present time we have no official account of the observations taken in Spain during the total eclipse of 1860. With the exception of Mr. Warren De La Rue's observations, published by himself, no results of that expedition have yet been made known to the public.

We have now only to wish the Eclipse Expedition, and every member of it, a pleasant and prosperous voyage, and a happy return to England with the consciousness of having contributed something to the progress of scientific investigation.

B.

HAECKEL'S NATURAL HISTORY OF CREATION

Natürliche Schöpfungs-Geschichte. Von Dr. Ernst Haeckel. 2te Auflage. (Berlin: Reimer, 1870. London: Williams and Norgate.)

WERE there any need of evidence to show how busy in the happier times of peace the German public was with Darwinism and general Natural History topics, it would be amply supplied by the history of this work, the preface to the first edition of which was written in August 1868, and the second edition of which is now before us. The work is, broadly speaking, a popular sketch, not so much of the Darwinian theory, as of Haeckel's extension of that theory; in many respects it is a new "Vestiges of Creation," the old question being viewed from a new stand-point, and the treatment of it adapted to new feelings and new times. The old work was modestly entitled "Vestiges;" Prof. Haeckel calls his a History; and indeed a detailed comparison of the two would bring out in a wonderfully vivid manner both the progress of zoological inquiry and the change in zoological temper which has taken place in the interval between the dates of their publication.

The relation of Prof. Haeckel's extended views to the original theory of Mr. Darwin is very well indicated in a few lines of the preface to the second edition. "Darwinism is neither the beginning nor the end of the theory of evolution; it is far away removed from tending to narrow or to fix an absolute limit to further inquiry. Just as every important onward step in science becomes at once the starting-point of many new lines of advance, so Darwin's theory of selection gives immediate rise to many large extensions of the general theory of evolution; and of these my Phylogenic theories are some of the first to hand. When, then, the orthodox Darwinians cast at me the reproach that 'I go too far,' that 'I out-Darwin Darwin,' that 'by my Radicalism I do harm to true Darwinism,' I see in all such reproaches nothing but an unwilling confession that I have extended the evolution theory away and beyond the limits within which Darwin investigated the question, and have not been afraid to carry it out to its grand consequences."

In the early edition, the first six lectures are devoted to a historical sketch of the evolution theory; the creation theories of Linnæus, Cuvier, and Agassiz, and the evolution theories of Goethe and Oken, of Kant and Lamark, of Lyell and Darwin being taken as landmarks. In the second edition these chapters have been somewhat enlarged and improved, but on the whole stand very much as they were. The next five lectures (7—11) form a general exposition of the theory of Natural Selection, with discussions on heredity, adaptation, and the struggle for existence. In the second edition these chapters remain almost exactly as in the first. The same may be said of the twelfth lecture, in which a sketch is given of Ontogeny, or the development of the individual, and a comparison made between it and Phylogeny, or the development of the kind or species—in other words, Genealogy. The two succeeding chapters discuss rapidly the cosmic history of the globe, the primordial differentiation of living from lifeless things, and contains, under the title of Periods of Creation, a short sketch of Palæontology. Between these two chapters the author has, in the secon edition, introduced a totally new chapter on what he call Chorology, *i.e.* the theory of migrations, in which he dis cusses the influence of migration on species, the cau of migration, the effect of changes of climate, and th question of centres of creation, and points out the probabl results of the Glacial epoch. The palæontological sketc is also much changed in the second edition, the "theory ante-periods," which has found but little favour with geol gists, being, though unwillingly, withdrawn.

The remainder of the volume, nearly half, is taken u with a concrete history of creation, *i.e.* with an accoun of how, and by what steps, all kinds of plants and animal have grown out of the primordial moners, those first exis ing living things which were, according to Haecke neither plants nor animals, but belonged to a third king dom of Protista. This part of the work therefore is descriptive genealogy of all living beings, the pedigree each kind of creature being made out, or rather conje tured out, as far as present knowledge will allow.

In the second edition, as might have been anticipate the genealogies are very much extended, and given wit much greater detail than in the first; in particular, the is a new whole chapter on the migration and dispersion mankind, and on the species and races of men. Th results of phylogenic speculation or inquiry are graph cally shown in elaborate genealogical trees; and a ne large plate shows at one glance how all races of men hav probably spread from a hypothetical paradise once situat in the great continent of Lemuria, now sunk below th waves of the Indian Ocean.

The result of criticism is shown in some few change in the several pedigrees, but on the whole these differ i the second edition very little from what they were in th first. The Halisaurians, for instance, have been broug back to the amphibians, and the Dinosauria have bee brought nearer to the birds; in fact, the whole arrang ment of the Reptiles has been a good deal upset. Othe wise the still larger changes suggested by Prof. Huxle and other anatomists, are referred to, but not admitted.

In the first edition the title-page was disfigured by bein opposed to a picture of heads of men and monkeys, whic was at once absurdly horrible and theatrically grotesqu without any redeeming feature either artistic or scientifi In the second edition the heads have been increased fro twelve to twenty-four, but their quality remains the sam As a set-off against this, however, we are presented wit two really beautiful and very instructive plates of the de velopment of several kinds of crustaceans and echinoderm and one comparing the development of a tunicate and am phioxus. There is also a large comparative view, we worth studying, of the embryos of the four vertebra classes at two different epochs of their development.

We have, in the above, attempted to give a gener idea of what the book is, and how the second editio differs from the first, rather than to enter into any crit cism. The first edition has already received the able criticism this country could give. We will ventur however, to make one reflection. Had the boo been written for scientific men, it would have bee read by some with delight, by others with feelings fretfulness and worry, but by all with more or less profit. Addressed as it is, however, to an intelligent an cultivated, but still to a general and unlearned public, prone to receive fanciful analogies as real reductions to simpler laws, and to confound together fond imaginings with sound and fruitful hypotheses, we very much doubt whether it may not turn out to be an engine rather of mischief than of good. Genealogies there must be, doubtless, and many, doubtless, also may in time be made out. In cases like the pedigree of the horse, the evidence already seems wonderfully strong; and it would be simple presumption to fix a limit beyond which we cannot hope for success. Still, by their nature, genealogies are like castles of cards in the shape of inverted pyramids, with each tier less safe and less sound than the one above it. A very little memoir may disturb one of the lower stages, and then a whole pile comes down with a run. They are not the kind of things to put before learners as the strongholds of science. Some readers would learn in the first half of this volume to love Darwinism better than biology, and before they had finished the second half, would love Haeckel better than either. Others would pass rapidly through a disbelief in Haeckel and distrust of Darwin to a state of complete doubt about biology in general. Worst of all would be the effect on such minds as that of a speaker at one of the meetings of the Biological Section of the British Association at Liverpool, who said he had believed for many years in spontaneous generation, in natural selection, in the evolution theory, and in most views of a similar kind, and who seemed ready to believe anything and everything except the old truth, that truth is very hard to get, but very precious when it is gotten.

M. F.

DEFECTS IN GENERAL EDUCATION

On Some Defects in General Education. Being the Hunterian Oration of the Royal College of Surgeons for 1869. By Richard Quain, F.R.S. (London: Macmillan and Co. 1870.)

DR. QUAIN begins the present lecture with a pleasant and suggestive sketch of the career and genius of Hunter, but the greater part of it is taken up with the subject indicated in the title. The point to which most attention is naturally directed is the predominance of classics in the present system of education. Against this Dr. Quain protests with all the ardour that we expect in a man imbued with the best scientific ideas of his time. In the first place, he insists that the study of our own language and literature should hold a much more important place in the education of our youth than is actually assigned to it. He thinks it monstrous that men should be carefully taught to read Latin and Greek, and be left in almost total ignorance of the history of their own speech, with scarcely any real power of using it, and without the smallest insight into the true spirit of one of the richest and most extensive literatures in the world. Above all, however, Dr. Quain urges that Science should become the staple element of modern education. On the ground of mere expediency, he points out, rich and poor ought alike to be taught Science, for it gives the former a truer conception of the duties which attach to property, and the latter it enables to improve their position. But what is even more important, Science imparts to those who devote themselves to it the freest and largest culture; and it is grossly irreligious to talk reverently of a Creator, and yet to refuse to seize every opportunity to become better acquainted with the Creation. "If the instructors of the young in schools believe, if parents believe, that the things of this world are in truth the work of the Creator ought not that belief, without anything further, to settle the question for them? Ought not these 'glorious works' to be acknowledged as subjects for diligent study, not disregarded as they are now?" Another fault in our educational arrangements to which Dr. Quain refers is the excessive devotion to athletic sports which at present prevails. This, he thinks, arises from the repulsive nature of the chief subjects of study at our schools and universities, and would probably come right if the intellectual tastes and propensities of every order of mind were more carefully studied and gratified. The lecturer also protests against the dangerous extent to which we have carried the competitive system at the present day. With all the best writers on the subject—Mr. Matthew Arnold in particular—he believes that excessive competition is the reverse of favourable to true culture; that it renders anything like real study distasteful, and produces in the end narrow and superficial minds. He suggests that it might be well, as in Germany, to have for boys leaving school one general examination, which it would be necessary to pass for entrance to the universities to the professions, and to the public services. For this examination there would be no special preparation; it would only serve as a test of the general culture derived during a series of years from the training of skilled teachers. Afterwards the student ought to be allowed to consult his own tastes in the choice of subjects of study. The other matters of which Dr. Quain speaks, are the necessity for a higher order of masters in our national schools, and the absurdity of mixing up with strictly professional training in medical schools instruction in physics, chemistry, and botany. We hope that so thorough and exhaustive an exposure of the weak points in our educational system, coming as it does from so high an authority, will not be without its effect in quarters where there is the power, if only there was the will, to bring about a more satisfactory state of things.

OUR BOOK SHELF

The Natural History of Commerce. By John Yeats, LL.D. (London: Cassell, Petter, and Galpin, 1870.)

THE design of this book is excellent; and it has, on the whole, been well carried out. The author is well known as the principal of a large "middle-class" school, who has long recognised the claims of Science as an essential item in the education of an English gentleman or merchant. And the information contained in this volume is exactly such as ought to be familiar to everyone who lays claim to the advantages of a liberal education. We are afraid, however, that, as a matter of fact, it will be found that the "Natural History of Commerce" is a *terra incognita*, especially to those engaged in commercial pursuits, who might often derive, not only pleasure, but, what is perhaps more to the point, profit, from some acquaintance with it. The work is divided into four parts. In the first we have commercial products treated from a geographical point of view; the different botanical zones of Meyen are defined; and the principal natural products described of Britain, Continental Europe, and the other quarters of the globe, with a supplementary chapter on Nature and Man as agents of change. The second part is descriptive of the commercial products of the Vegetable Kingdom, in which Meyen's plan appears again to have been followed in the main; it is subdivided into Food Plants, and Industrial and Medicinal Plants. In the third part we have, in like manner, the commercial products of the Animal Kingdom; and, in the fourth, raw mineral products. The comments which we have to make are almost confined to errors of omission which can be readily rectified in future editions. We regret to see still retained the antiquated classification of the Animal Kingdom into Vertebrata, Mollusca, Annulosa, Radiata, and Protozoa. The sentence by which (p. 260) the porcupine and the ant-eater are made members of the order Monotremata, is no doubt merely an oversight. Among food plants, it is strange to find no mention made of the potato, nor, indeed, of any of our culinary vegetables, the cabbage, turnip, or carrot, with the single exception of the onion! We demur to the assertion that the morel is "one of the few fungi found in this country which may be eaten with safety;" among these few we do not understand why the truffle and the morel only are given, the mushroom not being even alluded to. Indeed, the whole subject of Vegetable Products requires revision, many being entirely omitted of much greater importance than others to which considerable space is allotted. Among Industrial Plants, for instance, we should expect to find some description of the numerous fibres now used in the manufacture of paper, the esparto-grass, different kinds of wood, &c., which are daily becoming more important articles of commerce. An exceedingly useful vocabulary is appended, containing the names of natural productions in the principal European and Oriental languages; and the volume may be safely recommended as containing an immense mass of useful information on a very important subject.

Record of American Entomology for the year 1869. Edited by A. S. Packard, Jun., M.D., 8vo. (Salem, 1870. London: Williams & Norgate.)

THIS is the second annual analysis of the literature of American Entomology which has been published under the care of Dr. Packard. It must be gratifying to entomologists to find that their science is so popular in the United States as to render the production of such a work at all feasible, and we can only hope that the Editor may receive sufficient support to enable him not only to continue it in its present form, but even to enlarge it and make it still more useful. Of course, with the general entomologist, this Record can never take the place of the entomological portion of the *Zoological Record* which has been brought out in this country since 1865, but it is of the greatest value in giving the European naturalist intimations of papers and descriptions published in those out-of-the-way American periodicals which rarely fall into his hands.

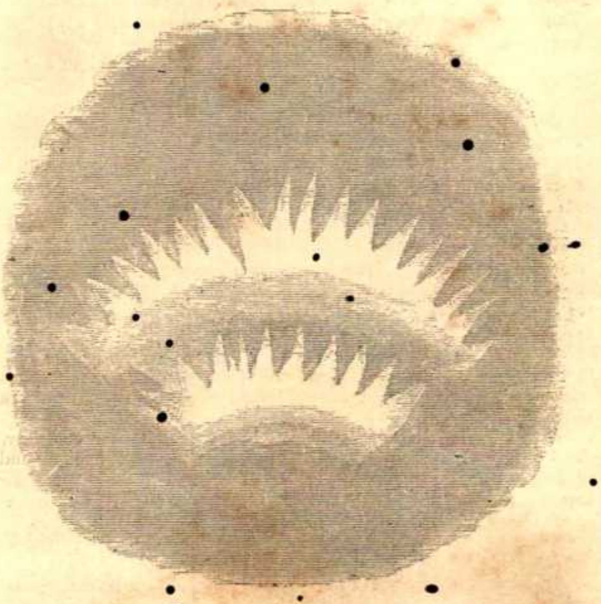
W. S. D.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Aurora Borealis by Daylight

I HAVE been expecting ever since the great display of Tuesday, Oct. 25, to see some statement that the Aurora was visible on the afternoon of that day. I was in Huntingdonshire, about four miles from Kimbolton, at the time, and being out of doors at half-past four P.M., saw a remarkable pale luminous appearance some 25° above the horizon and almost due east. At this point there were two arcs of faint white lines, one above the other, both radiating outwards with a number of short points. The breadth of the upper arc (which was rather the widest) might be from twelve to twenty times the apparent diameter of the moon. It was of course broad daylight at the time, and the sky was perfectly clear and cloudless for a long distance round these patches of light. They were visible for at least ten minutes, brighter and fainter alternately, and were seen by a friend as well as myself. I was unfortunately called away, and prevented from watching them while the sky became darker. After dark (at 5.30)



they had disappeared, but the dark irregular circle mentioned by Mr. Grove, from which the streamers radiated, was extremely conspicuous. It did not strike me at the time that the appearance at 4.30 was likely to have any special interest. But as it seems that the greatest disturbance of the telegraphs happened before the *evening* display of the aurora, it may perhaps be of use to record that it was also visible in the latter part of the afternoon.

26, Finsbury Place, E.C.

JAMES CUBITT

The Spectrum of the Aurora

THE following observations on the Spectrum of the Aurora were made by Mr. Alvan Clark, jun., in the neighbourhood of Boston, on the evening of October 24. He used a chemical spectroscope of the ordinary form, with one prism, and photographed scale illuminated with a lamp. Four lines were seen at the points marked 61, 68, 80, and 98. To reduce these to wave-lengths, the next day I measured the lines C, D, E, b, F, and G with the same instrument, and deduced the annexed table:—

Line.	Reading	Wave-length	Assumed Line.	Comments.	Error.
C	49·1	656			
D	57·8	589			
(1)	61	563	557	Common Aurora Line	− 2
(2)	68	532	531·6	Corona Line ?	+ 1
E	69	527			
b	71·2	517			
F	79·7	486			
(3)	80	485	486	F Hydrogen.	− ·3
(4)	98	435	434	G "	+ ·6
G	100·5	431·2			

The first column gives the name of the line; the second the readings on the scale; the third its wave-length, obtained from Angström's chart, and by interpolation; the fourth the wave-length of the line with which these lines are supposed to be identical. The fifth column gives the name of this line, and the sixth the error in parts of the scale. The first measure is evidently wrong, and should probably have been 63. For the other three, however, the agreement is remarkable, two coming close to F and G of hydrogen, the other to the line observed by Professor Young in the solar corona. The lines C and F having already been observed, the discovery of the line G seems to render certain the existence of this gas in the Aurora. The common methods of interpolation were not available for computing the wave-lengths of column three, on account of the large interval between the lines. A curve was therefore constructed, in which vertical distances represented scale-readings, and horizontal distances the square of the reciprocal of the wave-lengths. This, by the formula of Cauchy, should be very nearly a straight line, giving, consequently, results of great accuracy.

EDWARD C. PICKERING

ON the evening of Monday, October 24, a very remarkable auroral display was observed in New York City. In the north at first was an obscure segment, low towards the horizon, surmounted by a pale glow of light. Later, this was replaced by an extensive manifestation of beautiful streamers.

But the most notable part of the phenomenon was a band of brilliant red light, varying in its different parts constantly throughout the evening, in brilliancy, definition, and breadth, but in general some ten degrees wide. It was seen early in the evening, and could be traced from within fifteen degrees of the horizon in the east and the west, extending entirely across the sky, south of the Zenith.

The position of this rosy band viewed from the south-east corner of the Central Park, was from 8¼ to 8½ p.m., New York City mean time, about as follows.

In the east, just above the horizon, passing almost centrally, and diagonally through the figure of *Cetus*, say between δ and ζ Ceti; thence going westward it lay between *Piscis occidentalis* and γ *Aquarii*, and between *Altair* and *Delphinus*, often expanding in width beyond these boundaries. Thence it extended westward to the stars in the heads of Hercules and Serpentarius nearly to the horizon.

In all its splendid variations throughout the evening, it maintained its position in regard to the horizon of the observer standing in the locality before mentioned, the diurnal motion of the earth carrying the stars past it.

On the following evening the exhibition of this red band, in the same position, was at least equally pronounced; but without any decided boreal light, so far as I have noticed or learned.

I would also here mention the very brilliant display of variously tinted auroral streamers which engaged the admiration of members of the American Association for the Advancement of Science, as they returned by the steamer on the Hudson to the city of Troy, from their visit to Albany, from ten to eleven o'clock on the evening of Friday, August 19, 1870.

JAMES HYATT

Honey Brook, Stanfordville P.O., New York

Early Mentions of the Aurora Borealis

YOUR correspondent, Mr. Earwaker, should have mentioned that the list of appearances of the Aurora Borealis given in M. de Mairan's work is chiefly derived from another enumeration in Prof. Frobes's "Nova et Antiqua Luminis atque Auroræ Borealis Spectacula," Helmstadii, 1739. On reference to this list it immediately appears that it is very little to be relied upon for displays previous to the year 1707 or thereabouts. Some of the appearances recorded rest on no good authority, others were obviously meteoric; some occurred within the Arctic circles, others are entered twice over from inadvertence, or a neglect to allow for the difference of style; more than twenty are recorded on the authority of the meteorological diary kept at Breslau by Grebner, who in fact says nothing more than that on those occasions the night was somewhat bright (*sublustris*). When the necessary deductions on these accounts have been made, it will be found that the infrequency of the phenomena prior to 1707, and its extraordinary development since that date, are well-established facts. They are strongly insisted upon by M. de Mairan himself, who does not produce more than five instances of the aurora in France during the whole of the seventeenth century, and shows from the evidence of a missionary that it was unknown or forgotten in China until 1718. All contemporary notices prior to about this period, attest the astonishment with which it was regarded.

From the following curious passage in Sirr's "Ceylon and the Cingalese" (vol. ii. p. 117), it would appear that the aurora, or something resembling it, is occasionally visible in Ceylon:—

"There is a heavenly phenomenon which appears occasionally in Ceylon, called by the natives Buddha lights; this faintly resembles the Northern Lights, and is extremely resplendent. The priests declare this to be a sign of Buddha's displeasure when his followers have become sinful in the extreme, and that the light appears over the wihare, from whence the priests suppose the phenomenon to emanate, where those who have committed the sin which has aroused the god's anger last worshipped."

London, November 19 R. G.

IN many parts of Ireland a scarlet aurora is supposed to be a "shower of blood." In 1854 while stationed at Bearhaven, County Cork, a scarlet aurora that then appeared was said to be the blood of the people that were slain at Balaclava. About two years ago, while driving between Oughterard and Clifden, at two o'clock in the morning, there was a magnificent scarlet aurora (by far the most brilliant I ever saw), on which the car-driver remarked, "I wonder, can that be the blood of the Americans?" The late aurora is said to be the blood of the Frenchmen.

In the old Annals showers of blood are recorded at different times, always in connection with a great battle or the murder of some great chief. The earliest I can remember is that of A.D. 688, in the "Annals of Cloonmacnoise," after a battle of the Leinster-men and Ossory-men (now in the King's County, but formerly in Munster), wherein Foylcher O'Moyloyer was slain. This battle and shower of blood in the "Annals of the Four Masters," is said to have happened in the year A.D. 690. These Annals also mention that butter was turned into the colour of blood and a wolf was heard to speak; while the Annals of Tighernach place the battle in A.D. 693, and say that the blood flowed in streams for three days and three nights. During the aurora I saw about two years ago, and the two nights that the last aurora was seen here, the lakes and rivers looked to be full of blood. Therefore I should imagine the aurora of 688 must have lasted three nights, and that the people who saw it and recorded it must have had butter for supper the night it was seen, which reflected the colour.

G. HENRY KINAHAN

Geological Survey of Ireland,
Connemara, Nov. 26

Prismatic Structure in Ice

THE following extracts from a letter from Mr. Langton (whose name was, unfortunately, misread as Langters in sending his former letter to the press) may interest others as much as it has done myself, if space can be found for it in your columns. In that case, perhaps, you will kindly allow me to say a few words in reply on a future occasion.

St. John's College, Cambridge T. G. BONNEY

"I admit with you that the prismatic structure of ice on the point of melting does not appear to have any connection with the hexagonal crystals in which it is formed; and that the great analogy between the conditions of ice in that state and of igneous rocks, and I may add of clay in the process of desiccation, seems to point to contraction as the common cause. But then arises the question whether ice really does contract as it approaches the melting-point, as we know that most melted mineral matter does on cooling, and clay on drying. I am quite willing to admit that it may do so, and that, as you observe, its demeanour at a point about 32° F. has not been accurately ascertained; but if so, the fact should first be determined from independent observations before drawing conclusions from it. I am willing also to admit that I spoke loosely in my former letter when I said that the air bubbles in the process of freezing seemed to be formed in vertical lines. My object, in the concluding sentence of that letter, was to express a doubt as to there being any such contraction as you suppose on the ice approaching the melting-point, and to point out the lines of air bubbles as being the immediate cause of the structure of rotten ice. I did not then go into the origin of the bubbles, or into the cause of their being thus found in vertical lines. It is difficult to suppose that they were originally formed in those lines, for though it seems natural that the air which is always contained in water, should be excluded on its crystallising, there does not appear to be any sufficient reason why the bubble excluded to-day should be placed exactly under that formed yesterday. Upon reviewing the whole question, I am induced to think that, as will often happen, we were both right and both wrong: that you were right in attributing the prismatic structure originally to contraction, but wrong in supposing that contraction to be caused by an increase of temperature on the approach of the thaw, and that I was right in attributing the final break-up into prisms to the liquefaction spreading in every direction from the lines of air bubbles, but wrong in speaking of them as if they had originally been formed in those lines. We need not resort to a hypothetical contraction about the melting-point; we have a *vera causa* in accesses of cold, which will give you the desired contraction, and me my vertical lines of air bubbles. The true explanation I take to be as follows.

"After the ice has formed, and severe cold follows, it will contract, and probably equally in all directions. There is no impediment to its shrinking perpendicularly, as the whole sheet would then only be somewhat reduced in thickness; but the ice is entangled with the shores, and the whole sheet cannot contract horizontally, but relieves itself by a number of minute cracks. These are easily seen on the surface of glare ice, but one can hardly imagine that they do not more or less spread through the whole substance, and in your first letter you seem to say that you have noticed this after severe frosts. If then an air bubble comes near any such crack, it would seem natural that the direction of the crack should be diverted towards it. In fact the air bubbles, being weak points, would in a great measure determine the direction of these small fissures. Water would insinuate itself into them from below, carrying the air bubbles with it, which, upon the whole freezing, would not necessarily be exactly where they were before. A repetition of this process at frequent intervals during the winter would cause a rearrangement of the air bubbles, which one would naturally expect to find from this cause in more or less vertical lines. According to this explanation the prismatic structure would set almost from the first formation of the ice. The air bubbles would from the first be the indication of the direction of former fissures, and the lines which would determine that of new ones. As long as severe frosts continued, all other indications of the structure would be obliterated, but, as the temperature approached the melting-point, these lines of bubbles, as they formerly determined the direction of the fissures, would now be the weak points at which the thaw would commence pervading the whole mass.

"This explanation seems to supply everything that is wanted, and, upon looking back at your original letter, I am rather surprised, from the facts you mention, that you should have missed it. You say, 'I have seen it several times—in fact, *after every severe frost.*' This I have no doubt is correct; but why, then, do you say, lower down, that 'this finer structure may be found, if looked for, in *every tolerably gradual thaw*'? Again, you say that on one occasion you found the structure obliterated internally, 'except where some vertical lines of air bubbles marked the position of a tube or joint.' This exactly corresponds with what I have above supposed would be the process.

"Still, there are several points upon which more exact observations should be made before one can speak with any certainty upon the subject.

"1. Does ice contract on approaching 32° F.?

"2. Do air bubbles form from the first in vertical lines?

"3. Is there any indication, as the winter advances, of a rearrangement of the bubbles, as that they run into each other, and get more and more ranged in vertical lines?

"4. Is there any indication in the earlier stages of the ice, that, after a night's hard frost, the cracks seen on the surface spread through its substance? And if so, to what extent do they follow the lines of air bubbles?

"5. If a block of ice is cut early in the winter, before the prismatic structure from contraction with cold has commenced in any noticeable degree, and when it is no longer in a position in shrinking to crack in one direction more than another, to what extent does that structure afterwards develop itself?

"As there has been this one point in the behaviour of ice in which we have both taken an interest, I am induced to mention some other peculiarities which have come under my notice, with regard to some of which I have not even attempted to suggest an explanation. Several years ago I lived on the shores of a lake in the backwoods, and as in those early days the ice in winter and a canoe in summer formed one's only means of locomotion, one naturally thought a good deal about ice in its various stages of formation and decomposition. I lived about two miles below the head of the lake, where a river fell into it, upon which was a considerable fall, and which connected it with another much deeper lake, about a mile above. The effect of the stream was felt for a considerable distance into this lower lake, which was narrow at its head, and the ice was never safe there; indeed, excepting in the very severest frosts, there was as it were a bay of open water extending into the ice almost as far down as my house. But when the ice first formed in the fall it invariably took over the whole lake, even on that part which was afterwards open water during almost the whole winter. Indeed, upon one occasion, upon getting up in the morning I was astonished to see ice formed exactly on that part which was usually open water, whilst the rest of the lake, which was usually closed, had no ice upon it at all. Very soon after the sun rose that ice disappeared and the lake did not freeze over for a week after. The first ice, which always formed over the whole lake, would generally remain till there was a fall of snow on it, soon after which it would disappear where the effect of the stream was felt. The first winter I was there I nearly suffered from ignorance of this habit of the ice. I had occasion to cross the lake, which had been closed for some days, and had received a pretty thick coating of snow in the meantime, and, knowing that the ice must be pretty thin, I took the precaution to wear snow shoes. After a while I felt a peculiar sinking of the snow shoes, and observed that the track filled with water, and upon feeling with a stick I carried, I found no resistance of ice at all. I was, in fact, walking upon little more than a cake of snow. You may imagine that I at once made a little circuit and did not stop till my stick encountered good ice, and an hour or two afterwards that part of the lake was open water. Now I think I can explain this peculiarity of the early formation of ice where no ice remained during much colder weather in the winter. I take it that the whole surface, at least, of the water in my lake, which was rather a shallow one, had been reduced to 32° F., or nearly so, and that on a very cold night the water from above, being thoroughly exposed to the cold in coming over the fall, had been reduced even lower, so that when it reached the comparative quiet of the lake, where it naturally floated on the surface, it became ice for a short time that morning I spoke of, though it could not long maintain that condition. So also on ordinary occasions, when all the lower lake was ready to freeze, the water, thoroughly cooled at the fall, would freeze also, although the lake above had not yet been frozen over. At the foot of the upper lake, immediately above the fall, was a very shallow bar, so that the only part of the upper water which would come over would be the coldest layer on the top. But after the upper lake had frozen over also, and had received its coating of snow, very little more cold would penetrate to reduce the temperature of the surface, and the lake being very deep, and receiving fresh accessions of heat from below, the water would soon get considerably above the freezing-point, and with the aid of the friction of the stream would thaw away the snow-covered ice below with which it first came in contact. One is inclined to ask why the complete exposure of the water to the cold in coming over the fall, which I have supposed to reduce the temperature so much in the beginning of winter, had not the same effect in the severer cold afterwards; but the water which came from under the snow-covered ice would probably be much warmer than that which formerly came from the surface of the open water, and moreover the spray soon formed ice, which gradually crept over the shallower parts of the fall at its edges, and the exposure and its cooling effect may not have been as complete as at first. I cannot say that I am altogether satisfied with my explanation of the curious anomaly that a part of the lake would freeze over at a temperature of 20°, which would remain open when it was far below zero; but such are undoubtedly the facts.

"If there was this anomaly in the first formation of the ice, its sudden disappearance in the spring, which I mentioned in my former letter as giving rise to the popular prejudice that it sinks, was almost equally astonishing. Upon one occasion the ice was evidently in the last stage of decomposition, and I had got my canoe ready for a journey in the morning, when I fully expected the lake would be open; but before starting, I wanted to go to the two or three houses at the fall, which we dignified with the name of a village. Although the lake at my landing was an unbroken sheet of honeycombed ice, which had even borne me early in the morning, the open water extended to a point about half a mile above me, and I determined to carry my canoe so far through the woods. I cannot have been a quarter of an hour in doing so, but when I launched my canoe beyond the point, there was not a vestige of ice down as far as my landing, though I still saw it across the whole lake a little farther down. Being anxious to see the process of the actual disappearance, I turned my canoe down the lake, and paddled as fast as

I could; but long before I could get to where I had seen the ice, the whole lake as far as my view extended was open water. This almost instantaneous disappearance of a body of ice more than a foot in thickness can only take place in perfectly still weather. If there is any wind it breaks up, and the fragments are driven up against the ice which still holds together, and into the shores, where the rapidity with which it melts is not so striking. I never was fortunate enough to be actually in at the death.

"There are also some curious facts connected with the air-holes which form themselves during winter. There are often particular spots where partial openings in the ice will be formed every winter. These I conceive to arise from warm springs, and to have no connection with air-holes properly so called, which are not confined to any particular locality, but may appear anywhere. There is always a good deal of air under ice, and you may often see it scattered about in small bubbles when the ice is thin. It is probably air excluded in the process of crystallisation, and when there is added to it sundry gases formed from decaying matter in the water, it amounts during the winter to a considerable quantity. Such collections of air, like the bubble in a spirit-level, are in a very uneasy condition, and are rapidly transferred from one place to another on any casual disturbance of the level, giving rise to one of the numerous noises which are always more or less heard on a lake covered with ice—at least, we used always to attribute to this cause a peculiar groaning sound which was very common. Now, if there should be any casual inequality in the lower surface of the ice, the air will naturally collect there, and if it is above 32° F, which in so far as it consists of evolved gas it probably will be, the receptacle will be increased by thawing. A dome-shaped cavity will thus be gradually formed, which will finally reach the surface; air will escape from below, and the surface-water, of which there is almost always more or less after the snow has fallen, will run down from above, wearing the little jagged channels which are characteristic of air-holes. The whole thing will then after a while freeze up again, leaving an indication of where the air-hole has been in the different colour of the freshly-formed ice. I have tried several such air-holes with an axe when first formed, and have always found them to lead to such a dome-shaped cavity. I remember on one occasion an otter frequenting a large air-hole which remained open for some time, and which must have been from a mile and a half to two miles distant from the nearest open water. How did he reach it? for no otter can travel that distance under water without access to air. The Indians say that they will go to greater distances still under the ice, and that they always find air there. It is likely enough that there may be many such dome-shaped cavities, which have not yet reached, and may never reach, the surface as air-holes, but one would imagine the air they contain to be not of the most wholesome character. However, this otter did frequent that air-hole for about a week, which it certainly did not reach by travelling on the ice, and though it had few chances of breathing there, in the daytime at any rate, it contrived during that period to elude the snares of a white man and an Indian, who wasted a good deal of time in looking after it.

So far, the process of the formation of air-holes, if I am right in my explanation, is intelligible enough; but sometimes they are formed in a manner which is difficult to account for. Upon one occasion I had crossed the lake to a friend's house, about four miles off, and we had determined to start together next morning to our nearest town, but I had to go home first. I first went over by daylight, when there certainly was nothing unusual in the appearance of the ice, which might be four or five inches thick at the time, with a slight sprinkling of wettish snow on it. I returned home about eleven at night, and, as it was bright starlight, with only a few floating clouds, I should have noticed any change; but I came straight across, and saw nothing to attract attention. But when I crossed again at daylight in the morning, in one part of the lake the whole surface was covered with air-holes—there must have been hundreds of them. At first I gave them rather a wide berth, but, on approaching one to examine it, I found it frozen up again, the clear ice in the hole, with very slight indications of the characteristic jagged edges, being the only sign that there had been an open air-hole there during the night. I had no axe with me to try whether they were connected with any cavity, but the appearance was as if holes of from two to five or six inches in diameter had been punched through the ice. Of course, we attributed it to electricity, as people will do anything which they do not otherwise understand, and I have never been able to give any more intelligible explanation of the phenomenon. There certainly had been some faint sheet lightning that night, a very unusual thing in winter; but what connection, if any, there may have been between the two things, I cannot tell.

"Ottawa, Sept. 15" "John Langton"

The Difficulties of Natural Selection

I find, on looking again at Mr. Bennett's article, that I have misrepresented him on one point, for which I beg to apologise. On his supposition, that the first twenty possible steps on the road to mimicry are absolutely useless, his argument will have some weight. This supposition, however, is entirely unsupported by facts. Very large variations of colour are exceedingly common in butterflies; and when such variations are in the right direction, they must in some cases be useful. I believe myself that far less than fifty, or even twenty, steps of variation would in some cases produce very good mimicry.

Alfred R. Wallace

Cave-paintings by Bushmen

My friend, Mr. George W. Stow, of Queenstown, South Africa, refers in a letter to the interesting subject of the old cave-paintings by the Bushmen, as follows: "During the last three years I have been making pilgrimages to the various old Bushman caves among the mountains in this part of the colony and Kaffraria; and, as their paintings are becoming obliterated very fast, it struck me that it would be well to make copies of them before these interesting relics of an almost extinct race are entirely destroyed. This gave rise to an idea in my mind of collecting materials enough to compile a history of the manners and customs of the Bushmen, as depicted by themselves. I have, fortunately, been able to procure many fac-simile copies of hunting scenes, dances, fightings, &c., showing the modes of warfare, the chase, weapons, disguises, &c. This promises to be a collection of very great interest. In some places it is astonishing to what a degree of perfection some of the wild artists had arrived. I have found three different series of paintings, one over the other; and, as the most recent must be upwards of fifty years old, the undermost are most probably very ancient. The colours are very permanent, and would last for ages if not wantonly obliterated. Unfortunately, the Kaffir herds and others are constantly destroying them, and, by the time another generation has passed, few remains of them will be left."

The pigments used in the caves were derived from ochreous concretions abounding in some of the sandstones of the Karoo series of the interior of South Africa, as in the Rhenosterberg, Stormberg, and elsewhere. These concretions, when broken open, supplied the natives with paint-pots, and from among the several colours of yellows, browns, reds, &c., the chocolate was selected for painting the human form in the caves.

T. Rupert Jones

5, Terrace, Yorktown, Surrey

A Rare Fish

A Specimen of the Silvery Hair-tail (*Trichiurus lepturus*) was taken this morning at Seaton. It measures 2 feet 2 inches in length, and is in very good preservation, being only slightly injured on one side of the head. A specimen from the Collection of the late Mr. F. W. L. Ross, in this museum, is about the same size, and was taken on 6th August, 1852, off the Start Point, Devon. The recorded instances in which this remarkable fish has occurred on the British coasts are very few, and the specimens obtained have generally been much injured. The present specimen was brought to me to name by Mr. Frank Gosden, of the West of England Fish and Game Company, Queen Street, Exeter.

W. S. M. D'Urban, Curator

Devon and Exeter Albert Memorial Museum,
Queen Street, Exeter, December 3

The Ceratodus Forsteri

I am much obliged to Dr. Sclater for his remarks on the new fish discovered by me as *Ceratodus forsteri*, and I take this opportunity to inform your readers who may feel interested in this matter, that I spoke of the animal as an amphibian, principally because it is in the habit of leaving the water during the night. The works to which Dr. Sclater refers me are not at my command, and I adopted the generic term of *Ceratodus* because the bulk of my specimens greatly resemble those of the above fossil genus. Professor Agassiz has written to me to say that the discovery of the *Ceratodus forsteri* is of the greatest importance, and that he (Prof. Agassiz) is "amazed" at it. By this mail two of these interesting strangers (with intestines) will be shipped to England, as a present from Prof. A. M. Thomson to Prof. Owen, another by Mr. Ramsay to Dr. Sclater. I am glad to see that my friend Ramsay has complied with Dr. Sclater's request, and ceased classing the *Ceratodus* as "Salmon," which he confesses to have done frequently before. Mr. George Masters, the assistant curator of the Museum, is now at Gayndah, with appliances to catch the fish, and he will, if possible, send some alive to Sydney and to the Zoological Society of London.

Sydney, Sept. 7 GERARD KREFFT

The British Museum Collections

As it is proposed to remove the Natural History collections from the British Museum to Kensington, I hope care will be taken to make the collections as serviceable as possible to students. In particular the British Department might, with great gain to all, be extended and improved. There is now but one small room devoted to British zoology, and this interesting branch of science is poorly represented by a selection of species not always well chosen nor even strictly indigenous. Still, imperfect as it is, I believe this is the only attempt to present a comprehensive view of the British fauna in London. In the new buildings it is much to be desired that a large and well-lighted gallery should be devoted to the zoology of the British Islands, and as complete a collection as possible exhibited. The specimens should be labelled with local as well as scientific names, and, when desirable, short interesting particulars might be given, as on the labels of the art collection at South Kensington. I think no part of the museum would be so well frequented or so generally appreciated by the public. When the labour of removing the collections is over, I hope we may be furnished with catalogues of different departments, with notes of the time and mode of acquisition, &c., of the most important specimens. If begun for the British collections, it might afterwards be extended to the rest of the museum. The nation, which possesses such truly choice and extensive collections, ought to take care that the advantages to be reaped from them are fully developed and placed within the reach of all.

A. W.L.

Glass Floats off the Isle of Lewis

THE glass globes to which you refer as having been found on the shores of Lewis, are no doubt fishing floats. The Bergen fishermen have recently begun to use such balls as floats for their nets, and they are occasionally picked up in the North Sea. Those which have been brought to this office were empty, *i.e*, contained no liquid, and bore no distinguishing mark at all. They were picked up about 100 miles S.W. of the Loffoden Islands.

116, Victoria Street, London, S.W. ROBERT H. SCOTT

P.S.—If any of the globes are sent to me, I shall be happy to inquire in Norway about them, and return them, after inspection, to their owners.

The Milky Way

YOUR correspondent, Mr. Jeremiah, after quoting the words of the Llangadock "Oracle," adds, "meaning that the wind will blow from that quarter." Did Mr. Jeremiah interpret the man's meaning correctly? If so it is at variance with a popular belief in Hampshire, *viz.*, that in whichever way the Milky Way may be seen over night, the wind is sure to blow *across* it, or at right angles to it on the following day.

HENRY REEKS

The Cockroach

IN some ships infested with these insects, sailors frequently complain of having their toe and finger nails, and the hard parts of the soles of the feet and palms of the hands, nibbled by them. The men have exhibited to me their nails and skin, which had the appearance of having been attacked. I can vouch for the following, as I was the unhappy subject of it. On returning from a shooting excursion in salt swamps in tropical Australia, with my feet blistered and sodden, I was put to sleep in a room swarming with cockroaches (the small species). The night was intensely hot, and my feet were exposed. I had slept soundly for some hours, when an intolerable itching and irritation about my feet awoke me. I felt these objectionable insects running over and gnawing at my feet. On striking a light, I found they had attacked the skin, and entirely eaten it away from a large blister, leaving a raw place as large as a shilling. I slept again, and in the morning found they had completed the work, and established a painful sore. The whole of the hard skin on the heel was also eaten down to the pink flesh. The nails were not attacked. I have now, at a distance of four years' time, bluish scars on the skin.

Mill Hill, Nov. 11 ARTHUR NICOLS

I HAVE to thank the Rev. W. Houghton for his references on this subject, and to explain that I wrote Aristotle inadvertently for Aristophanes. My only objection to adopting σίλφη as the Greek equivalent for our cockroach is that the unpleasant smell which is mentioned as a characteristic of the former, is not particularly marked in the latter. If we adopt the view that the cockroach was known to the ancients, we must, of course, reject Gilbert White's story of its American origin, and, as he thought, its recent introduction into England.

C. J. R.

KAIETEUR WATERFALL, DEMERARA

THE great Kaieteur Fall, recently discovered by Mr. Brown, has a clear descent, according to barometrical observations, taken simultaneously by Mr. Brown at the bottom, and by Mr. Mitchell, at the top, of 750 feet. Above, the Potaro glides smoothly in a slight depression of the table of conglomerate sandstone, and disappears over the edge in a body, which is estimated at eighty yards in width, and of depth uncertain in the centre, but shallowing rapidly towards either bank. When the Fall was discovered in April, the rocky channel was completely covered, and the stream must have had a width of, at least, 100 yards. During the summer it is diminishing in volume, and, as the Indians state that it will continue to do so till October, only the central and deeper portion, about one-third of the whole, will then remain. The best time, therefore, for a visit is in spring, at the end of what appears to be the rainy season of this elevated tract.

As the Fall was seen by the exploring party who discovered it, nothing can be imagined more beautiful. The central portion, which is never dry, forms a small horse-shoe, or re-entering angle, and the water in this part preserves its consistency for a short distance from the edge. But everywhere else, and here also at a few feet from the top, all semblance of water disappears; it breaks up, or blossoms, into fine foam or spray, which descends in the well-known rocket-like forms of the Staubbach and similar waterfalls, but multiplied a thousand times, into a small dark pool, over a semicircular curtain. The cavern behind the Fall is the home of thousands of swallows, which issue from it in the morning, and may be seen returning in their multitude at night. The Fall itself is one vast descending column of a fine, dry-looking, snow-white substance, bearing a resemblance in colour and consistency to the snow of an avalanche, but surpassing all avalanches in size and in the beauty of the forms taken by the material as it falls. Rainbows of great splendour were observed, one from the front of the Fall in the morning, one from the summit in the afternoon; but this last reverted, forming a coloured loop or ring, into which the whole mass seemed to precipitate itself, and disappear and dart out underneath, black and foaming at the gorge and outlet of the pool.

Eleven days were spent in ascending the Essequibo, which was heavy in flood, and detained the party double the time anticipated; five days brought them from Tomatomari, the lowest fall on the Potaro, to the Patamona village. In this stage there are five cataracts, two of which, at least, are inaccessible. Two days were occupied in visiting the foot and summit of the Fall, and in descending to the Settlement, leaving Messrs. Brown and King to complete the survey and sketches of the country in four days and a half.

QUERIES RESPECTING ÆTHER

THE following speculations first appeared in the pages of the *Engineer*:—

When light and caloric were supposed to have a material existence, the hypothesis of the universal existence of a highly elastic medium was unnecessary, since matter might with the utmost freedom be projected through vacuous space; but as light and heat are now generally admitted to consist not of transmitted matter, but of transmitted vibratory motion (and why may not electricity, so freely interchangeable with the former, be admitted into the same category?), the necessity of the existence of a transmitting medium, pervading infinite space, becomes at once apparent; and this medium, hitherto not cognisable to our senses, has been termed æther.

But it has been further assumed that æther is alone capable of transmitting the extremely rapid vibrations of light and heat, and that it must therefore necessarily pervade or permeate all kinds of sensible matter. The questions proposed to be raised in this communication are the necessity of this interstitial hypothesis, and the probable capability of ordinary matter to transmit the vibrations of light and heat.

It is now generally admitted that when a body becomes heated, its own particles, and not those of the supposed interstitial æther, are thrown into a state of vibratory motion, the amount of heat corresponding probably to the amplitude of the vibrations; hence a certain amount of energy has been communicated to those particles, and, at all events, in the case of celestial mediations, the molecules of æther must previously have possessed the energy or *vis viva* which they have communicated. Hence æther, being susceptible of *vis viva*, has recently been admitted to be ponderable, but this admission is not a necessary consequence, for although the idea of the existing energy is associated with that of weight, in consequence of the constant energy acquired by gravitation having been taken as the measure or unit of energy, however acquired, there is no necessary connection between them. Suppose, for example, a flea were placed on an orbitating planet of the size of a pumpkin, while its muscular energy would remain undiminished, its weight would be infinitesimal, and the first leap would obviously plunge it into infinite space, to perform subsequently, perhaps, an independent orbit.

Further, it has been shown from the investigations of Mr. Norman Lockyer, to whom the progress of solar physics is so largely indebted, that incandescent gases are capable of initiating vibrations of definite periods, which are, moreover, occasionally accelerated or retarded by the proper motion of the emitting gas. What reason can there then be for doubting that gaseous matter is capable of transmitting heat waves, and if so, of likewise transmitting the waves of light, since the two are so intimately connected by the identical phenomena of reflection, refraction, and polarisation? May not in fact, in some instances, the perceptions of light and heat be but different sensuous impressions produced by the same vibrations?

The only basis on which the interstitial-æther hypothesis rests is the assumed incapacity of ordinary matter, whether in the solid, liquid, or gaseous state, to transmit the vibrations of light and heat, because the only vibrations, namely, sonorous, with which we are acquainted, are almost immeasurably slower than those of light and heat, the one being numbered by at most a few thousands, the other by hundreds of millions of millions in one second of time.

But it must be borne in mind that sonorous vibrations are always longitudinal, in the production of which repulsive forces are alone concerned; whilst, on the contrary, light and heat vibrations are necessarily transverse, and the production of these is solely due to attractive forces. Now these respective forces obey very different laws, for whilst attractive forces obey generally, and probably universally, the laws of the inverse square of the distance, molecular repulsion must obviously, at all events in gaseous matter, obey the laws of the inverse cube of the distance; therefore, from the rate of transmission of longitudinal vibrations, nothing can be predicted respecting the rate of transmission of transverse waves. It has been asserted that molecular repulsion is a dynamical resultant effect, and, therefore, incapable of expression by a statical law; but it is very doubtful whether molecular attraction is not equally a dynamical sequence, and, therefore, not a whit more entitled to claim a statical law than the former. Now, in the denser forms of matter, namely, the solid and liquid, it appears that the wave-lengths of created transverse vibrations are indefinitely modified, probably by the more energetic action of repulsive forces; for whilst any given kind of matter in the solid or fluid state is found, when incandescent, to emit light and heat waves of all lengths, and so to form a continuous spectrum, the same matter in the form of incandescent gas will emit only a few sets of waves of definite and invariable lengths; and, moreover, some of these wave-lengths are frequently found to bear very simple numerical ratios to each other. And even in gaseous matter it has been observed that the bright lines in the spectrum become narrower and more sharply defined by rarefaction; and, on the contrary, broader and less defined by condensation. Moreover, as regards density, the absorption bands in the spectrum appear to obey the same law as the bright lines. In other words, every kind of matter appears to be capable of emitting or absorbing its own peculiar waves, according to its tenacity; that is, as the results of molecular attraction are less and less interfered with by those of repulsion. The well-known peculiar incapacity of any given translucent substance to transmit the heat rays emitted by a heated portion of the same substance; or, in other words, the ability of the molecules to freely appropriate the wave motion that has been induced by some intervening medium by similar molecules, seems further to argue that ordinary matter is capable of assuming vibrations having the extreme rapidity of those of light and heat. And that there exists no valid ground for a distinction between light and heat in this respect is further confirmed by the experiments of Prof. B. Stewart, who has shown that the emission of light by incandescent bodies closely corresponds with their absorptive power (whether selective or otherwise) when not incandescent; and, further, that even in the decomposition of light into true polarised beams by the tourmaline it emits, when incandescent, the ray that is otherwise absorbed.

Can there, then, be any valid reason for doubting the ability of ordinary matter to transmit those transverse vibrations, which it is obviously capable of either absorbing or emitting: and if so, what ground is there for the hypothesis that the transmission of light and heat waves necessitates the presence of imperceptible æther in the interstices of perceptible matter?

If the existence of æther in infinite space, essential to the undulatory theory, be admitted, it may be asked, how is it possible to conceive its exclusion from any portion of space? A very simple hypothesis propounded by the writer in the introduction to the last edition of his "Elements of Physics" will meet this difficulty, namely, that æther (like its fluid namesake with water) is *immiscible* with known gaseous matter. This, it must be admitted, is sheer hypothesis; but if true, it must ever remain so, as being beyond the reach of human ken. But of this we may rest assured, that if it be not wanted in and around even our corporeal frames, it is not there; the contrary supposition would be inconsistent with the infinite wisdom of the Creator of the universe.

CHARLES BROOKE

SPECTROSCOPIC NOTES*

A NEW FORM OF SPECTROSCOPE

THE instrument, a description of which follows, was designed for attachment to the equatorial of 6·4in. aperture and 9ft. focal length, belonging to the observatory of Dartmouth College. It is especially intended for observations upon the solar spots and protuberances, and accordingly the principal object kept in view has been to combine a very high degree of power with compactness, lightness, facility of manipulation, and firmness of construction. Having the dispersive power of 13 prisms of heavy flint, each with an angle of 55°, it yet weighs less than 15lb., and measures over all 15in. in length, 8in. in breadth, and 4½in. in height. It was make by Alvan Clark and Sons.

The accompanying plate (Fig. 1), taken from a photograph, gives a correct idea of its appearance and general arrangement. The collimator and observing telescope have each an aperture of ⅞ths of an inch, and a focal length of 7in., which might advantageously have been increased to 12in. were it not for the necessity of compactness.

The light from the slit, after passing the collimator, is transmitted through the lower portion of a train of six prisms of heavy flint glass each 2¼in. high, and having, as stated above, a refracting angle of 55°. A seventh *half-prism* follows, and to the back of this is cemented a right-angled prism, by which, after two total reflections, the light is sent back through the upper part of the same train of prisms, until it reaches the observing-telescope. This is placed directly above the collimator, and firmly attached to it. Finally, a diagonal eye-piece brings the rays to the eye in a convenient position for observation.

The instrument has thus the dispersive power of thirteen prisms, and even with the low magnifying power of only five on the observing telescope, shows perfectly the lines of aqueous vapour, which make their appearance between the sodium lines when the sun is near the horizon. Of course, everything shown on the maps of Kirchhoff and Angström is readily seen with it, and many lines besides.

Its definition is very beautiful, and the only optical fault of the instrument seems to be a curvature of the lines, resulting from the shortness of the collimator.

After planning the instrument, I learned that the same idea of sending the light twice through the prisms by a right-angled prism at the end of the train had also occurred to Mr. Lockyer and others; but I do not know that it has yet been put in practice elsewhere.†

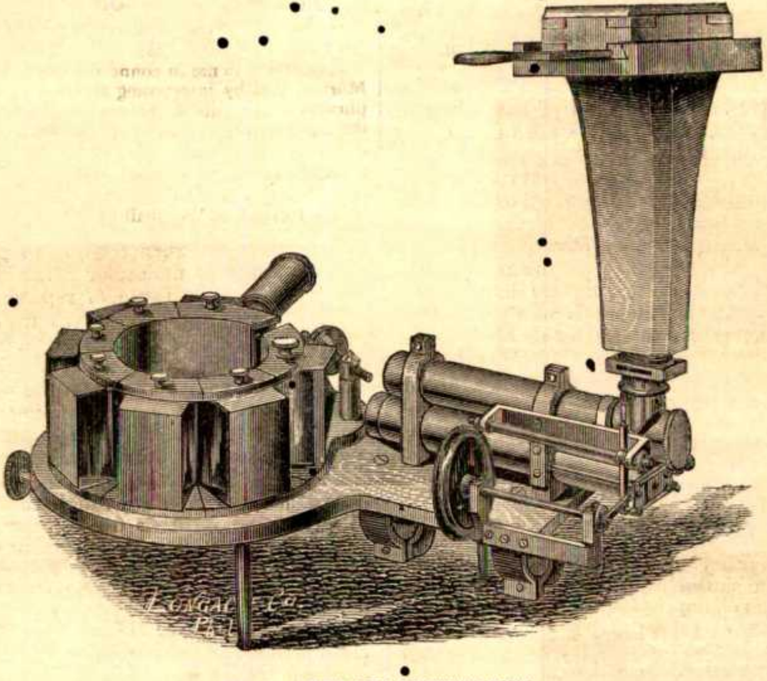


FIG. 1.—A NEW FORM OF SPECTROSCOPE

The prisms, for protection and convenience of handling, are set in frames of blackened brass. They are arranged around the circumference of a hollow cylinder of elastic gun metal, 3¼in. in diameter, with stout flanges above and below, between which they are clamped by little thumb-screws, so that they can be readily removed or transposed: it requires less than a minute to put the last prism with its reflector in place of any other of the train, thus reducing the dispersive power to any extent desired.

No particular care is required in placing the prisms, as a couple of narrow flanges were cast upon the cylinder near the top and bottom, and afterwards planed off to form true bearings for the backs of the prisms. They are thus always correctly set by being simply slid home before tightening the clamping screws.

The lower flange of the cylinder is attached to the base-plate by a screw directly under the middle of the front face of the first prism. Around this point as a centre the whole system of prisms is movable by means of a double-threaded tangent-screw, which brings the different portions of the spectrum into the field of view. The adjustment of the prisms to their angle of minimum deviation is effected by a method devised by Mr. George Clark, which is exceedingly simple, and, if not theoretically exact, answers every practical purpose. The flanges between which the prisms are clamped, are sawed through between the prisms, and a portion of the cylinder, flanges and all, equal to an arc of about 30°, is cut out between the first prism and the last. On closing up or spreading open this gap by means of a suitable tangent-screw, the circumference of the circle around which the prisms stand is correspondingly enlarged or diminished. Probably, when the ends of this opening are drawn very near together, or spread very far apart, the cylinder is somewhat distorted, and a corresponding mal-adjustment of the prisms results; but if so the effect is very slight.

The instrument gives a perfect view of every part of the spectrum from below A to H: above *h*, however, when all seven prisms are used, there is a loss of light occasioned by a partial obstruction of the apertures of the collimator and telescope by the corner of the reflecting prism.

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* From the Journal of the Franklin Institute.

† An instrument exactly similar in all essentials to the one here described has been used by Mr. Lockyer for more than a year past.

Were it important to secure the perfect cylindricity of the prism-frame through the whole range of adjustment, it could be easily done by merely fastening at the back of each prism a radial bar acting upon a central pin, as in the arrangement first devised by Mr. Rutherfurd, and since adopted by Mr. Browning, in his automatic spectroscope.

This plan of Mr. Clark's, doing away with all joints and hinges, has the great advantage of perfect firmness and solidity in every position of the instrument, an advantage hardly to be overrated in an astronomical spectroscope.

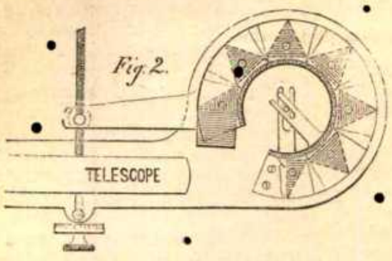


Fig. 2.

Had it occurred to me in season I might have made the instrument still simpler, firmer, and perfectly automatic in its adjustment, by merely substituting for the first prism a half-prism,[*] like the last of the train, to which the right-angled reflecting prism is cemented.

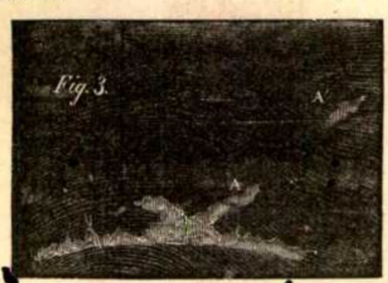


Fig. 3.

Placing the first half-prism with its front face perpendicular to the line of collimation, it would never need to be disturbed; the flange of the cylindrical frame which carries the prisms would be firmly fastened to the bed-plate immediately beneath it, and the pivot joint at this place with the corresponding tangent-screw would be dispensed with. The only adjustment required would be that produced by the screw which is now used to adjust for minimum deviation by opening or closing the gap of the cylinder.

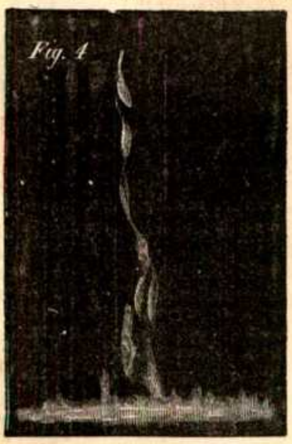


Fig. 4.

Of course, this arrangement would reduce the dispersive power of the train by the amount of one prism, a loss easily made up by adding a degree or two to their refracting angles.

Fig. 2 exhibits the plan of the proposed arrangement, and requires no explanation, unless to remark that for the sake of distinctness I have represented only two of the radial bars which may be used to render the adjustment accurate.

It might be better to place the face of the first prism not exactly normal to the line of collimation, in order to avoid repeated reflections between it and the object-glass of the collimator, which would be likely to produce a troublesome ghost, or the same thing might be accomplished by simply cementing the object-glasses of both collimator and observing telescope directly upon the front of the prism; this would make the instrument still more solid and compact.

The eye-piece of the instrument has an apparatus attached, which, however, thanks to the high dispersive power, I find unnecessary.

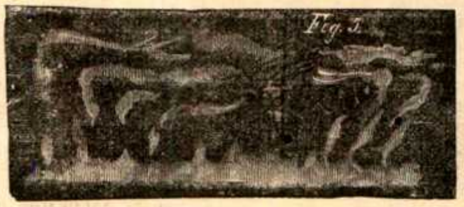


Fig. 5.

It was early proposed by Janssen to use a vibrating or rotating slit in order to make visible the form of a solar prominence, but as Zöllner has shown, the mere opening of the slit answers just as well, the light of the protuberance being diluted to precisely the same extent in either case.

It occurred to me in connection with a suggestion of Professor Morton, that by interposing at the focus of the eye-piece a diaphragm which should move with the vibrating slit, the light of the neighbouring portions of the spectrum might be cut off and this dilution avoided. Mr. Clark has devised and constructed a very beautiful mechanical arrangement by which this simultaneous and accordant motion of slit and diaphragm is effected by the rotation of the small fly-wheel shown in Fig. 1.

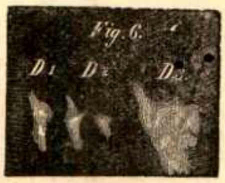
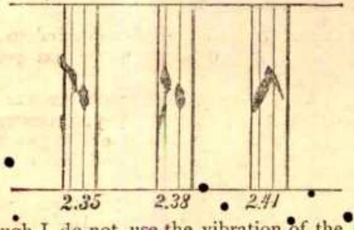


Fig. 6.

But I find, that although seen in this way, the prominences appear very bright; yet the working of the apparatus always causes a slight oscillation of the equatorial, which interferes with the definition of details, and I prefer to work with the slit simply opened. When the air is free from haze, the whole extent of a prominence 30,000 miles in height is readily examined through the C or F line, and the most delicate details reveal themselves with a beauty and clearness of definition which even yet always surprises me, and speaks most emphatically for the exquisite workmanship of the 43 different surfaces by which the light is either refracted or reflected on its way from the slit of the collimator to the eye.

Fig. 7.



But, although I do not use the vibration of the slit and diaphragm, I find the mobility of the slit so convenient as to be practically indispensable. In examining the spectrum of a group of sun-spots, for instance, it is very much easier to move the slit to the particular point we wish to observe, than to move the solar image by the tangent screws of the equatorial.

Photographs of the Solar Protuberances

The protuberances are so well seen through the F and 2796 (near G) lines, that it is even possible to photograph them, though perhaps not so satisfactorily with so small a telescope as the one at my command. Some experiments I have recently made show that the time of exposure, with ordinary portrait collodion, must be nearly four minutes, in order to produce images of a size which would correspond to a picture of the solar disc about 2 in. in diameter. This length of exposure demands a more perfect clockwork than my instrument possesses, and a more accurate adjustment of the polar axis than it had during the experiments, as well as a steadier condition of the atmosphere.

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[*] On returning from the Eclipse Expedition my instrument will be made automatic in accordance with this plan.

Thus far, therefore, I have not been able to produce anything which could properly be called a good picture. Negatives have been made which show clearly the presence and general form of protuberances, but the definition of details is unsatisfactory. This amount of success was reached upon September 28, when impressions were obtained of two protuberances on the S.E. limb of the sun, and, slight as this success was in itself, I consider it of importance in showing the perfect feasibility of going much further with more sensitive chemicals, more delicate adjustments, and greater telescopic power. I was aided in the experiments by Mr. H. O. Bly, our local photographer, to whom are due my warmest acknowledgments for the interest, patience, ingenuity, and skill with which he assisted me.

We worked through the Hydrogen γ line (2796 of Kirchhoff's scale) which, though very faint to the eye, was found to be decidedly superior to F in actinic power. The photographic apparatus employed consisted merely of a wooden tube, about 6 in. long, attached at one end to the eye-piece of the spectroscope, and at the other carrying a light frame. In this frame was placed a small plate-holder, containing for a sensitive-plate an ordinary microscope slide, 3 in. by 1. The image of the prominence seen through the open slit, is magnified and thrown upon this plate by the eye-piece. Fig. 1 shows the instrument with this apparatus attached.

It would be easy to improve this arrangement in many respects, and whenever I resume the subject I propose to do so.

As the equatorial, however, has been dismounted, to be put in order for the observation of the December eclipse, further attempts in this direction must be postponed until next spring.

Observations of the Solar Protuberances

Without prolonging this article with the detail of observations, I add a few of the results which have been obtained since Sept. 10.

About forty different prominences have been more or less carefully observed; sixteen have been sketched. Most of them fall, naturally enough, into the categories established by Zöllner and Lockyer, and are fairly represented by figures already published in the *Journal of the Franklin Institute.*

A few deserve especial mention, however. Fig. 3 represents a small one which was observed upon the E. limb of the sun, on September 14, about 4.30 P.M. From the point marked A, which was very brilliant, a small fragment detached itself and rose towards A′, enlarging in size and growing fainter as it rose. It disappeared (from faintness) in about 12½ minutes, at a distance of 2′ 30″ above the limb of the sun, as determined by the time, 8″·5, which was occupied by the intervening space in passing over the slit of the spectroscope. Allowing for the obliquity of the motion to the parallel of declination, the length of path passed over by this cloud was more than 90,000 miles, and the velocity above 120 miles per second.

Fig. 4 represents a prominence observed September 20, at 4 P.M., on the S.E. limb. (Pos: S., 60° E.) It was a nearly vertical stream, made up of spindle-formed filaments, and had attained the enormous height of 3′ 20″ or 90,000 miles (determined, as in the case above mentioned, by a time-observation, corrected for inclination). It was very brilliant near the base, and at two or three other points along its length. At 4.30 it was nearly gone, only a few faint wisps of cloud remaining.

Another, observed on September 27, at 4.10 P.M., and situated on the W. limb of the sun, is represented in Fig. 5. It was formed of separate, well-defined narrow streamers, which appeared to consist of matter, first violently ejected, and then as violently deflected, by some force acting nearly at right angles. The altitude of the highest point was 1′ 25″, the length of the whole about 3′ 30″. I am unable to see how any mere projection from the sun could have produced such a form, and cannot help feeling that it indicates a something in which powerful currents may exist, even at great elevations above the solar surface; in short, an atmosphere extending far beyond the limits which calculation would seem to assign as possible. Is it wholly unlikely that at such an enormous temperature the law of Mariotte may fail so completely as to destroy the reliability of any computation that assumes it as one of the data?

Upon the next day the prominence still persisted, but its type was wholly changed: it was replaced by one of the mushroom-formed masses which are so common.

Bright Lines

In the spectra of different protuberances, the following bright lines have been observed, the numbers referring to Kirchhoff's scale: C; D_1; D_2; D_3; 1474; 1515; b_1; b_2; b_3; b_4; 1990; 2001; 2031; F; 2581·5; 2796; h—17 in all. On one occasion, September 27, the base of a prominence on the N.W. limb, close to a spot just leaving the limb, exhibited as many as twelve or fifteen short bright lines between E and F, which are not included in the above enumeration, as I had not time to identify them. It is the only instance in which I have seen this phenomenon, more than once described by Mr. Lockyer.

I desire to call special attention to 2581·5, the only one of my list, by the way, which is not given on Mr. Lockyer's. This line, which was conspicuous at the eclipse of 1869, seems to be *always present* in the spectrum of the chromosphere, and shows the form of its upper surface or of a protuberance nearly as well, though of course not so brightly, as the 2796 line. It has no corresponding dark line in the ordinary solar spectrum, and not improbably may be due to the same substance that produces D_3.

The reversal of the sodium and magnesium lines is not at all uncommon. In some instances these lines were so bright that on opening the slit the form of the prominence could be made out through them. This was the case with a small hand-shaped prominence observed on September 27. Comparing the form thus seen through D' and D_2 with that given by D_3, it appeared that the sodium line was sufficiently developed for observation only along the edge and at one or two bright points in the prominence, most brilliantly neither at its summit nor its base. Fig. 6 represents the appearance (the slit was perpendicular to the sun's limb). The case was similar with the magnesium lines.

Spectrum of Solar Spots

Several spots have been carefully examined at different times; most of them, in their spectra, gave evidence of unusual disturbances; but by far the most interesting phenomena were exhibited by a large group which was first observed near the E. limb on September 19. Changes of wave-length were frequent in its neighbourhood.

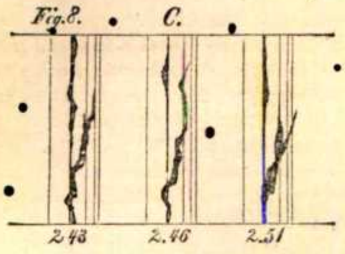
Figs. 7 and 8 represent the appearances assumed by the F and C lines respectively, at the times indicated below each figure, during an observation on the afternoon of September 22. The point where these changes of wave-length occurred was at the western edge of the penumbra. At other times similar changes were observed, but not so great or rapidly varying.

The calcium and titanium lines referred to in my note published in the July number of the *Journal of the Franklin Institute*, were always conspicously thickened in the nucleus spectrum.

The C and F lines were reversed in some portion or other of the group nearly every time I observed it. On September 22 the sodium lines were both reversed for several hours, while D_3 appeared as a dark shade. On September 28, again, at 4 P.M., the southern nucleus of the group (which at this time contained four large umbræ, besides many small ones) reversed all of the following lines, viz.: C; D_1; D_2; D_3; 1474; b^1; b_2; b_3; b_4; F; 2796, and h. All of these were conspicuous, except 1474; D_3 and b_3 especially so, and the latter (a nickel line) showed considerable changes of wave-length, alternate increase and diminution, which were not shared by its magnesian neighbours, b^1, b_2 and b_4.

At 4·05 P.M. the brilliance of the F line increased so greatly that it occurred to me to widen the slit, and to my great delight I saw upon the disc of the sun itself a brilliant cloud in all its structure and detail identical with the protuberances around the limb. Indeed, there were *two* of them, and there was no difficulty in tracing out and delineating their form. Fig. 9 represents them as they were from 4·05 to 4·10; Fig. 10 gives the form at 4·15 to 4·20. They were then considerably fainter than at first. During the intervening ten minutes I examined the other lines of the spectrum, and found that the form could be distinctly made out in all the *hydrogen* lines even in h; but that the reversal of the other lines, including D_3, was confined to the region immediately over the spot-nucleus, where the smaller but brighter cloud terminated abruptly; or, I might better say, *originated.* The larger one faded out at both ends. When the clock-w of the equatorial was stopped, the luminous cloud took 16·7 seconds of time to traverse the slit which was placed parallel to the hour-circle. This indicates a length of at least 130,000 miles without allowing anything for the foreshortening resulting from the nearness of the sun's limb.

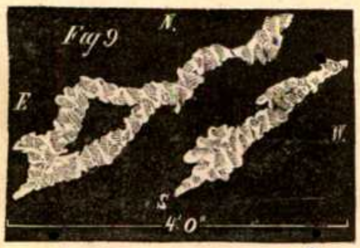
Fig. 8.



By five o'clock the clouds had nearly disappeared; a little rack alone remained.

At 4.20 I examined the spot with the equatorial, using the ordinary solar eye-piece. Nothing remarkable was to be seen—not the merest trace of the enormous masses of incandescent gas.

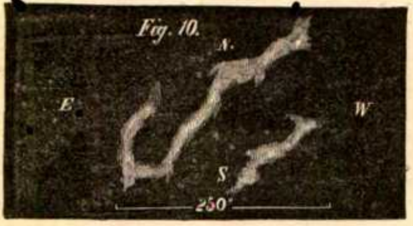
Fig. 9



It will be interesting to learn whether the earth responded to this magnificent eruption on the sun by any magnetic storm.

I may add that in the telescope this group of spots, from their first appearance, exhibited a strong yellowish tinge, which appeared to overlie all the central portion of the cluster. So conspicuous was it that several persons, unaccustomed to astronomical observation, noticed it at once before I called their attention to it. The penumbra of the group was unusually faint.

Fig. 10.



Hanover, N.H., October 3 C. A. YOUNG

NOTES

THE first detachment of the Eclipse Expedition for Spain and Algiers started from Portsmouth in H.M.S. *Urgent* on Tuesday morning; the Sicilian party followed overland yesterday evening. An error crept into the names of the party in our list published last week. Professor Roscoe's assistant is Mr. Edward Ernest Bowen, M.A., late Fellow of Trinity College, Cambridge.

AT one of the most recent sittings of the French Institute a communication was made by M. Faye on the intended departure of M. Janssen to join the Eclipse Expedition. This celebrated astronomer was to leave Paris in a balloon constructed for his private use at the expense of the French Government, and which will also carry a telescope, constructed in eight weeks by the most skilful workmen in Paris, and of the capacity of 2,000 cubic metres. It will be fitted up with a new kind of valve, invented by M. de Fonvielle, and which was not quite ready when that gentleman left Paris. According to every probability M. Janssen will ascend with M. Tournier, one of the passengers in the "Pole Nord," but taking the management of the balloon under his own care. The telescope was put in hand a very few days after the 4th of September. The Government appeared so anxious to show the interest taken in the matter that they did not lose a single day after they came into power in fitting up this expedition. The expenses of the construction of the telescope were incurred by the Bureau des Longitudes.

THE French *Académie des Sciences* has held its sittings regularly since the commencement of the siege, and the *Comptes rendus* has been published regularly every week. Every sitting is reported fully, and several numbers have had even more than the average number of pages. A large part of them is devoted to military science and to ballooning. The scheme put forward by M. Dupuy de l'Ome was fully discussed and illustrated by copper-plates: an article contributed to the *Presse*, by Mr. Giffard, the celebrated engineer, when reporting upon his aërial experiments as much as twenty years ago, has been reprinted. It was shown that Dupuy de l'Ome's experiment was almost of the same nature, and the *Académie des Sciences* has apologised for not publishing it in proper time. M. Dumas and M. Elie de Beaumont, although members of the former senate, now act in their capacity of *sécretaires perpetuels* of the Academy. M. Leverrier has not appeared at any of the sittings. M. Chasles is most punctual in his attendance. Lectures are given at the *Conservatoire des Arts et Métiers*, and are to be given at the *College de France.* No lectures have been given this session at the *Sorbonne.* Since the commencement of the siege, a few numbers only have been issued of the *Révue des Cours Scientifiques*; *Les Mondes* and *Cosmos* have been entirely suspended.

A LARGE number of the animals at the *Jardin des Plantes* and *Jardin d'Acclimatation* have been sold and slaughtered for food, even the bears having now been sacrificed. The trees in the latter garden have been almost entirely cut down either for charcoal or for the necessities of the defence.

WE are very glad to be able to report that Sir Roderick Murchison has gained strength during the past week, though his state still continues critical. Prof. Balfour Stewart will have to remain at Harrow for at least some weeks, but is progressing as satisfactorily as possible, considering the nature of his injuries.

THE ratepayers of Marylebone have done good service to Science in electing Miss Garrett, M.D., and Prof. Huxley to the London School Board, first and second on the poll, the former by a triumphant majority over every other candidate. If their example is generally followed throughout the country, we may anticipate great things in the future for the scientific education of the country.

OWING to Professor Tyndall's absence with the Eclipse Expedition, the first conversazione at the London Institution, announced for Dec. 21st, is postponed till Jan. 25th, when he will deliver his lecture on Dust and Disease.

WE regret to learn that the North London Naturalists' Society terminated its existence on Monday, Nov. 27. It has been established for about six years, and was for some time carried on with spirit; but the interest in it has for some time been on the decline, and it was considered advisable to bring it to a close. We understand that it is in contemplation to form another society in its place, which only actual workers will be invited to join. When we consider how many useful bodies of this description are scattered throughout the country, it seems strange that a similar one cannot be kept afloat in London; but the fate of the North London Club, preceded as it was by the collapse of the Society of Amateur Botanists, and of the West End Naturalists'

Society, is somewhat discouraging, and a well-supported body of workers in all branches of Natural Science seems likely to be a desideratum.

PROF. HASSKARL reports that the cultivation of Cinchona in Java is proceeding satisfactorily. The weather, on the whole, has been favourable, and the growth of the plants leaves nothing to be desired. The number of plants grown from seeds and layers is upwards of one and a half millions, by far the greater number belonging to the species *C. calisaya*, a good many to *C. officinalis* and *succirubra*, and a very few to *C. lancifolia* and *micrantha*. In addition to these, 870,000 plants have been transplanted, and the whole shows an increase of nearly 200,000 plants since the commencement of the year. 460 kilogrammes of the dry bark were sent to Holland in December, 1869, and sold at from two to three florins per kil.; 900 kilogrammes have since been exported, and more than 1,000 were ready at the date of the despatch. Prof. Hasskarl reckons the total produce of 1870 at no less than 4,000 kilogrammes of dry bark for exportation, besides some hundreds for home use in the island. An important branch of industry in the colony is now formed by the stripping, cutting, drying, sorting, and packing of the Cinchona.

THE report of the Hartley Institution, Southampton, for the current year gives a very favourable account of its position and prospects. The students in the day classes have increased from twenty-nine last year to eighty-six; in the evening classes there have been thirty-one. Among the students of the engineering department there have been as many as eighteen government nominees to the Telegraphic Service of India, who have been pursuing an advanced course of instruction in mathematics and physical science to qualify them for their final examination. A class of fourteen nominees has also been under instruction in the Institution during the year in practical telegraphy. At the last final examination for appointments to the Indian Telegraphic Service, all the successful candidates, except one, had been instructed in the Institution. There is a library of 400 or 500 volumes in connection with the Institution, a reading-room amply supplied with the current literary, scientific, and political publications of the day, and a good course of lectures was delivered during last session.

A WRITER in the *Boston Post* of October 24 thus describes the present condition of the Museum of Comparative Zoology in that city:—Since the acquisition of the private collection of Prof. Agassiz, the Boston Museum may claim to rank among the foremost institutions of its kind; for although the British Museum in London and the Jardin des Plantes in Paris are on a very much larger scale, yet in certain departments, such as corals and fishes, the Museum of Comparative Zoology is superior to both, while the increase of its collections since its existence, and the prominence it has attained among other museums, are such as no like establishment has reached in the same time and with the same means. From want of room the greater part of the Museum as it now exists is occupied by working-rooms and store-rooms, and only four rooms are devoted to exhibition. Each of these contains the representatives of one great division of the animal kingdom, and it is intended to complete them in such a manner that they shall exhibit in an easy and conspicuous way the natural relations of all the animals known in creation. In the new building now going up, which adjoins the present Museum and is to be of equal dimensions, it is intended to exhibit all the animals peculiar to the different parts of the world, in such a manner as to impress the observer with their natural association in nature, so that the student of Natural History shall be able to make himself familiar in one part of the building with the latest result of scientific research in working out the system which binds together the whole animal kingdom as a unit; while in the other part of the building their geographical distribution upon the whole surface of the earth, and their various combinations and associations on different continents will be made apparent. Such a twofold arrangement of collections has never yet been attempted in any museum, not even in the largest and most prominent institutions of the kind in Europe. The fossil remains of past ages will be exhibited in like manner in such an arrangement as to display at the same time their order of succession in geological periods, and their relations to the animals now living. It is intended to complete this plan by exhibiting also the different stages of all known animals, from their earliest period of development in the egg to their adult condition. This is a truly magnificent plan, but although the addition to the Museum will double the amount of room, yet the whole of this plan cannot be carried out at present, and a large part of the collections must still remain in the store-rooms until another section of the building can be completed. It is sincerely to be hoped that the strong interest which has already been shown by the Legislature of Massachusetts and the citizens of Boston will not flag, and that sufficient aid will be given to carry out and fully complete this admirable work, within the lifetime of the distinguished man who has done so much to elevate the tone of thought, and improve the method of education in this his adopted country.—We imagine it must be only typical animals of each group that are referred to in the above account. It is to the munificence of a private patron of this Museum, Mr. Nathaniel Thayer, that Science is indebted for Prof. Agassiz's recent exploration of Brazil, with six trained assistants.

A SECOND paper on "Mystic Trees and Flowers" appears in *Fraser's Magazine* for this month. Herbaceous plants form the subject of this portion, the mandrake (of the history of which an especially interesting sketch is given), mistletoe, rose, lily, violet, and primrose, being the principal which are touched upon in detail. The author, Mr. Moncure D. Conway, in some concluding remarks, states his belief that "the reverence paid to trees and flowers" must be looked upon "not as fetish worship, but as a sacred regard paid to them as oracles of beings higher than themselves," and combats the idea "that it was from these lower objects that reverence gradually ascended to the adoration of the sun and stars, as the case was really the reverse."

THE *British Medical Journal* quotes the following description of "The Nemesis of Tobacco," from the ninetieth observation of Theodorus Kerckringius, M.D. (*Spicilegium Anatomicum*, Amsterdam, *circa* 1670) describing the *post mortem* appearances of an inveterate smoker:—"Too greatly, now, alas! in Europe, prevails that *cacoëthes* of sucking up the smoke of the herb tobacco, as they call it, through tubes actually manufactured for that special purpose! In consequence, what a perversity of morals has arisen they must have noted whose duty it is to attend to the public morality, whether they be politicians or theologians. How noxious it is to the health of those who indulge in the habit of sacrificing so often to Vulcan, or rather to Charon, I shall not here explain. Let it suffice, that I adduce the case of a man whose body I opened before the Faculty. He, ordinately given to these fuliginous delights, had scarcely ever engaged in any kind of work, as it appeared, without inhaling this fatal juice. When, however, at length, Nature, assailed by frequent attacks, began to fail, and to give way to disease, he rejected for so long a time a black-looking matter, both upwards and downwards (*per utrumque gutturem*), that at last he vomited forth his dusky soul; which to accompany in its visit to the realms of Pluto would be far from agreeable, for, I suspect, it would greatly, and that from habit, have preferred those black lakes, steaming with the bubbles of Stygian vapours, to the lucid stars of heaven, inasmuch as it had long been fed, though no nourished, by smoke; the abode, however, it had relinquished, I visited and examined by the aid of the scalpel of the anatomist. What did I observe, you ask? It appeared to me that I was passing into the very house of Pluto himself; even the entrance-doors were tinged of a black colour, and the tongue, imbued, as it were, with the poisonous juice, was in a state of tumefaction. What as to the windpipe? It was like the inside of a chimney, coated completely with black grime. The lungs were dry, sapless, and scarcely at all friable. The liver, as if it, beyond all the other organs, had attracted the fire, was altogether inflamed; from the flames of this fire not even the bile in its receptacle had been safe, for its colour had changed from purple to green (*ex purpureo virescentem*). In the intestines, however, the drains of the body, the carbonaceous matters from the whole combustion had become concentred, for they were full of a black substance which exhaled no milder stench than that of Hell itself. Such, of this frequent suction, are the medicinal fruits!"

At the meeting of the Geologists' Association, on Friday evening last, Mr. R. Etheridge read a paper on "The British Islands Past and Present, Physically considered." After making some remarks on the intense interest and importance of the subject, he proceeded to describe the distribution of land and water at different geological epochs, and to show that England, Ireland, and the Continent, were once united, and that the many and great changes which have taken place have arisen from the elevation or depression of the land, not from alteration of the sea level. Mr. Etheridge then referred to the changes which have taken place in the relative positions of land and water during the historic period, giving instances of towns and cities that in the time of their prosperity stood some distance from the sea, but have been gradually submerged, and other places, whose importance arose from their contiguity to the ocean, now left high and dry inland. The paper was illustrated by a splendid collection of diagrams.

The Seventh Annual Report of the Belfast Naturalists' Club shows that this useful Society is making good progress. By the kindness of the Council of the Natural History Society, members of the Club have been permitted to re-arrange the valuable local collections in the Museum; and a large sum has been granted by the Council for cases, &c., in which to exhibit a complete local collection. The local land, freshwater, and marine shells have been named and arranged; the Herbarium is in progress; and the Geological collection selected and named, ready for mounting. The Committee have also considered it desirable that the Club should prepare complete lists of the fauna, flora, geology, and archæology of Ulster, by publishing an annual contribution to such a work in addition to the ordinary report. The Appendix in the present issue consists of a list of the Irish Liassic Fossils, with notes on new and critical species, by Ralph Tate, Esq., F.G.S. The number of species enumerated is 189; of which the following are new to science:—*Chemnitzia punctata, Solarium thompsoni, Tornatella robinsoni, Pleurotomaria tectaria, Hinnites angularis, Avicula pattersoni, Leda v-scripta, L. quenstedti, Cucullæa grangeri, Mytilus subtilis Thracia æquata, Anatina myacina, Pollicipes alatus.* Figures of many of these are given in an accompanying plate, and the paper is a valuable contribution to local geology. We congratulate the Belfast Club on having successfully started a work which will give a permanent value to their annual reports, and trust that their example will be widely followed by other local societies.

In the Chittagong district the Government of India has discontinued explorations for coal at present, as the samples found are of an inferior and unpromising quality.

BALLOON ASCENTS FOR MILITARY PURPOSES

As soon as the war broke out, balloons, which had been so long forgotten by statesmen, were recalled to their memory by hundreds of projectors. Some of the schemes suggested were of the wildest description, and scientific men took advantage of this circumstance to reject everything connected with aeronautics. But surprises and reverses became so frequent in the French army, that it became evident that any apparatus able to carry observers would be considered as a preserver from such disgraces. As soon as it was clear that the Prussians were intending to besiege Paris, the Minister of War issued orders for the construction of a captive balloon, intended to watch the movements of any besieging army moving round the capital; but instead of having recourse to Mr. Giffard, the constructor of so many magnificent balloons, it was resolved to employ MM. Godard and Nadar. Paris was divided into aerial districts, the first being given to Nadar and the other to Godard. Nadar then received orders to establish his balloon on the foot of Buttes Montmartre, and Godard close to the Montsouris Meteorological Observatory on the banks of the small streamlet Bièvre, where it crosses the fortifications. The balloons intended to be attached were not made on purpose, they merely used old ones which were worn out; the gas-pipes were also not sufficiently large, and the gas-pressure was very low, so that when the first attempts at inflating were made, the Godard balloon took more than three days to be filled; and, when filled, was tossed so heavily by the wind, that it was necessary to let the gas escape. Nadar was still more unfortunate, and could not arrive even at the inflating of his balloon, except after immense labour, by laying a pipe along the ground for a space of more than 300 yards. Moreover, when the first balloon was floated, it was as late as the 4th of September. I then ordered Godard to continue his inflating process. Many scientific bodies met, and deliberated upon the modes of improving captive balloon ascents, but none of the members had ever ascended, and hence their practical knowledge was so small as to amount practically to nothing.

I tried to improve in some respects the construction of captive balloons, by using the process of fixing to the rope invented and practised by the aëronauts of the First Republic, and offered to the State a balloon, which had been given to me by my friend, Mr. Giffard, and which had, unfortunately, only a capacity of 28,000 cubic feet. I had already used this balloon for an ascent, executed for the benefit of the Arène de la Rue Monge.

That balloon was fitted up in a more scientific manner, the appendix being also firmly attached with rope, so that the pressure of the wind could not let a single puff of gas escape. The equatorial ropes were attached together and connected by means of little pulleys, the pulleys being connected by ropes, and so on till the whole of the network ended in three large ropes. This machinery worked admirably well, but the material of the balloon was not fit for the purpose. After two or three weeks' standing, the company was dissolved, and the balloon was sent free into the air. Captive observations were not so useful as had been hoped; that partial failure was owing to the unfitness of officers entrusted with the duty of making observations, and of the men employed in the drawing of the ropes. Nothing of the kind would have happened if Government had accepted the offers made by Mr. Giffard before the beginning of the war. That great engineer had offered to spend 40,000*l.* in the construction of a large balloon of 15,000 cubic metres capacity, able to carry 40 persons to the height of a full kilometre, but the Government had refused this proposal because Mr. Giffard asked for a place in the Champs Elysées, where it would have been necessary to displace a few shrubs.

When the investment of Paris was completed, the question naturally arose of using balloons for carrying messages, the resolution having been taken by the minister, M. Rampont, Post-office Director, to summon to his office several aeronauts, Nadar, Artoise, myself, and a few gentlemen supposed to be acquainted with aërostatics; and the ascent was decided upon in a long discussion.

The first who ascended, was Duruoff with his own balloon, famous from several ascents. Duruoff started up into the air early in the morning, and employed an immense lifting power, the wind blowing strongly besides, and Duruoff disappeared like a dream. He was alone in his car, carrying a bag of letters, with plenty of ballast; I protested in the most urgent manner against sending into the air a single man unassisted, but without any success. The advice was neglected in consequence of the success of the first operations. Reverses were necessary to call postal authority to a better sense of the real state of things. M. Garnier Pagès, a member of the new Government, invented the carrying by balloons of aerial pigeons, and the second balloon ascent was the occasion of the first pigeon expedition.

One of the aeronauts known to our readers is Mangin, the proprietor of the unfortunate "Union," of which the wreck was fully described, who tried an ascent a few days after Durioff. He made a foolish agreement with the Post Office, to carry with his poor worn-out balloon a weight of 1,000lbs., but the balloon was unable to retain a single puff of gas, and the attempt was doomed to failure.

Two or three days afterwards, Mangin tried another ascent with the "City of Florence," a large balloon of 1,200lbs. capacity and belonging to Eugène Godard.

The "City of Florence" was inflated and fitted up by its proprietor, and left the ground on the morning of a clear day, with a light north-easterly wind. It carried with Mangin a medical man practising at Lyons, with a special mission from the Government for the eastern departments. The ascent succeeded very well, and Dr. Lutz was landed safely. But the landing of Lutz gave rise to a singular circumstance. A Prussian spy, having read in the papers that Lutz had come down from the heavens, presented himself at Dijon as the real Lutz, and acted in accordance with that suggestion. The fraud was not discovered without some delay and some trouble, but owing to some peculiar circumstances it was at last exposed, the false Lutz was seized, tried by a court-martial, condemned to death, and shot on the spot.

Amongst singular ascents one was executed two or three days afterwards by Louis Godard, carrying with him two merchants. Godard's balloon being too small for the purpose, he fixed one additional balloon to the end of a long narrow piece of wood, and in the middle of that long singular bridge there was a second balloon of no more than 100 cubic metres. The floating of this extraordinary trio created a great deal of amusement among our Belgian people, and is in itself an aerial aeronautical success; Godard landed near Nantes, where the Prussians had not yet set their feet.

Next to Godard's singular ascent, we must mention the one executed by Trignot for carrying Gambetta to his post at the head of the Government. An accident took place in the air while it was open, and the balloon emptied itself at an extraordinary rate, landing, against the will of the aëronauts, in Prussian territory. If sharpshooters had not come to the rescue, Gambetta would have been made a prisoner. Kératry was in the same manner sent in a balloon, and succeeded in escaping after some adventurous feats.

We must mention the ascent conducted by the elder Tissandier, as well as the one conducted by his younger brother. The first of these two ascents was remarkable for the firing at it by the Prussians when the balloon was passing over Versailles. Tissandier, as well as his brother, fell beyond Prussian territory, but not far from the enemy's force.

From the time of the landing up to the present moment, the brothers Tissandier have tried twice to return to Paris; but they ascended from Tours, which is a bad station for such a purpose. The first time their balloon was sent towards the south; the second time it ascended too high, and the brothers were conducted into a frozen cloud, which compelled them to come down. This second ascent was tried during the night, which is a decidedly awkward time, as an aerial traveller is unable to find his way to the land.

Returning to Paris will be tried, however, and is a great object to be attained, but for success to be secured requires more powerful means. I must not omit my own arrival in Belgium in a balloon. The "Egalité," which I employed on that occasion, had a younger brother called "Liberté." The "Liberté" was inflated with pure hydrogen, prepared specially for the purpose. It was the old captive of the Universal Exhibition, fitted up and carefully repaired for the purpose. "Liberté" was intended to carry ten persons, besides 3,000lb. weight of letters. It had two cars attached, the one fastened to the other by eight ropes. The upper car was intended for passengers, the lower one for letter-bags. Everything had been carefully prepared; bags of sand had been attached round the upper car, and a hole had been cut into the bottom to permit communication between the two cars. Unhappily, when the process of inflation was half finished, the wind began to blow with such violence that people holding the net let the ropes loose with so much force that the balloon escaped, turned round like a whirlwind, being lost in a minute in the clouds, and leaving thousands of spectators in consternation. "Liberté" was seen turning round two or three times again between different strata of clouds, and was finally observed turning no more, ascending no more, but falling straight like a meteoric stone. The fall took place within the Prussian lines, and it remained in the hands of the Prussians, who were enabled to repair and to use it for their purposes.

Seeing that I was unable to recover my balloon, I managed to get another constructed. The new balloon, though smaller than "Liberté," was larger than any other balloon in existence in Paris. It had a measurement of 3,000 tons instead of 2,000, the average. The ascent was delayed by an accident which happened during the process of inflating; a hole was discovered round the appendix, and the valve was open. For the three following days the weather was unfavourable, and the passengers were obliged to come back every morning, but the following morning several thousand people witnessed the ascent, which was very successful. After having landed in Belgium, I came to London on my way to Tours, and purpose shortly attempting to re-enter Paris by the same means.

W. DE FONVIELLE

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, viii. Suppt. Bd. 1 Heft. This number opens with a paper "On the occurrence of Ammonio-Magnesic Sulphate in the lagoons of Tuscany," by Dr. O. Popp, who has observed that a double salt of ammonic and magnesic sulphates is of constant occurrence, together with ammonic sulphate and boric acid, in the lagoons. He also remarked that the relative amounts of boric acid and ammonic sulphates are inversely proportional, so that those containing large quantities of boric acid contain but little ammonic sulphate, and *vice versa*. The salt, which separates out either during the concentration of the water or in the crystallising vessels, is obtained pure by recrystallisation in forms belonging to the monoclinic system, and of the composition

$$Mg\,SO_4 + (NH_4)_2\,SO_4 + 6\,OH_2.$$

In the natural double sulphate Mg is often replaced by the isomorphous Mn and Fe.—"On the origin of the Boric Acid in the Fumaroles of Tuscany" is by the same author. After a criticism of the theories of Dumas and Bolley on this subject, he quotes the observations of Woehler and St. Claire-Deville, that boric nitride heated in the presence of aqueous vapour is decomposed into boric acid and ammonia, and that at a high temperature boron and nitrogen combine directly; they remarked that the presence of ammonia salts together with boric acid in volcanic craters and the lagoons of Tuscany might be due to such a decomposition. This the author also considers to be the probable explanation, and assumes that boric nitride is present in these volcanic localities, which come in contact with water at a high temperature, forming boric acid and ammonia; this latter combines with sulphuric acid, formed by a process of roasting from the layers of pyrites, or of coal containing pyrites, which would also account for the presence of marsh gas and free hydrogen in the lagoon gases.—"O Chloranil and Bromanil," by J. Stenhouse. A modificatio of Grœbe's method of preparing chloranilic acid is described Chloranilic ether was obtained by the action of ethylic iodide o the silver salt. Nitric acid of sp. gr. 1·45 oxidises chloranili acid to chlorpicrin and oxalic acid. By the action of bromin on chloranilic acid a compound of the formula $C_6\,Br_8\,Cl_3\,O$ was obtained. Bromanil is readily obtained by the action of mixture of one part iodine and two parts bromine on phenol Bromanilic acid was prepared in a similar manner to chloranilic and in all its reactions found to be analogous. A compound $C_6\,Br_{11}\,O\,H$, was obtained by the action of bromine on it.—"On Coumarin, Hydrocoumarin, and Hydrocoumarinic Acid," by C. Zwenger. The author observes that the preparation o coumaric acid from coumarin is attended with considerable diffi culty, inasmuch as the acid is first formed at a temperature a which the potash readily causes a further decomposition. most characteristic and delicate reaction for coumaric acid is th fine pea-green colour of its solutions when viewed by reflecte light, and which is perceptible when only traces even of the ac are present. As is well known, melilotic acid is obtained b the action of nascent hydrogen *in excess* on coumarin; the auth finds, however, that if the coumarin be kept in excess a new acid which he calls hydrocoumarinic, is formed. This acid is derive by the addition of H_2 to two molecules of coumarin, and has the omposition $C_{18} H_{18} O_6$. On heating in a water bath, or ven on heating with dilute HCl or $H_2 SO_4$, it loses water nd is converted into the anhydride $C_{18} H_{14} O_4$, which may also be obtained directly from coumarin.—"A New Method of Synthesis of the Organic Acids," by M. Berthelot. On allowing acetylene mixed with air to remain in contact with dilute potash solution for six months, the acetylene was found to have been converted partly into acetic acid and partly into a bituminous substance, containing C H and O. Employing a solution of chromic anhydride, a more perfect conversion of the acetylene to acetic acid took place. A lylene reated in the same way gave propionic acid; but it is probable hat an intermediate compound $C_3 H_4 O$ is formed by the direct addition of O, and which should also be obtained by the abstraction of OH_2 from propionic acid. Propylene gave propionic acid, acetone, and acetic acid; even carbon was oxidised by this reagent, giving small quantities of oxalic acid. It deserves notice that a solution of pure chromic anhydride is a much gentler oxidising agent than the usually employed mixture of sulphuric cid and potassic dichromate, probably partly by reason of its volving a smaller proportion of oxygen. By its decomposition, free oxygen and chromic chromate, a salt corresponding to ferric sulphate, are formed:

$$5\, Cr O_3 = Cr_2 O_6\, 3\, Cr O_2 + O_3$$

'On the synthesis of aromatic acids, by A. Wurtz." By the action of $Na_2 Hg$ on a mixture of chlorocarbonic ether and bromoluol, the ethers of toluylic and isotoluylic acids were formed, which is explained by the fact that bromtoluol is a mixture of wo isomeric compounds. On treating a mixture of chlorocarbonic ether and benzylic chloride, the following more complicated reaction takes place:

$$2\, C_7 H_7 Cl + CO\, Cl\, OC_2 H_5 + 3\, Na = 3\, Na\, Cl + H + C_{14} H_{13}\, CO\, OC_2 H_5$$

(Dibenzylcarbonic ether.)

The first action of the sodium is probably to form chlorinated dibenzyl, $\left\{\begin{matrix} CH_2 C_6 H_5 \\ CH\, Cl\, C_6 H_5 \end{matrix}\right.$ from 2 mols. of benzylic chloride, and this by the action of Na and CO Cl $OC_2 H_5$, becomes converted into $\left\{\begin{matrix} C H_2 C_6 H_5 \\ C H\, CO\, OC_2 H_5\, C_6 H_5 \end{matrix}\right.$ By the dry distillation of its lime salt, stilben and dibenzyl are obtained:

$$2\, (C_{15} H_{13} O_2)\, Ca = Ca\, CO_3 + CO + C_{14} H_{12} + C_{14} H_{14}$$

—"On the molecular weight of some Protoxides," by A. Ladenburg. This is really so important and interesting a paper that is difficult to give an idea of its contents in the short space at ur command. Although the determination of the vapour density is still the most certain method of establishing the molecular volume, it cannot be denied that the true interpretation of chemical metamorphoses also affords a means of fixing the molecular formulæ of compounds. The author also considers this to e the case in those reactions which cause a change of type, viz., in cases of direct addition. The molecular volume of the resulting product permits in many cases at least an inference as to the formula of the unsaturated compound. For instance, from the passage of ferrous into ferric chloride, he believes himself justified in drawing the conclusion that $Fe_2 Cl_4$ is the formula f the former, $Fe_2 Cl_6$ being that of the latter, as is proved by its vapour density. The same may be urged of Cr and Mn compounds, although not with the same certainty, and it was to btain further proofs in this direction that the author strove. By the action of a mixture of 3 eq. acetic and 1 eq. formic acid on manganous carbonate, a compound $Mn_2 (C_2 H_3 O_2)_3\, C H O_2$ was obtained, but it is not placed beyond all doubt that this is not a mixture of two salts. A series of experiments with tin compounds were then undertaken with the view to ascertain whether the compound $Sn_2 Cl_6$, intermediate between $Sn Cl_2$ and $Sn Cl_4$ ould not be obtained. By the action of Br on $Sn Cl_2$, $Sn Cl_2 Br_2$, $Sn Cl_3 Br$, and $Sn Br_3 Cl$ were formed, revealing the curious act that similar molecules so react on one another that dissimilar molecules result:

$$Sn Cl_2 Br_2 + Sn Cl_2 Br_2 = Sn Cl Br_3 + Sn Br Cl_3.$$

No experimental proof of stannic triethide being $Sn_2 (C_2 H_5)_6$ and not $Sn (C_2 H_5)_3$ had hitherto been adduced; it was prepared by the action of Na on $Sn (C_2 H_5)_3 I$, and its vapour density determined and found to agree with that required by $Sn_2 (C_2 H_5)_6$. The action of Cl on stannic triethide is as follows: $Sn_2 (C_2 H_5)_6 + Cl_6 = 2\, Sn (C_2 H_5)_2 Cl_2 + 2\, C_2 H_5 Cl$. By the action of I first $Sn (C_2 H_5)_3 I$, and then $Sn (C_2 H_5)_2 I_2$ is formed. By the action of sodic ethylate on $Sn (C_2 H_5)_3 I$ stannic ethylotriethide $Sn\, O\, C_2 H_5 (C_2 H_5)_3$ was obtained. There then follow a number of similar experiments on stannic methides, &c.—"On Schiel's chloraluric acid," by N. Lubavin. Schiel described an acid obtained by the action of chlorous acid on uric acid, to which he gave the formula $C_{14} H_{11} N_6 Cl O_1$ (old atomic weights); this Lubavin proves to be a mixture ammonic chloride and parabanic acid. He also mentions an oxalate of urea of the composition $C O N_2 H_4, C_2 H_2 O_4 + O H_2$ the formula of the ordinary oxalate being $2\, C O N_2 H_4, C_2 H_2,$ (. In all the text-books 120° is given as the fusing point of urea; he finds it to be 132°.—"On derivatives of Anethol," by A. Ladenburg. On fusing anethol (oil of anise) with caustic potash, the following reaction takes place:—

$$C_6 H_4 \left\{\begin{matrix} O C H_3 \\ C_3 H_5 \end{matrix}\right. + K O H = C_6 H_4 \left\{\begin{matrix} O H \\ C_3 H_5 \end{matrix}\right. + C H_4 O$$

Treated with $P Cl_5$ H becomes replaced by Cl

$$C_6 H_4 \left\{\begin{matrix} OCH_3 \\ C_3 H_5 \end{matrix}\right. + P Cl_5 = C_6 H_4 \left\{\begin{matrix} O C H_3 \\ C_3 H_4 Cl \end{matrix}\right. + H Cl + P Cl_3$$

This body treated with alcoholic potash loses H Cl and gives $C_{10} H_{10} O$, a liquid boiling at 240°. All experiments to replace the Cl in chloranethol by $C_2 H_3 O_2$ failed. Anethol combines directly with Br_2.—"Researches on Vanadium," by Henry E. Roscoe. In this communication are described vanadic tribromide and oxytribromide; also a large number of meta, ortho and pyrovanadates, among others the artificially prepared vanadinite. As this paper appeared in English in the Chemical Society's Journal, it has already been noticed in this journal.—"On the second fundamental theorem of the mechanical theory of heat and its application to several decompositions," by Dr. A. Horstmann. The author has given a popular treatment of the mechanical theory of heat, as he considers that although of the greatest interest to chemists, it has remained comparatively unknown to them by reason of its requiring a somewhat high mathematical knowledge. The decompositions to which it is applied are those of ammonic chloride and calcic carbonate by heat, and also the expulsion of the water of crystallisation from hydric disodic phosphate.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, November 23.—Mr. Joseph Prestwich, F.R.S., president, in the chair.—1. "On some points of South-African Geology." Part 1.—By Mr. G. W. Stow. In this paper, which was illustrated by numerous sketches, sections, tables, and specimens, observations were made on the stratification of the Jurassic beds of Sunday's and Zwarktop's rivers, resulting from researches made by Mr. Stow, with the view of determining the exact position of the several species of fossils found at the exposures on the cliffs of these rivers, and from this the sequence of the various beds. He indicated the existence of at least nine separate fossiliferous bands, pointing out the relative positions of the several *Trigonia*-beds, Hamite-beds, Ammonite-beds, &c. He next treated of the so-called Saliferous beds of the district, and gives his reasons for regarding them as later in age than the *Trigonia*-sandstones above alluded to, and therefore not equivalent to that part of the series named "Wood-beds," by Dr. Atherstone. Other researches of the author related to the Tertiary beds both inland and on the coast. He distinguished three zones on the coast later in date than the high-level shell limestones (Pliocene?) of the Grass Ridge and other parts of the interior. One of the coast-zones he named the *Akera*-bed, from the prevalence of a delicate species of that genus. Another zone was described as following the river-valleys in the form of raised terraces, characterised by the presence of a large *Panopæa*. The latest shell-banks have been thought to be kitchen-middens, but the author regarded them as shore-deposits in place. The author concluded by tracing the probable climatal and geographical changes in this region during geological times, and indicated, as far as his material allowed, the probable migrations of the Mollusca, especially of the *Venericardia* characterising the Pliocene Limestone. Mr. Gwyn Jeffreys remarked that all the shells belonging to the genus *Akera* which he had examined were shallow water or littoral shells. Dr. Duncan remarked on one of the corals as being of a well-known Crag form, the *Balanophyllia calyculus*. Mr. Searles Wood, jun., observed that there appeared some probability on the face of the paper, of the shells of the older post-tertiary beds denoting a warmer climate than the present, instead of, as here, a colder.—

2. "Note on some Reptilian Fossils from Gozo." By Mr. J. W. Hulke, F.R.S., F.G.S. The author described the remains of two reptiles said to have been brought from Gozo by the late Captain Strickland. One of them was a fragment of the symphysial part of the slender mandible of an *Ichthyosaurus*, having teeth of precisely the same character as those of the form from the Kimmeridge Clay described by the author under the name of *Enthekiodon*. For this species the name of *Ichthyosaurus gaudensis* was proposed. The other was the skull of a species of crocodile, for which the author proposed the name *C. gaudensis.* Dr. Duncan suggested that the Ichthyosaurian fossil might be derivative from some secondary rock. He mentioned that Dr. Leith Adams had once sent him an *Aspidiscus cristatus* from the Hippurite Limestone, which was stated to have come from Malta. To account for this, he suggested that the Miocene of Malta might have been supported on beds of Cretaceous age, so fossils from that source might have become indebted in the coral reefs of the later date. Capt. Spratt expressed a doubt of the fossils having really come from Gozo. He did not recognise the cretaceous-looking matrix among any of the rocks of that island, with all of which he was acquainted. The nearest approach to that kind of rock was to be found in the lowest of the deposits near Cairo, which were probably Eocene. Prof. T. Rupert Jones suggested an examination of the Foraminifera in the matrix, with the view of determining its Secondary or Tertiary age. He mentioned the occurrence of rolled nodules of older rocks in beds of later age at Gozo. Mr. Busk stated that a stone of similar character to the matrix occurred in Malta, if not in Gozo, but probably in both. Mr. Hulke, in reply, observed that he had in this paper intentionally left the stratigraphical part of the question untouched, and confined himself to the palæontological aspect of the remains.—3. "On the discovery of a 'Bone-bed' in the lowest of the 'Lynton Grey Beds,' North Devon." By F. Royston Fairbank, M.D. In this paper the author called attention to the occurrence of a thin bed of rock to the west of the harbour of Lynmouth, containing an immense number of fragments of bone, some of them of large size, and associated with massive bodies which he regards as coprolites. The author proposed to call this the "Lynton Bone-bed;" and he thought that its discovery might throw some light on the relative age of the whole series of rocks of North Devon. Mr. Whitaker had examined the beds in company with Mr. Wetherell. He did not agree with the author as to the amount of iron in the beds The bone-remains appeared to him to be those of *Steganodictyum*, which had already been found in the lowest of Devonian beds. He was not prepared to accept the nodules described as being undoubtedly coprolites. Mr. Valpy stated that there were at least a dozen beds on different horizons of much the same character as that described along the coast of North Devon, an account of which had already been published at Ilfracombe.

London Institution, November 24.—Mr. H. W. Field, F.C.S., in the chair. Prof. Morris, F.G.S., delivered a lecture "On the Precious Metals and their Distribution." Having indicated the principal sources of the gold and silver worked by the ancient nations, he explained the distinguishing characters of these two metals, and dwelt at length on their mode of occurrence, geographical distribution, and geological position. Gold usually occurred in nature in the metallic condition, nearly pure or alloyed with certain metals, while silver was found in combination with various elements, and but rarely in the native state. The distribution of gold throughout the world was illustrated by a large map, on which the known gold-yielding localities were plainly marked. Gold was found in rocks, quartz veins, and alluvial deposits. The silurian rocks and the granites associated with them furnished the chief supplies, but the cretaceo-oolitic rocks of Peru, Bolivia, and California, when traversed by dioritic igneous rocks, were also auriferous; showing, according to Mr. David Forbes, that there had been two well-marked epochs of gold intrusion. From both formations, but specially from the silurian, the gold occurring in alluvial deposits had been derived by the enormous erosion which the rocks had undergone at a comparatively late geological period, namely, the Newer Pliocene. The remains of extinct mammalia discovered in the deposits of the Urals, and also in those of Australia, had satisfactorily fixed their geological position. In Australia and California subsequent volcanic flows had covered thick accumulations of auriferous gravels, and had diverted the courses of many streams, so that they no longer conformed to the old valleys. The minerals mistaken for gold were enumerated and their distinguishing chemical and physical characters were indicated. In conclusion, the lecturer alluded to certain points relating to the use of gold in coinage, and called special attention to the brittleness produced by the presence of minute quantities of palladium in the standard alloy. The lecture was illustrated by numerous diagrams and maps, models of famous nuggets, gold-washing apparatus, and many beautiful specimens of native gold.—Dr. Odling's educational lectures "On Chemical Action," delivered on Mondays at four o'clock, continue to attract crowded audiences, comprising a large number of boys and girls from the schools of London and its suburbs.

Linnean Society, December 1.—Mr. Bentham, president, in the chair.—"On the Source of Radix Galangæ minoris of Pharmacologists." The source of the Greater Galangal has long been known to be *Alpinia galanga*, Linn.; that of the Lesser Galangal has been more obscure. Galangal is not used in English medical practice, and on the Continent has become almost obsolete; its export from China is, however, considerable, and is rapidly increasing. During an expedition to the Island of Hainan, a quantity of the root which provides the Lesser Galangal was observed exposed to the sun in baskets. On a subsequent occasion the plant itself was discovered at a spot six miles inland, at an elevation of 100 feet above the sea, growing in a dry red soil, the result of volcanic action. Here it was evidently planted, but was subsequently detected growing wild in jungles in the same island. Twenty or thirty stalks spring from each root, but rarely more than one or two bear flowers. The fruit appears to be the bitter kind of Cardamom figured by Mr. Hanbury. The plant is closely allied to *Alpinia calcarata*, which flowers readily in the Calcutta Botanic Gardens; but was determined by Dr. Hance to be a perfectly distinct and well-defined species, to which he gave the name *Alpinia officinarum*. A diagnosis of the plant was also given by Dr. Hance.—Supplementary note on the Chinese Silkworm Oaks, by Dr. Hance.

MONTREAL

Natural History Society, October 31.—The President, Principal Dawson, in the chair. Mr. A. S. Ritchie read a paper entitled "Aquaria Studies," Part 2. In a previous paper the author had described the habits of some of the larger inhabitants of his aquarium. In the present sketch an attempt was made to illustrate the peculiarities of the microscopic denizens of the same. The structure of some of the lowest forms of vegetable life was first illustrated, and some points in their physiology described. The first example of the animal kingdom selected was the Amœba or Proteus. In this animal we see a creature devoid of muscular or nervous system, with no head, no stomach, or alimentary canal. Its body consists of a jelly-like substance, of irregular shape, from any part of which finger-like processes are at times protruded. It lives by absorption, and can improvise a stomach from any part of the exterior of its body. The Amœba is one of the very lowest forms of animal life. The lecturer then proceeded to explain the structure and habits of other microscopic animals, a little more complex than the preceding. Among these were the blue Stentor, the bell animalcule (*Vorticella*), the glutton (*Lurco*) Rotifers, or "wheelbearers," Paramœcium, the four-horned Cyclops, and other microscopic animals. He stated that he had frozen water, containing Rotifers, solid, and upon melting the ice the Rotifers were as lively as ever, also that they could endure a considerable degree of heat. A large diagram, with figures of the several plants and animals spoken of, materially helped to illustrate the paper, which will shortly appear *in extenso* in the next number of the *Canadian Naturalist.*—Mr. Billings then made a communication on the bones of a whale lately discovered at Cornwall, Ont., of which the following is an abstract:—"Several months ago Mr. Charles Poole, of Cornwall, wrote to the secretary of the Society that a large skeleton, resembling that of an Ichthyosaurus, had been found in that neighbourhood by the men engaged in excavating clay for brick. In another letter he stated that Mr. T. S. Scott, architect, of this city, had procured the lower jaws. On receipt of this information, Mr. Billings called upon Mr. Scott, who very liberally presented the jaws to the Geological Museum. Mr. Billings then went up to Cornwall and obtained from Mr. Poole the bones which were in his possession. These were discovered in the Post-pliocene clay formation, about sixteen feet below the surface. They are those of a small whale closely allied to the white whale, *Beluga leucas*, which lives in the Northern seas, and at certain seasons abounds in the Gulf and lower part of the St. Lawrence. The lower jaws are nearly perfect. The skull and upper jaws are much damaged, and some of the parts lost. Thirty-five of the vertebræ, the two shoulder blades, most of the ribs, and a number of small bones were collected. The length of the animal was probably about fifteen feet. The lower jaws have the sockets of eight teeth upon the right side, and of seven on the left. The number of teeth in the upper jaw could not be ascertained. In the head of a white whale belonging to the cabinet of M'Gill College, there are nine teeth in the right lower jaw, and eight in the left. The teeth of the fossil, judging from the size of the sockets, were longer than those of the white whale. In 1849, a small whale was discovered in Vermont about twelve miles south of Burlington, in a railway cutting, through a deposit of clay of the same formation as that of Cornwall. Judging from the figures and description published in *Silliman's Journal* by the late Professor Thompson, there can be little doubt that ours is the same species which he described, and which he called *Beluga vermontana*. Another specimen consisting of about half of the backbone, was discovered several years ago near the city of Montreal, and is now in the Museum of the Geological Survey. The locality at Cornwall is about half a mile from the railway station, sixty feet above the St Lawrence, and over two hundred feet above the level of the sea.—The President, in inviting a discussion on the phenomena observed during the recent earthquake, said that there were records published or preserved of the appearances observed during 83 earthquakes in Canada and neighbouring parts of N. America. A severe shock was felt in Canada in 1860, an account of which might be found in the *Canadian Naturalist* for that year. Many of the phenomena noticed in 1870 were observed in the shock of 1860. Judging from the facts on record, there would seem to be a periodicity in earthquakes. They seem to occur much oftener in autumn and winter than in spring or summer, and between the 60th or 70th years of a century. On this ground he had stated that the shock of this year might prove to be the beginning of a series, if the law of periodicity holds good. A slight shock was, however, felt in Canada in the spring of 1864 The president next referring to the causes which produce earthquake, said that here there are no centres of active igneous agencies, as in Southern Italy and elsewhere. He suggested the idea that large masses of sediment are drained off by rivers from this continent and deposited on the Atlantic coast, and when, in addition to this, a pressure amounting to many millions of tons of atmospheric air is removed from the denuded portion, vibrations occur from long-continued tension of the earth's crust, and finally a break takes place. It was found that during the last earthquake the mercury in the barometer was an inch lower than the average. Dr. Smallwood gave a description of peculiar phenomena observed in the heavens before and after the earthquake. Among these were noticed several clusters of spots on the sun's disc in connection with peculiar auroral displays He exhibited diagrams showing the barometrical and thermometrical appearances presented before and during the shock. During the continuance of the vibration the descent of the mercury was most marked in this respect, confirming Dr. Dawson's view. From telegrams received by the courtesy of Mr. Dakers, it would appear that the first shock was observed at Owen Sound, at 10.52 A.M. local time, and the latest at St. John's, N.B., at 11.45 A.M. local time. Accounts were received also from Toronto, Montreal, Quebec, and intermediate places. Judging from the accounts received, the extent of the vibration thus recorded would appear to have been from S.W. to N.E, and the shock to have occupied fifty-three minutes of time in traversing the 840 miles, without calculating for the difference of longitude between the places This would give a rate of about sixteen miles per minute, but if the differences of longitude were calculated the rate would be about thirty-two miles per minute. This last estimate would agree nearly with that given by Humboldt and Mallet. The width or amplitude of the vibration, judging only by telegrams received by the speaker, would appear to have been some 340 miles. After some remarks by Dr. J. B. Edwards and others, the meeting adjourned.

HALIFAX, NOVA SCOTIA

Institute of Natural Science, November 14.—J. M. Jones, F.L S., president, in the chair. Rev. D Honeyman, F.G.S, read a paper, entitled "Record of Observations on the Geology of Nova Scotia from 1855, Part I.;" from which it appeared that Dawson's "Acadian Geology," first edition, was published in 1855, and that in this work the author of the present paper was introduced as an explorer in the Nova Scotian field. His attention had been chiefly directed to the "Silurian (?) and Devonian rocks," so designated in that volume, and to the lower carboniferous conglomerates, grits, sand-stones, argillites, limestones, and gypsums. He had also made gleanings in the coal field. Since that period Dr. Dawson and the author, with the aid and advice of Sir R. Murchison, Hall, Salter, and Barrande, had deprived the Devonian system of its ascendancy in Middle and Eastern Nova Scotia; and Cape Breton; taking away the mark of interrogation from the Silurian and affixing it to the Devonian. This was done chiefly by a thorough investigation of the palæontology and geology of the thoroughly typical district of Arisaig on the south-east coast of Northumberland Strait, in the Gulf of St. Lawrence; and the application of this type to the geology of the province. It was shown by the author that while the fossils of the series of rocks of Arisaig had so great a resemblance to those of the British Silurian, that Mr. Salter could without hesitation designate the respective groups according to the British nomenclature; still, the series could be more satisfactorily and thoroughly compared with the Silurian of the United States; consequently, the Arisaig series were now arranged thus, in ascending order:—Oneida conglomerate, Medina sandstone, Clinton (Middle Silurian), Niagara limestone, Lower Helderberg (Upper Silurian), Oriskany sandstone (?) (Devonian). In this locality these are overlaid unconformably by conglomerate with interstratified traps and limestones (Lower Carboniferous). The Silurian and Devonian (?) have been thrown into a synclinal by greenstone and amygdaloids of Lower Carboniferous age. The Arisaig fossiliferous series are bounded on the north by Northumberland Strait, on the south by mountains formed of another series of the same age, but different from the other in being highly metamorphic and, apparently, non-fossiliferous; the lowest member of the series is a *conglomerate* with cleavage. This series has been elevated by the upheaval of syenite. These conjointly attain to an elevation of from 1,000 to 1,010 feet above the sea level, according to Captain Bayfield's measurement; this being about the highest elevation of mountains in Nova Scotia. While numerous localities named in Dawson's "Acadian Geology," and others discovered by the author since 1855, have fossiliferous rocks of the Arisaig type, the general character of the rocks of the principal ranges of the mountains of Nova Scotia is Arisaig metamorphic non-fossiliferous. The President read a paper "On the Diurnal Lepidoptera of Nova Scotia, Rhopalogera, Part I." The following species were common in the province, *Papilio turnus* Linn.; *Pieris oleracea* Harris, *P. rapæ* Boisd., *Colias philodice* Godt., *Argynnis aphrodite* Fabr, *Argynnis myrina* Cram., *Melitæa tharos* Cram., *Grapta C. argenteum* Kirby, *Vanessa antiopa* Linn., *Pyrameis cardui* Linn., *P. huntera* Smith, *Nymphalis arthemis* Drury, *Erebia nephele* Kirby, *Satyrus alope* Fabr.; while *Danais archippus* Fabr., *Melitæa ismeria* Boisd., *Grapta interrogationis* Godt., *G. comma* Harris, *Vanessa J. album* Boisd., *V. milberti* Godt, *Pyrameis atalanta* Linn., *Nymphalis disippus* Godt., *Debis Portandia* Fabr., were rare. The author dwelt upon the introduction of *Pieris rapæ* into this part of the Canadian dominion within the last few years, and alluded to its abundance last summer in the neighbourhood of Halifax, where it did an immense amount of damage to the cauliflower crops. He mentioned the probable benefit that would arise from the introduction of the house-sparrow of England (*Pyrgita domestica*) that great enemy of caterpillar life, which would amply repay the trouble and expense of importation. At the present time the caterpillars were almost free from molestation, and it was but proper, when possible, on the introduction of an insect pest, to introduce also its known enemy. The author had observed that even in so small a country as Nova Scotia many species of butterflies were very local in distribution, and species quite common on one side of the province were altogether unknown on the other, although the distance between such positions was not more than thirty miles. Several Hesperians were yet unnamed, and these when identified with some Lycænians, would be included in Part II.

WELLINGTON, NEW ZEALAND

Philosophical Society, September 17.—Dr. D Hector exhibited a preparation showing the egg of the large Kiwi (*Apteryx australis*) *in utero*. The bird had recently died in confinement, and was sent to the museum to be skinned. The egg, though full-sized, is soft, and squeezed out of shape by the pressure against the pelvic bones.—"Critical Notes on the Ornithological Portion of the Rev. R. Taylor's recent work on New Zealand" by W. L. Buller, F.L.S.—"On the Ornamental Cultivation of Native Shrubs," by T. H. Potts and William Gray, Canterbury.—"Notes on New Zealand Birds," continued, by T. H. Potts.—"On the Nomenclature of New Zealand Rocks" and "Notice of some New Mineral Forms in the Museum," by E. H. Davis, F L.S.—"On the Absorption of Sulphur by Gold," by W. Skey, Government analyst. The author of this paper, while recently investigating the causes of the reported loss of gold at the Thames gold fields during its extraction from the ore, found the gold is acted on by sulphuretted hydrogen, and thus a sulphide is formed which tarnishes the surface. Also that gold combines with free sulphur at a temperature of 212° Fahr. Gold thus sulphurised on the surface will not amalgamate with mercury. The loss of gold is not altogether due to the condition of the mercury, as has hitherto been supposed, as he has found this sulphide on the surface of native gold of every degree of purity.

VIENNA

I. R. Geological Institution, Sept. 30.—During the summer season the geologists of the Institution were partly occupied with the survey of Northern Tyrol from the environs of Kufstein, along the valley of the Inn westwards as far as Innsbruck, and partly on the military frontier between Barziasch on the Danube, and Brod in Slavonia. Messrs. Foetterle and Fr. v. Hauer, invited for this purpose by the Roumanian Government, investigated also parts of the Wallachian territory, and studied more particularly some sections between Bucharest and Kronstadt in Transylvania. Very interesting results were obtained here. The large portion of Southern Wallachia seems mostly covered with loess; but a boring at Bucharest, for an artesian well, perforated up to the depth of 250 metres beyond the loess nothing but strata of clay and sand, belonging to the Congeria-beds, a freshwater deposit which, in the Vienna basin, forms the highest member of the Miocene formation. Farther up towards the north the same strata constitute a broad zone of mountains of lower elevation, which board the plain, and form a passage to the high mountain which separates Wallachia from Transylvania. Large masses of petroleum are here imbedded in the lower parts of the congeria deposits Whilst, therefore, the Galician petroleum belongs to the Eocene Carpathian sandstones, the Wallachian petroleum is of the upper Miocene age, and the strata which contain it abound in characteristic fossils of our congeria beds. Immediately below these beds follows the salt formation, consisting of salt clay, with large masses of rock salt, which is worked in the mines of Telega, Sylatina, Okna, &c. The higher mountains are formed chiefly of Eocene strata, partly sandstones, partly coarse conglomerates, with enormous boulders of jurassic limestone. These Eocene strata underlie immediately the salt formation in the Bucsecs mountain; they are upheaved to the height of more than 8,000 feet above the level of the sea. The crystalline rocks which form the nucleus of the Fogarasch mountains in Transylvania finish eastward in the neighbourhood of Ruckur, in the Dibmovizza-valley. On the passage between Kronstadt and Sinaja they are no longer to be observed.—Dr. T. Schmidt, director of the Observatory at Athens, gave notice of the violent earthquakes of July 31, August 1 and 5, in Greece, and mentioned that the volcanic eruptions in Santorin, which began five years ago, continue up to the present day with undiminished force.—M. Herbech discovered in Eastern Transylvania, on the Nagy-Hagymas mountain, some red limestone, with the characteristic fossils of the famous upper triassic Hallstatt marble, among them the magnificent Ammonites Metternichii.—Count H. Wilcjek and Count G. Wurmbrand discovered last summer an old pile building (Pfahlbau) in the Atter Lake in Upper Austria, near Kammer. Stone implements, fragments of rough pottery, &c., were found in abundance at the bottom of the lake.

Imperial Academy of Sciences, October 6.—Professor Barth transmitted some communications from the Chemical Laboratory of the University of Innsbruck. These notices included the results of an investigation made by Prof. Barth on the reaction of fusing potash upon phenole, of an investigation of the bromophenolic acids, by M. Carl Tenhofer, and on some derivatives of gallic acid, by Professor Reynold.—Dr. S. von Basch presented a memoir on the first chyle-ducts and the absorption of fat. The results of the observations made at the Central Observatory for meteorology and terrestrial magnetism during the months of July and August were laid before the meeting.

October 13.—M. Julius Peterin communicated a memoir on the formation of electrical annular figures by the current of the influence machine.—Dr. L. J. Fitzinger presented the fifth section of his critical revision of the bat family, relating to th genera *Nycticejus*, *Lasiurus*, *Amblyotus*, *Murina*, *Harpyiocephalus*, *Nycliptenus*, *Æorestes*, and *Natalus*.—Dr. von Zepharovicl of Prague, communicated a notice of the crystals of cerussit lately found in the Galena mines at Kulibaba. His paper contained accurate measurements of the crystals.—Dr. E. Bruck communicated a paper by M. A. Schapringer on the contractio of the *tensor tympani*.—A memoir was read by Professor Lc schmidt, containing an account of experimental investigations o the diffusion of gaseous mixtures, by M. A. Wretschke.—And Di J. Peyritsch communicated some further observations on pelorisn in the Labiatæ.

DIARY

THURSDAY, DECEMBER 8.

ROYAL SOCIETY, at 8 30.—Report on Deep-Sea Researches carried on durin the months July-September, 1870, in H M. Surveying Ship *Porcupin* Dr Carpenter, F R S., and J Gwyn Jeffreys, F R.S

SOCIETY OF ANTIQUARIES, at 8 30.—Autographs of Eminent Italian Princes Dr O'Callaghan, F.S A.—On a Deed appointing Sir John Fastolf Gove nor of the Bastille, 3 Hen V.: Mr. J G. Nichols, F S.A.

LONDON MATHEMATICAL SOCIETY, at 8.—Further Remarks on Quart Surfaces: Prof Cayley.—On the Polar Correlation of two Planes, and i Connection with their Quadric Correspondence. Dr. Hirst, F R S.—O Systems of Tangents to Plane Cubic and Quartic Curves: Mr J. J Walke.—On the Order and Singularities of the Parallel of an Algebraical Curve Mr S. Roberts

LONDON INSTITUTION, at 7 30.—On Count Rumford and his Philosophic Work. Mr. W. Mattieu Williams

FRIDAY, DECEMBER 9.

LIVERPOOL NATURALISTS' FIELD CLUB.—Lecture by Prof W. C. Willian son, F R S

SUNDAY, DECEMBER 11

SUNDAY LECTURE SOCIETY, at 3 30.—On the Telescope and its Discoveries Mr. R. A. Proctor.

MONDAY, DECEMBER 12

LONDON INSTITUTION, at 4.—On Chemical Action (Educational Course) Professor Odling, F.R S.

TUESDAY, DECEMBER [illegible]

ROYAL GEOGRAPHICAL SOCIETY, at 8.30

ETHNOLOGICAL SOCIETY, at 8.—On Stone Implements from Africa: S John Lubbock, Bart, M P.—On Stone Implements from the Cape of Goo Hope: Mr. Edgar Layard—Second Report on the Prehistoric Mon ments of Dartmoor: Mr C Spence Bate.

PHOTOGRAPHIC SOCIETY, at 8.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY, at 7.

WEDNESDAY, DECEMBER 14

SOCIETY OF ARTS, at 8.—On a new Method of producing Durable Mur Paintings by Fictile Vitrification: Mr Alan S. Cole

ROYAL MICROSCOPICAL SOCIETY, at 8.—On the Anatomy of *Ascarus lumbricoides*: Mr B T. Lowne, M R C.S.—Observations on the Aëroscope or Air-dust Collector. Dr Maddox.

THURSDAY, DECEMBER 15

ROYAL, at 8 30

CHEMICAL SOCIETY, at 8.—On some New Derivatives of Coumarin Mr. W H Perkin

LINNEAN SOCIETY, at 8.

LONDON INSTITUTION, at 7 30.—On Count Rumford and his Philosophic Work: Mr. W Mattieu Williams.

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ERRATA.—Page 76, first column, line 29 from bottom, for "high" rea "low"; second column, line 8, insert after "winter," "—there atmospher pressure is low;" page 92, second column, sixth note, for "Mr. Mathe Williams" read "Mr William Mathews," and for "Mr Williams" rea "Mr Mathews."

THURSDAY, DECEMBER 15, 1870

PRACTICAL PHYSICS

THE Vicissitudes of Families of Words, and especially of scientific nomenclature, would require another Burke to write their changeful history. Take, for instance, the word *Philosophy*,—how odd its present distorted meaning as compared with its literal sense, and how curious its alliance with such terms as Natural, Experimental, Mechanical, Chemical, and the like. Then, again, take *Science*,—how strange its present opposition to *Learning*, and how remarkable the adoption of the word *History* in conjunction with *Natural!* Most surprising of all, however, is perhaps the opposition set up between the words *natural* and *physical*; which has gone to such length that Prof. Huxley, in his recent Address to the British Association, could properly and intelligibly employ such a phrase as "those phenomena of nature which we call physical."

In French, the equivalent term for Natural Philosophy, "philosophie naturelle," is still sometimes used, and in a sense, if not coincident with, yet kindred to, that of its English representative; in German, however, the similar term, "Natur-Philosophie," has assumed a totally different meaning, and the word "Philosophie" by itself is, if possible, still farther from the English *philosophy*. For the latter word, in its English meaning, there is no equivalent whatever in German; while the occurrence of such expressions in English as Philosophical Instruments, co-existent with Moral Philosophy, strikes the German ear and intellect as insular eccentricity. But the German terminology is also in this respect not free from oddities. Thus, while "Experimentalische Physik" and "Theoretische Physik" (or sometimes "Mathematische Physik") cover nearly, although not precisely, the ground occupied by Experimental Philosophy and Natural Philosophy (in the orthodox sense) respectively; the word "physikalisch" has assumed a meaning opposed not to Moral but to Chemical, and a distinction has grown up between "physisch" and "physikalisch," corresponding to that between the English terms Natural and Physical.

To the terms just mentioned, Prof. Kohlrausch has now added a new one, on the title-page of a recently published little work, entitled, "Praktische Physik."* He thereby designates a series of practical exercises designed originally for the students who frequent the so-called "Physikalische Prakticum" in the University of Gottingen, for the purpose of being initiated into the use of physical instruments and the execution of physical operations. A work of this nature has long been a desideratum; although it has had a kind of forerunner in Prof. Frick's well-known "Physikalische Technik," *i.e.* Technical (or Operative) Physics, and to be distinguished from "Technische Physik," which means Physical Technology, or the application of Physics to manufactures and arts. Prof. Frick's work, however, of which three editions have appeared, was intended rather as an instruction in the making of lecture experiments and in the handling of the required apparatus, replacing in so far the older works of Abbé Nollet* and Sigaud de la Fond;† while the present work of Prof. Kohlrausch is designed as an initiation into original experimental measurements and researches. It has long appeared to the present writer as a kind of double drawback, inherent to current lectures as well as text-books on philosophy, that while, in all instances, more time or space is devoted to the description of apparatus and practical processes than is necessary or useful for the common student, they are yet, in this very respect, insufficient for the intending physicist. Taking in hand the best kind of treatises on Physics, whether of an entirely elementary or a more ambitious character, it will be found that a preposterous amount of space is taken up by drawings of instruments which the general student will never have to handle in his life, and by explanations of the manner in which certain procedures, measurements, and so forth have been taken, which it is practically equally useless for him to know, and which as a means of educating the mind have no value, while they tend to make science repulsive. On the other hand, the most voluminous works cannot but be pronounced, in this very respect, as deficient, if the wants of the young physicist are taken into account. Special works, like those of Profs. Frick and Kohlrausch, go some way towards supplying this want; and it is to be hoped, also, that, by multiplying or extending their scope, they will prove not only of additional benefit to intending physicists, but also to ordinary students, by ridding the vulgar treatises, and eventually lecture courses, of much superfluous matter that acts as a serious incumbrance and impediment to the spread of *real* physical science.

Prof. Kohlrausch's little book, of scarcely more than one hundred pages, reproduces, or very nearly so, the practical *curriculum* familiar to those who, in former time, attended at Gottingen the exercises which Profs. Weber and Listing superintended there for many years. The subjects selected range over a considerable field, and include a variety of problems; but the work was not intended to be exhaustive. Common weighing, specific weights and densities, thermometry, magnetism and galvanism, and optical instruments, furnish the chief topics on which exercises are indicated. Of course, the work is not designed to be read by itself, but to serve as a manual of instruction in the practical execution of the several processes. Nor should it be thought that even practical work of the kind here indicated can serve as a training for future discoveries, any more than early verse exercises make any one a poet; it completes, and familiarises with, the knowledge of discovered truths, but does not teach the discovery of truth. Original research of high value can be made as little on the pattern or with shreds of old, as genuine poetry can be composed in imitation and with patches borrowed from the ancients. Scientific investigation is a work of inspiration, and if directed towards a new aim, requires also novel instruments and new procedures. Chemical operations proper possess, it is true, a considerable degree of uniformity, and are capable of methodical treatment and exposition; but physical processes and manipulations are multiform, numerous, and difficult to classify. This is the reason why physical laboratories are as yet few and far between, and none of them so systematically organised as the chemical laboratories; and that, while the workers in

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* *Leitfaden der Praktischen Physik.* Von F. Kohlrausch. (Leipzig, 1870.)

* L'Art des Expériences.

† Cabinet de Physique.

Chemistry surpass in numbers, they also outdo in individual productiveness, the workers in Physics. But since the institution of physical laboratories is nevertheless spreading, the very novelty of their existence makes the publication of books like that of Prof. Kohlrausch, proceeding from older establishments, a real boon to those called upon to superintend or take a share in the direction of the practical work of new ones; and it is only to be hoped that from other places of renown also, both in England and abroad, similar publications may also soon proceed.

C. K. Akin

GALLOWAY'S QUALITATIVE ANALYSIS

Manual of Qualitative Analysis. By Robert Galloway, F.C.S. Fifth edition, xxi. and 415 pp. (London: John Churchill and Sons.)

OF the various manuals of Qualitative Analysis now in use, none is more deservedly popular than the work of Professor Galloway. Much of this success is doubtless due to the painstaking care with which the author has sought in successive editions to reduce the operations of Qualitative Analysis to a more methodical and systematic process. The present edition is in great part rewritten, and much new and original matter has been incorporated. More systematic methods for separating the alkaloids are given, together with many additional reactions for their individual discrimination. The processes for detecting the poisonous metals in presence of organic matters are also much improved. A description of Bunsen's neat and expeditious flame reactions is likewise a new feature in the book. The delicacy and certainty of these reactions ought undoubtedly to lead to their more general adoption in our laboratories; even if their application is found to be limited, the lesson in neatness and dexterity in working to be acquired in their performance would prove invaluable to the student.

One of the characteristics of this book is its thoroughness, and the very example of this quality will not be lost upon the beginner. As an illustration of what we mean, we give the following extract from the introductory remarks on the province and scope of qualitative analysis:

> The analyst, by means of re-agents, interrogates the substance to be analysed as to what are its component parts; the reactions are the language in which the answer is returned. The student has therefore to learn the mode of questioning the substance, and the language in which the answer will be conveyed; in other words, he has to learn, not only what general and special re-agents are to be employed, but the order in which they are to be applied, and also the reactions they produce with the bases and acid-radicals, before he can attempt to search for these bodies in substances. No amount of *reading or lecture-hearing* will furnish the student with this knowledge; he can only obtain it by making the experiments himself of the different bases and acid-radicals with the re-agents, and "*he must always reflect, before the addition of the re-agent, for what purpose he applies it, and what are the phenomena he intends to produce.*" And the conditions indispensable for the production of correct and decisive reactions must be carefully observed, for a half-knowledge in all departments of science is of little worth, but in chemical analysis it is worse than useless.

There must always be diversity of opinion respecting the best method of teaching Qualitative Analysis, or indeed of teaching any branch of practical science; since so much depends upon the qualifications and conscientiousness of the teacher. In the Preface to the present edition of his book, Professor Galloway makes some very pertinent observations respecting the advantages which the method he adopts possesses over that employed by Fresenius and in the Giessen Outlines. It is quite possible that by faithfully following the plan laid down by the German professor, the student may succeed in correctly determining a larger proportion of the mixtures given to him for analysis, and yet the amount of actual benefit which may accrue to him may be far less than if he were more frequently unsuccessful by working under a system which left more to his individual judgment and intelligence. We believe that the method of Fresenius, as an educational agent, is radically bad; its tendency is to make the learner degenerate into a mere analytical machine. Such a system (to employ the phraseology of Mr. Galloway), of simply *telling* this and *showing* that, may be most pernicious in its consequences. The chief object in teaching chemistry is thus too frequently missed; science so studied renders the student utterly incapable of correctly reasoning upon the knowledge he acquires, for merely to create a number of proficient analysts is not the primary end of the introduction of qualitative analysis into the *curriculum* of our schools and colleges. A perfect system, then, is that which, whilst employing the most satisfactory and expeditious analytical methods, yet allows sufficient latitude for the student to exercise and strengthen the powers of his originality, reason, and intelligence. Such a system Professor Galloway has attempted to frame, and we have no hesitation in asserting that in the hands of a conscientious student his book will lead to the result which he desires to obtain.

T. E. Thorpe

WORKS IN NATURAL HISTORY

Works in Natural History, &c. By the Rev. F. O. Morris, B.A.

THIS is, in several respects, a very remarkable pamphlet. It shows us, to our great gratification, that the study of Natural History is, thanks to the labours of Mr. Morris, gradually finding its way into Royal Palaces and Baronial Halls, for we learn that Her Most Gracious Majesty the Queen has accepted the dedication of his "History of British Birds;" that his Excellency the Right Honourable the Earl of Carlisle, K.G., &c. &c., influenced probably by Her Majesty's example, has extended his patronage in a similar manner to the "Natural History of the Nests and Eggs of British Birds;" that his "British Butterflies" and "British Moths" are under the genial and aristocratic guardianship of the Honourable Mrs. Musgrave and the Right Hon. Lady Muncaster; that his "Anecdotes in Natural History," and "Records of Animal Sagacity and Character," are dedicated by permission to the Most Hon. the Marquis of Westminster, K.G. and the Hon. Anne Emma Cavendish; while His Grace the Archbishop of York stands sponsor to "The Difficulties of Darwinism." Surely even in the so-called Golden Age of English literature, no author could have had the good fortune to secure so many noble patrons.

Appended to the title of each of Mr. Morris's works are "Notices of the Press,' and in this department the compiler of the pamphlet would, we think, have acted more wisely and more in accordance with the dignity of

Science, if he had restricted himself to journals, reviews, &c., of acknowledged literary merit, or treating specially of Natural History. Who, with a grain of common sense, cares to know, or attaches a shade of value to, the scientific opinions of such periodicals as the *Doncaster Gazette*, the *York Herald*, the *Derbyshire Advertiser*, the *Yorkshire Gazette*, the *Worcester Journal*, the *Bromsgrove Messenger*, the *Yorkshire Post*, *Our Own Fireside*, the *Oriental Budget*, the *Threepenny Magazine*, the *Penny Post*, the *Rock*, or the *Record?* This kind of puffing (for we can find no other term to express our meaning plainly) is bad enough; but there is worse, far worse, to come. It surely cannot have been with the knowledge and concurrence of "The Rev. F. O. Morris, B.A., Rector of Nunburnholme, in this county, and Chaplain to his Grace the Duke of Cleveland," as he is designated in the *York Herald*, that a series of anonymous opinions on his "Difficulties of Darwinism"—many of them abounding in the most gross and fulsome flattery of himself, and in vulgar abuse of his opponents—have been appended to the more legitimate notices of that book. Lest we should be supposed to be using unnecessarily strong language, we shall quote a few of these precious criticisms:—

> "Professor Huxley's letter to you is in his usual style—flippant and rude. Your reply is in every way admirable.—Professor ——."
>
> "I have read your pamphlet, and do not see how it can be answered.— ——Esq. M.D., A.B."
>
> "Prof. Huxley's impertinent letter *deserved* what it has *got*. You certainly have given him a castigation.—Rev. ——, Cantab."
>
> "The papers here steer shy of your pamphlet. They are on the other side, and find it awkward to reply to. I am very glad to hear that you purpose meeting Huxley at Liverpool. He requires a man that can expose his shuffles, and turn his banter against himself.—Rev. ——, D.D."
>
> "Glad to see you an opposer to Darwin's nonsense.— ——, Esq."

The last of these absurdities is apparently a round-robin printed in a straight line: "We agree with all you say most perfectly. We cannot imagine how it can be answered by the Darwinites." Then follow three dashes.

Mr. Morris is not merely a well-educated English gentleman, but a clergyman and chaplain to a duke; and we feel that we are doing him a service in giving him an opportunity of stating (1) whether these extracts are really taken from letters addressed to himself, or whether they are forgeries; and, (2) in the former case, of explaining how they found their way into print in this obnoxious form.

A geological friend of ours, while trying to make a short cut, trespassed on a railway line. He had not gone fa[illegible]e he was stopped, and told by a grim official to retrace his steps. Our friend, thinking that a little "soft sawder" might help him out of the difficulty, observed: "My good fellow, you are perfectly right, and are only doing your duty; but I am much mistaken, when I look at your kindly and good-natured face, if you are the sort of man to turn a harmless geologist a couple of miles out of his route." The heart of the guardian of the road was so far softened by this speech as to let our friend proceed rejoicing, but, as a parting shot, he observed:—"Well, sir, I *do* like a bit of butter, but I ain't partial to grease." Now, to apply this anecdote, we sadly fear that Mr. Morris is "partial to grease." Many clergymen indulge in this taste, and one who has associated so much as this distinguished naturalist has done with the *Dii Majores* is apt to grow unctuous. On the assumption that these extracts are genuine, we can fancy that he carried the parcel of letters in his coat-tail pocket when visiting his parishioners, and occasionally sat down to enjoy a bit of grease, or, in other words, to read one or two extracts, as a cheerful mental stimulant; that probably on one occasion, the stimulant was too much for him, and that he inadvertently left the packet "*sub tegmine fagi;*" and that an enemy (probably a rabid Dissenting minister of a low class) picked up the prize, at once saw its value as a weapon against the Church of England, and gave it up to the Elders of his congregation, on the condition that they should publish it. It was then probably placed in the hands of a literary gentleman—possibly the Editor of the *Threepenny Magazine* or the *Penny Post*—and thus, and thus only, the appearance of the pamphlet can, we think, be rationally accounted for.

OUR BOOK SHELF

Resources of the Southern Fields and Forests, Medical Economical, and Agricultural. By Francis Peyre Porcher, M.D. (Charleston: 1869.)

In this book we have very full accounts of the uses of the plants of the Southern States of America. The author freely acknowledges in his Preface (or, as it is here termed, "Preliminary,") that he has availed himself largely of numerous works on kindred subjects, most of which have been published in America. In the introduction a few practical instructions are given for collecting and drying medicinal products of the vegetable kingdom. The plants are arranged according to their natural orders, the vernacular names being placed first, followed by the scientific names, and the distribution of the plants in the States. No attempt is made either at a scientific or popular description of the plants themselves; so that the book is literally what it professes to be, without being made bulky with matter that can, if required, be found in floras or purely botanical works. The book, indeed, is written more with an eye to the exposition of the medicinal uses of the plants; but the "economical and agricultural" portion is by no means lightly treated. In short, the information is most varied, as will be seen from a few quotations. Here is a recipe for making blacking from elder-berries, certainly an application we never before heard of:—"Boil elder-berries well, mashing the pulpy matter; then strain through a colander, and bottle for use. The liquid sours somewhat by age, but retains its qualities. Another way is to simmer ripe elder-berries over a slow fire in an iron kettle for one hour and let the mass cool, and you will have good blacking." Under the head of White Beech (*Fagus sylvatica*, and *F. americana*) our author tells us "the leaves of the beech trees, collected in autumn in dry weather, form an admirable article for filling beds. The smell is grateful and wholesome, they do not harbour vermin, are very elastic, and may be replenished annually without cost." There is nothing new in this application of beech leaves; they are used in many parts of Europe for a similar purpose, and were at one time so employed in England. Evelyn speaks of them as affording "the best and easiest mattress in the world to lay under our quilts instead of straw;" and by way of recommendation says that "divers persons of quality in Dauphiny" use them. The above are examples of what may be called purely economical applications. We take a cruciferous plant, the Gold of Pleasure, or False Flax

(*Camelina sativa*) as a single example of agricultural produce. "The cultivation of this plant for the seed would repay the farmer; an abundance of chaff would be produced which would be of infinite service for horses or for manure. In a grazing country like England, where vast sums are annually expended for foreign oil-cake, the Gold of Pleasure will soon be found an excellent substitute under manufacture, and, consequently, a grower should find a good remuneration in cultivating the seed. The oil-cake has been found highly nutritious in the fattening of sheep and oxen, as it contains a great portion of mucilage and nitrogenous matter, which combined are found very beneficial in developing fat and lean." The prospects of making this a most important agricultural plant are, we think, too brightly drawn, considering that it has not escaped the notice of English agriculturists. Neverthless, a few words of this kind on different products might help to promote experiments on their culture and utility. The writer's aim throughout seems to be a general utilisation of vegetable productions, and he very ingeniously finds a variety of application for those of the Southern States. We do not hesitate to say that a few books of this description on the economic products of different parts of the globe, would make us much better acquainted with the true value of the vegetable kingdom than we are at present.

JOHN R. JACKSON

Adventures of a Young Naturalist. By Lucien Biart. Edited and adapted by Parker Gillmore. (London: S. Low, Son, and Marston, 1870.)

THIS is a narrative of travel in Mexico, intended especially for young people interested in Natural History. The party consists of a young lad the hero, his father, a Swiss naturalist who does all the moralising, a dog, and one of those half-bred Indians who know everything and can do everything, who are such a bore in most books of Western travel. Though written in a somewhat pedantic style, we have no doubt it will find many admirers among our adventure-loving young readers, the country described being one of unsurpassed beauty and interest. For our own part, we should decidedly object to being cross-examined in the following manner before being allowed to eat our breakfast. "Do you know the family of the animal we are going to have for breakfast?" asked Sumichrast. "Yes; it is a Rodent." "Well done; but how did you recognise it to be so?" "By the absence of canine teeth in its jaws, its large incisors, and its hind-legs being longer than its fore-legs." Especially if the lesson were given in such a confused style as this:—'The bird belongs to the family of Climbers, that is to say, to that order which have two toes in front of their claws and two behind, like your great friends the parrots." Still the young naturalist will find in the book much that is interesting and amusing; and the numerous illustrations and gorgeous binding will make it an acceptable present during the Christmas season.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Contribution to the Dioptrics of Vision

IN the course of some experiments in reference to vision under water, I have ascertained some facts which I do not remember to have seen mentioned by writers on optics, and which may perhaps interest your readers.

Every swimmer knows that, however clear the water may be, and however distinctly he may see from the bank the smallest particle of gravel or weed, the moment he plunges beneath the water all becomes obscure, and he can see the outline of nothing at the bottom or suspended in the water distinctly, but only blurred patches of various colours. In my first endeavours to find a remedy for this imperfect vision, I found two ways of restoring perfect sight. The one was to surround the eye with a watertight box, with a piece of plain glass in front. By this means, the eye being in the same condition as to receiving the rays of light through an aerial medium as when we are on land, perfect vision is retained beneath the water. The other was, allowing the eye to remain exposed to the water, to look through a glass lens whose proper focal distance in the air, I found, after numerous trials, to be half an inch. The first method is attended by the disadvantages that the glass soon becomes dim from the condensation of vapour, and it is difficult to make it fit so accurately as to exclude the water; the second is more convenient, as any optician can construct a pair of spectacles suitable for the water, and fitted with lenses of the required focal distance.

Fishes, cetaceous animals, and seals, see perfectly below the water, while man's vision, unassisted, is of the most imperfect character. The eyes of these marine animals differ from those of terrestrial vertebrates chiefly in this: the latter have a very convex cornea, with a large chamber containing aqueous humour and a double convex lens behind; whereas the former have a flat cornea, hardly any aqueous humour, and a spherical lens, lying, at least in fishes, close behind the transparent membrane which is their substitute for a cornea.

Now, as an optical instrument, the eye of terrestrial vertebrates—and let us take that of man for an illustration—consists of two lenses, one placed behind the other. The anterior lens is formed by the aqueous humour, its actual figure being a meniscus, one surface being convex the other concave, but both surfaces uniting if prolonged. According to Donders, the anterior radius of curvature, formed by the cornea, is 8 mm., the posterior, formed by the front of the crystalline, being 10 mm. The posterior lens is the crystalline, a double convex lens, its posterior surface, according to the same authority, having a radius of 6 mm. only. The combination of meniscus and double convex lens is known to possess peculiar optical advantages. The vitreous humour cannot act as a convex lens, its form being that of the concavo-convex lens, whose property is to cause divergence of rays of light; but, as it lies in contact with the retina, it cannot even produce this effect. It acts, together with the aqueous humour, as a watery medium for the suspension of the crystalline.

What happens when the human eye is immersed in water? A transparent lens-shaped body will refract the light in converging rays, if it is much denser than the surrounding medium through which the rays of light reach it. A simple experiment will prove this. Take two watch-glasses with their concavities facing one another; fill the space between them with water; this will form in air, than which it is so much denser, a lens of power proportioned to the convexity, but in water it will not refract the light at all, being of the same density as the light-conducting medium. The aqueous humour of the eye being much denser than the air, acts as a lens in the atmosphere, but being of the same density as water, when the light is transmitted to it through water in contact with the eye, we at once lose the use of our anterior lens, and can see nothing distinctly; because the crystalline, which alone now acts as a lens, throws its focus, as we shall presently see, beyond the retina.

How, then, are we to recover perfect vision under water? Obviously, by supplying the loss of our anterior lens by another lens of equal power. The focal distance in the air of a water lens of the meniscus shape and the dimensions given above may be calculated; it is, in fact, two inches or thereabouts; but, as we have seen, it = 0 in water. But, as the refractive power of a lens diminishes in proportion as that of the medium through which it receives rays of light increases, we find that a glass lens when immersed in water has only one-fourth of the refractive power it possesses in air. So, in order to supply the loss of our anterior lens, we find we must use a glass lens of about half an inch focus, which, in water, has a focus of about two inches. I need scarcely say, that in the case of a double convex lens of dissimilar curves it makes a great difference as regards the refractive power whether the lens be wholly immersed in water or one or other of the convex surfaces only. But I need not dwell on this subject at present.

But it is a clumsy method to supply the loss of a lens of two inches focus by one of the high refracting power of half an inch. Besides, a glass lens of this power is so small that the lateral field of vision is of necessity very limited, and it has a further disadvantage that we can see nothing with it in the air. I therefore sought for a lens that should be free from these defects.

As the ocular lens whose place had to be supplied is formed of water, and receives in the normal condition the rays of light through air, I thought I might make my subaqueous lens of the same media. A couple of watch-glasses, placed with their concavities towards one another, so as to enclose a convex lenticular portion of air, when immersed in water, disperse the rays of light and diminish the size of objects seen through them, because they force the more refractive medium, the water, to assume a *concave* shape in relation to the air between the glasses. The same watch-glasses placed with their convex surfaces towards one-another, and connected round their edges by a water-tight rim, thus enclosing a concave lenticular portion of air, when immersed in water, refract the rays of light convergently to a focus and magnify objects, because they force the more refractive medium to assume a *convex* shape in relation to the air between the glasses. Their magnifying power or focal distance under water is somewhat less than that of the same glasses in the reversed position and filled with water is in air; the slight difference being owing to the greater refractive power of the glass in air than in water. I found that two glasses of a curvature of about 1¼ inch radius thus placed formed in water a lens having a focus of about two inches. This *air-lens*, as it may be called, completely supplies the loss of our anterior lens in water, and restores perfect vision. Of course the same magnifying power may be obtained by various combinations of differently curved glasses, or by plano-concave or concavo-convex air-lenses. The advantages of this kind of lens for subaqueous vision over a glass lens are obvious. It can be made of any required size so as to command a large lateral field of vision. It ceases to act as a lens the instant it emerges from the water, and does not interfere with vision in the air, as then we merely look through two thin pieces of glass with some air between them. There is no provoking loss of refractive power, as in the case of the glass lens; and lastly, it can be made very cheaply. With either form of lens we can see from below the water objects in the air above us quite distinctly if the surface of the water is smooth, less distinctly if it is agitated.

Air lenses constructed on the principle described may be made of any magnifying power, and are much better adapted for the microscopic examination of objects under water than glass lenses, whose refracting and magnifying power is reduced to one-fourth by immersion. Thus a glass lens of a quarter-inch focus in air, would scarcely be equal in water to an air lens of one inch focus.

I have said that the difference between the refractive power of a glass lens in air and water is as 4 to 1, or even more. The difference is about the same in the case of the crystalline. Thus, the spherical lens of a cod, which has a focus of about $\frac{3}{20}$ of an inch in air, has a focus of about $\frac{3}{5}$ of an inch in water, which is about the distance of its posterior surface from the retina in the fish. Supposing the focal distance of the human crystalline to be, in air, $\frac{1}{6}$ of an inch, it will be more than $\frac{2}{3}$ of an inch in the fluid in which it floats. But, in front of it, we find what I have called the anterior lens—I mean the aqueous humour—with a focus, as I have proved, of about 2 inches. If we take two lenses respectively of 2 inches and $\frac{2}{3}$ of an inch focus, and place the weaker over the stronger, we shall find their united focal distance to be about ½ an inch, or about the distance between the back of the human crystalline and the retina. My measurements, in the absence of appropriate instruments, lay no claim to exactness; they are, however, a sufficient approximation to truth for my present purpose.

How is it that after the operation of extraction of the crystalline lens, which has a focus of less than 1 inch in its natural position, the patient can see distinctly with a lens of from 3 to 4 inches focus? The reason seems to be that the optical character of the eye is completely altered by the operation. The space formerly occupied by the crystalline is now filled with aqueous and vitreous humour, and the eye represents a sphere of water, bulging in front into a more convex form by means of the cornea, which will have the effect of a superimposed meniscus of about 2 or 2½ inches focus. A thin glass sphere filled with water of 1 inch diameter will roughly represent the eye deprived of its crystalline. We find the focus of this sphere to be about ½ an inch. Let us place in front of it a lens of 2½ inches focus to represent the bulging cornea, and we find the focal distance diminished by more than one-half. Another lens of 3 to 4 inches focus will bring this focus close to the posterior surface of the sphere, in fact, to the situation of the retina in the actual eye. This explains what happens in the eye deprived of its crystalline. Such an eye will require a much more powerful lens for subaqueous vision than that above described.

Montagu Square R. E. DUDGEON, M.D.

Dr. Lankester and the Scarlet Fever Epidemic

PERMIT me to make a few remarks on some notices of my paper on scarlet fever, published in your pages on the 17th of November last. Referring to my recommendation as to the destruction of the poison of scarlet fever, the *Pall Mall Gazette* says—"All this is very well in its way, and may be carried out by the upper and middle classes, among whom the mortality from scarlet fever is comparatively small; but the plan is quite out of the reach of poor creatures who have but one room, one bed, and one suit of clothing, which even at night takes the place of blankets." My object in writing the paper was to show that scarlet fever might be averted by certain measures, and I left it to those who read it to devise the means of making them available for all. When the cattle disease broke out, an Act of Parliament was passed for the purpose of diverting it. The lives of human beings are surely of not less value even in a money point of view than those of cows and oxen, and I have the conviction that certain measures might be adopted by the Government that would reach even the "poor creatures" to whom the *Pall Mall Gazette* alludes. Even now there exist Acts of Parliament which, if at once put in force by boards of guardians, town councils, vestries, and other local authorities, would at once enable them to put down this disease. The inhabitants in "one room, one bed, and one suit of clothing," are reached by medical men, and they might be empowered to remove the sick from the healthy, to destroy useless infected clothing, to have the infected linen washed, and generally to see that the disease is arrested. What can be done amongst the rich ought to be done amongst the poor, and expense ought not to be allowed to stand in the way of such merciful measures. It should be remembered that such outlay on the part of wealthy ratepayers would, in the end, repay them, as they catch this disease from its being fomented among the poor, and they would no longer be liable to these attacks when their less opulent neighbours were free from them.

In the pages of the *Lancet* "A General Practitioner" states that I have reflected on the members of the medical profession in stating that they do not exert themselves to suppress this disease. I spoke from a rather extensive experience on this subject, and regret to say that I have nothing to withdraw on this point. I did not say it was the fault of medical men. I said they were not instructed This is the fault of a system of medical education in which public health is not contemplated as a part of its course It is true that within the last two or three years chairs of Hygiene have been established at University and King's Colleges, London, but these are exceptional. So deficient is the education of medical men on this very point, that the Government felt itself justified in opening a special medical school at Netley for the purpose of supplementing the defects of our ordinary medical schools. It is from the Chair of Hygiene at the Military Hospital that the most admirable work on Hygiene in our language, by Dr. Parks, has issued.

"A General Practitioner" could not suppose that I was ignorant of the fact that all that has been done for our knowledge of the nature of contagious diseases had been done by medical men, and that our medical officers of health have especially exerted themselves in endeavouring to prevent the spread of contagion. I must, however, again express my surprise at the small amount of information that can be gained from the text-books on the practice of medicine as to how best to prevent the spread of contagious diseases.

I will not in your pages do more than allude to the offensive tone and expressions of "A General Practitioner," but I may add that no amount of "support" I may have had or may expect to derive from members of my profession, will ever induce me to refrain from speaking the truth of them in the interests of the public. I am, however, fully convinced that it is only by such a course that I can hope to retain the respect and continued "support" of the more intelligent and honourable members of my profession. EDWIN LANKESTER

Professor Tait on Bain's Logic

IN your last week's number, Prof. Tait publishes a portion of his Introductory Lecture to his class, in which he criticises certain passages in my work on Logic, having reference to the doctrine of the Conservation of Force. Although I do not, in every instance, admit the justice of the strong condemnatory phrases used in the criticism, I am aware of having committed a mistake in stating the relation of Momentum to Energy or *vis viva*, and will endeavour to rectify it.

In quoting a dictum of the late Dr. Boole, Prof. Tait styles him the greatest logician the world has produced, or is likely to produce for many a long day. In dissenting from this superlative, I do not refer to the men most widely known in recent years as logicians—Whately, Hamilton, Mill, Mansel; I consider that the comparison of them with Boole fails through the dissimilarity of the matters compared; his Logic was but to a very small extent the Logic of any one of these writers. The only person who cultivated Logic in the manner of Boole was another noted mathematician, the veteran De Morgan. Now, without undertaking to say which of these two had the greater genius, I do not scruple to affirm that the labours of De Morgan, in their common department, if only through longer continuance in time, resulted in a much larger number of contributions to the science than can be credited to Boole. The two men were friendly co-operators, not rivals; and they will, I have no doubt, be mentioned together as often as reference is made to the Algebraic extensions of Formal Logic.

Aberdeen, Dec. 9 A. BAIN

The Spectrum of the Aurora

THE brilliant displays of the Aurora Borealis observed in England on the 24th of September and the 14th of October, 1870,* were also generally observed in this country. The fact may be worthy of record in your journal, as indicating the unusual extent of the phenomena. As the newspapers in different parts of the United States contained full descriptions of these displays, a detailed account need not here be given. I may remark, however, that they were generally regarded as the most brilliant displays we have had since 1859. Fine auroras were again witnessed on Monday morning, October 24th, from 5 to 6.30 A.M., and on the evening of the same day, from 6 to 11 P.M. At 9 P.M. (on the evening of the 24th) an auroral arch passed very nearly through the zenith from the eastern to the western horizon, or rather from a point a few degrees south of east, to another somewhat north of west. An extraordinary number of more feeble auroras have been noticed during the last two months.

DANIEL KIRKWOOD

Bloomington, Indiana, Nov. 9

Can Aurora be Seen in Daylight?

I VENTURE to believe *not*, in spite of circumstantial accounts to the contrary, and I ground my belief on the following considerations:—1. No description of a daylight aurora that I have ever seen will bear a critical examination. Take that published in the last number of NATURE. Here two arcs of faint white lines are said to have been seen in a direction "almost due east," and certainly the illustration given is not very unlike the appearance that auroral arcs sometimes present. But auroral arcs, so far as I know, never appear in the east, and the conclusion, therefore, is unavoidable that the object observed was nothing more than a remarkably symmetrical form of cirrus cloud. In another instance, lately published, although the thing described is called a daylight aurora, I fail to see in the description anything more than an account of the appearances presented when a high canopy of cloud clears off bodily from the sky with a sharp, straight edge, which by perspective becomes an arch. In the case referred to, the clouds clearing off from the direction of magnetic north, the arch corresponded in position with that of an aurora, and hence was set down as auroral. In a third account of a daylight aurora, it is expressly mentioned that the sky was hazy, and a solar halo visible, a condition of things which, while it would make the occurrence of aurora-like cirrus extremely probable, would be specially unfavourable to the visibility of a true aurora; for certainly if so delicate and phosphorescent a light as that of an aurora is to be seen at all in the daytime, it can only be under circumstances the most favourable as regards clearness of the lower atmosphere.

2. A comparison of the auroral light with the light of other objects whose visibility can be more easily measured, tends strongly to confirm the view I have advanced. No one who remembers Donati's comet at its brightest will hesitate to allow that for intrinsic brilliancy that object surpassed the most vivid aurora. Yet Donati's comet at its brightest could not be detected with the naked eye until about half an hour after sunset, and then only the head could be seen.

3. The modifications of cirrus cloud are so infinitely diversified, and sometimes so very remarkable, as to offer a great temptation to the observer to invest them with the mysterious attributes of the aurora. Moreover, they do occasionally present a very striking resemblance to pencils of auroral light, differing, however, essentially in the character of fixity which they possess, as well as in the absence of any determinate relation to the magnetic pole or zenith.

On the grounds now stated I venture to refer daylight auroras in general to the large class of "errors of observation."

Clifton, Dec. 13 GEORGE F. BURDER, M.D.

The North London Naturalists' Club

THE Secretary of the North London Naturalists' Club desires the Editor of NATURE to correct an incorrect statement which appeared in the last number of that journal. The North London Naturalists' Club is not broken up, it is not six years old, nor has it ever met on a Monday. Its last meeting was on Thursday, Nov. 24, at Myddelton Hall. Three subjects were then exhibited and explained, viz. "The Structure and Growth of the Yeast Plant," "The Structure of the Gastric Teeth in the Lobster," and "The Anatomy of Amphioxus." The meeting was thinly attended, but that is no alarming phenomenon for societies of this kind. The previous meeting on Oct. 27 was a very full one, owing to a paper read by a deservedly well-known member of the club on the highly interesting subject of "Spontaneous Generation." It must be confessed that the club is not so vigorous as when first started, but these facts show that it is by no means defunct.

J. SLADE,
Hon. Sec. N.L.N.C.

Browning's Spectroscope

IN the last number of NATURE there is a description by Prof. Young of a spectroscope, in which the prisms are made to alter their positions relatively to each other by bending backwards and forwards the metal work to which they are attached. I should of course not wish to offer any opinion on the efficiency or otherwise of this arrangement.

My reason for writing is that in the course of the article Prof. Young goes out of his way to remark that, in attaching bars at right angles to the bases of the prisms in my Automatic Spectroscope, I have adopted a plan of Mr. Rutherford's. Will you kindly permit me to state that I began my Automatic Spectroscope in the year 1862, and that, so far as publication consists in exhibiting anything to a large number of persons, I had published it in the year 1863. I have reason to believe that M. Duboscq also attached bars in a similar manner to the bases of prisms with the intention of obtaining a minimum deviation adjustment, about the same time as myself, or soon afterwards. I do not know at what time Mr. Rutherford may have contrived his plan, but, as I have never read any description of his instrument, I must disavow having adopted any plan of his. At the same time I must remark that it is a small step towards obtaining the complicated movement required to produce an *automatic* minimum deviation adjustment, and it seems to me that it is a step every person would be likely to take who wished to obtain the adjustment by a mechanical motion.

111, Minories, Dec. 12 JOHN BROWNING

Evolution of Light

YOUR correspondent, who describes in the number of NATURE of the 17th ult. a faint light observed by him on tearing strips from a woven fabric in the dark, may be interested to know that a similar phenomenon is noticed by Mr. Grove in his "Correlation of Physical Forces," as occurring with indiarubber waterproof cloth (4th ed. p. 48).

Mr. Grove ranks it under phenomena of heat and light, rather than of electricity.

C. J. T.

Fungi

IT is very unfair that the mushroom family should lie under a ban, because Locusta, at the instigation of Agrippina, employed some kind as a medium for conveying poison into the stomach of Claudius. With equal justice the mild Calenian wine would have been in ill repute because poison was not unfrequently mixed with it—

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* NATURE, Nos 49, 50, and 51

> Occurrit matrona potens quæ molle Calenum
> Porrectura viro miscet sitiente rubetam.

I cannot therefore think that the bad name clinging to the whole family of agarics was thus incurred; for Locusta did not employ a poisonous fungus for her deadly purpose: she mixed poison with some kind of mushroom of which Claudius was particularly fond, and of which he had no doubt often partaken. The words of Tacitus are explicit; he says that "the writers of those times have related that poison was poured into a dish of *boleti*, of which the Emperor was fond;" "Temporum illorum scriptores prodiderint infusum delectabili cibo boletorum venenum." (An. xii. 66.) Suetonius is equally clear: "Boletos in quo cibi genere venenum acceperat." (Nero 33.) Pliny, too, seems to regard the boleti, which he calls an excellent food, as the vehicle conveying the poison: "Veneno Tiberio Claudio principi per hanc occasionem a conjuge Agrippina dato." (Nat. Hist. xxii. 22.) Cases of accidental poisoning by fungi no doubt occasionally happened amongst the ancients as amongst ourselves, but I doubt whether any of the family of fungi were ever designedly employed as a poison. According to Pliny, Annæus Serenus, the prefect of Nero's guard, with his tribunes and centurions, accidentally met their death by eating some poisonous fungus; I am not aware that any other writer records the circumstance; it is rather curious that Seneca, a very dear and intimate friend of Serenus, makes no allusion to the cause of his friend's death, in his touching lament over it, when we remember the philosopher's intense aversion to the fungus tribe. Here is a specimen of his vigorous diatribe: "Good gods! how many men does one belly engage! What! Do you think that those boleti—a pleasant poison—albeit they hurt not now, conceal within them no hidden mischief?" (Ep. xcv.) In another place (Ep. cviii.) he speaks of boleti and oysters together as things he had for ever renounced: "For they are not food, they serve only to tickle the appetite, constraining those that are full to eat more; a very gratifying amusement to such persons as stuff themselves with such things as readily go down, and as readily return." The *boletus* instrumental in causing Claudius's death has been supposed to be the *Amanita cæsarea*, the specific name being given to this fungus on that account, but the point cannot be decided. That the genus *Amanita* was known to Pliny appears pretty evident from his description: it is first covered by a volva, egg-like, and then it breaks through this and rises on its stem. I can find no distinctive mention of the tubes or pores, characteristic of the order Polyporei, in any classical author. The *boletus* of the ancients might have included the modern genus Boletus and some of the Agaricini Some of the Polyporei are no doubt denoted by the *μύκητες ἀπὸ τῶν ῥιζῶν καὶ παρὰ τὰς ῥίζας φυόμενοι* of Theophrastus (iii. 7, § 6); and Pliny probably means the same when he speaks of fungi growing on trees. Whatever the boleti were, they were highly esteemed; we find them not unfrequently contrasted with *fungi* and *suilli*:—

> Vilibus ancipites fungi ponentur amicis,
> Boletus domino. (Juv. *Sat.* v. 146.)

Compare also Martial (iii. 60):

> Sunt tibi boleti; fungos ego sumo suillos

Boleti were so good that you could not trust a slave to convey them to a friend; he would be sure to eat them on the way:—

> Argentum atque aurum facile est, lænamque togamque
> Mittere: boletos mittere difficile est. (xiii. 48)

What the kind known as *suilli*, "hog-fungi," were, cannot be determined.

W. HOUGHTON

Hereditary Deformities

THE facts about hereditary epilepsy in guinea-pigs, mentioned in NATURE of 3rd ult., on page 14, appear to show that mutilations may be inherited when accompanied by functional derangements; though there appears to be very little, if any, evidence of mutilation being inherited when not so accompanied.

Dr. Carpenter says somewhere (I cannot find the reference) that small scars are sometimes more persistent than large ones. We might consequently expect that they would be liable to become hereditary. But this does not appear to be the fact. To mention an obvious instance: in many countries, the ears of all the girls, and of many of the boys, are pierced for earrings. We could not expect to find the perforation hereditary, but it would not be wonderful if the external scar were to be so; the smallness of the operation, not amounting to mutilation, and not producing any functional disturbance, might be thought to be in favour of this result. But I am not aware that it is ever found.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim

The Colour of Feathers and of Butterflies' Wings

THE change of colour observed by E. V. F. (NATURE, No. 55) in the red parts of the wing of a butterfly by the application of muriatic acid, is in all probability due to the red colouring containing a trace of copper in its composition. I have demonstrated the almost universal presence of that metal in the sea, in the earth, in fish, in flesh, in vegetables.

Not long ago Professor Church showed it to exist in the red feathers of birds.

SEPTIMUS PIESSE

Chiswick

Man's Bare Back

WILL you be good enough to favour me with a small space in your excellent journal for these few lines, in answer to Mr Wallace's difficulty with regard to the nudity of the back of man. According to Darwinian principles, there is what is called correlation of growth, by which I believe is meant, that an organ, or some part of the organism, is selected, not because itself is useful, but because its growth is somehow correlated to some other organ which is useful. Now, as a growth is admitted which exists by virtue of its correlation to some useful organ, why should not an atrophy of some part of the organism also be admitted as correlated to some organ which has been naturally selected on account of its usefulness? Although the nudity of the back of man is not in itself useful, nevertheless the atrophy of the hair on his back may be correlated to the development of some organ peculiar to man, and which is useful to him; or, in other words, the growth of the hair on the human back, although in itself useful, is incompatible with the growth of some other organ which may be infinitely more useful to him. Such atrophy, for all we know to the contrary, may be in some way correlated to cerebral development, to the erect posture, to the development of the hand, to the organs of speech, &c. At all events, if we cannot positively state that our dorsal nudity is so correlated, we certainly cannot say that it is not. I do not think that Mr. Wallace is justified in excluding the nudity of the back of man from the theory of natural selection, because he cannot show that it is useful. It may not in itself be useful, but it may be subordinate to some organ which is most useful. I consider that if the principles of a correlated atrophy be admitted, the stumblingblock of our bare backs will cease to trouble Darwinian thinkers. The argument struck me when I first read Mr. Wallace's remarkable "Contributions to Natural Selection," but as I now see, by his article in p. 9, No. 53, Vol. III. of NATURE, that his difficulty has not been answered, I venture to address the foregoing to you, with the hope that my argument may be of some utility.

E. BONAVIA, M.D.,
M.E.S. of London

Nov. 13

Loss of Temperature in Climbing

FREQUENT reference has been lately made to the thermometrical results obtained by Dr. Lortet while walking up Mont Blanc, in which, as stated by Dr. Corfield in NATURE of Dec. 1, his temperature fell about 4° C. in ascending nearly 4,000 metres. Mr. E. R. Lankester informs me that when undertaking the same journey, he also found his temperature much lower when he was up high than when he started.

At first sight this result is unexpected, but it was predicted long ago by Joule, who, in the appendix to a paper read at the British Association in 1843, states thus:—"If an animal were engaged in turning a piece of machinery, or in ascending a mountain, I apprehend that, in proportion to the muscular effort put forth for the purpose, a diminution of the heat evolved in the system by a given chemical action would be experienced."

This is evidently the key to the whole subject, and I hope shortly to publish other results, now in an incomplete form, which bear on the point.

It is evident that the potential energy which results from ascending a hill is gained by the expenditure of work, and a loss of heat from the body must naturally follow; while in walking on level ground, the potential energy acquired when the foot is lifted, and the consequent slight loss of heat, is neutralised by the *internal* work necessary to prevent the foot having any actual energy at the moment it touches the ground.

Upon this theory it is clear that the fall of temperature must be greater as the height arrived at is more considerable; and that the body must soon regain its normal temperature when the experimenter ceases to ascend. Lortet's observations agree perfectly with these requirements, he finding that his temperature was normal in less than half an hour after he had reached the summit.

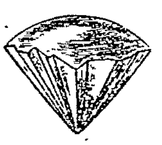
In descending a hill the temperature ought evidently to rise greatly if this explanation is the true one.

A. H. GARROD

St. John's College, Cambridge, December 3

Hailstones

I HAVE frequently observed that when hailstones are large and well formed, they are almost invariably round and smooth at one end, and roughly conical at the other (as in the annexed sketch), so as to suggest the idea that they are broken portions of spheres, of a structure radiating from the centre.



Perhaps some of your correspondents can inform me if there is any proposed theory which accounts for this peculiar form, which should throw some light on the formation of hail.

H. R. PROCTER

Clementhorpe, North Shields

ENCOURAGEMENT TO NATURAL SCIENCE AT TRINITY COLLEGE, DUBLIN

NOTICES have from time to time appeared in this journal of scholarships, exhibitions, &c., obtainable in various colleges at Oxford and Cambridge, for proficiency in the purely Natural Sciences. From these we see that the neglect with which the study of nature has been treated is gradually giving way, and that our great Universities are at last becoming alive to the importance of this branch of learning, and to the necessity for encouraging its pursuit among the students. It is a matter for surprise that no similar mention of rewards for Natural Science is ever made with respect to the University of Dublin. And yet, were a Fellow of Trinity College asked what was being done in this direction at his University? he would probably answer, "Oh, a great deal! there are gold and silver medals awarded at the Moderatorship Examination for Natural and Experimental Science; then there are four or five Science Scholarships given annually." It is true a student may take out his degree with honours in Natural Science, and receives a medal, but let us see what is the course for the so-called Science Scholarships; the subjects are mathematics, pure and applied, for which 350 marks are obtainable, and, as a secondary course, either logic or physics, for which fifty marks alone are given. Such are the rewards and inducements held out to the student of *Natural* Science. It is scarcely to be wondered at that the most promising men in the University do not go in for them, but devote themselves to the more profitable classics or mathematics. For these the rewards are liberal and numerous; there are no less than seventy foundation scholarships, and many others, besides a great number of exhibitions, of which, within the last few months, thirty in addition have been granted by the Board. Not one of these has the man who devotes himself to chemistry, zoology, botany—in short, to Natural Science—a chance of obtaining. We ask—is this fair? Even supposing it unadvisable to divert any of these scholarships or exhibitions from their accustomed channels, yet surely the Board might establish one or two additional ones out of the—confessed to—60,000*l.* annual income.

About a year ago it was rumoured that a "studentship" would in future be given at the degree examination to the first gold medallist in Natural Science, but the idea seems to have died a natural death, and those in whose bosoms a ray of hope had arisen have been doomed to disappointment. It is not to be supposed that the authorities of Trinity College, Dublin, are in any great degree adverse to changes. On the contrary, when a reform (? is not especially needed, they are not unlikely to introduce it; thus, for instance, the harmless old custom of setting the college clock a quarter of an hour late—giving the students, as it were, a quarter of an hour's law—has been abolished, and the hour for "commons" has been altered But we have no wish to lead any one to imagine tha an altogether bigoted and unchanging spirit pervades the University: we have much pleasure in saying that many of its institutions are truly liberal; and we can scarcely doubt that before long the governing body will look with more favour on the Natural Sciences, and that they wil become aware that Ireland—not so very flourishing a present—will be anything but a loser when the Nationa University sends forth a greater number of scientific men

A.

THE CONSTRUCTION OF HEAVY ARTILLERY

II.

CHOICE OF MATERIAL

IN an article which appeared on the 24th of last month we endeavoured to explain the construction of ou large ordnance, and to trace briefly the steps by which the combined strength and simplicity of the present pattern in the British service—the Woolwich Gun, as invented by Mr. Fraser—were attained. Simplicity is one chief element of strength; the fewer pieces anything is made of in general the stronger it is, and it has also the advantage of cheapness; but simplicity is seldom the beginning, it is rather the end of a series of inventions and improvements, and this has been the case in gun manufacture.

Having traced the steps of the process, and glanced a the history of its development, one topic more remains to be treated in order to give completeness to the subject and that is the choice of material; and although the choice of material must come first in actual construction, to know the manner in which the gun is formed, and the qualities sought to be developed in the construction, will be a grea help in understanding what qualities it is desirable that the material should possess. There are two qualities between which the choice lies; these are *hardness* and *toughness* The British Government has decided, we think wisely, in favour of the latter. Hardness is the proper quality to resist a statical force, or pressure; toughness to resist a dynamical force or blow, and the explosion of gunpowder is not only a dynamical force, but it is the greatest that we have to deal with in any mechanical problems. If a hard substance is subjected only to a blow which it is quite able to resist, whose strain is well within the limits of its elasticity, then it is a very fit and proper material for the purpose; and this was the case with the old smooth-bore guns, which were all cast-iron. They were quite strong enough to do with safety all that was required of them But for the force now imparted to rifled projectiles with their immense range, their tremendous armour-piercing *vis viva*, cast-iron guns are altogether inadequate Much lower charges than those of our wrought-iron rifled guns would burst them into fragments. Did nature supply us with a material so hard that the strain of gunpowder was easily overcome by it, i would do very well for all guns. If, for instance, diamonds existed of sufficient size that a piece of heavy artillery might be bored from one, then they would be a very admirable material for the purpose. But, as this is not the case, we must fall back on tough instead of hard substances, the more especially as it does not do to approach the limit of resistance, for hard and crystalline substances have often flaws which no inspection can discover, and which only reveal themselves at the moment of destruction, and also such guns, when they burst, always do so explosively, without previous warning. This is not the case with tough material, which can yield through a very considerable extent before breaking. It may be objected that this could be overcome by increasing the thickness of cast-iron guns. But it is found that, after a limit very soon reached, increase of thickness does not produce increase of strength. The following law has been ascertained, that "no possible thickness can enable a cylinder to resist a pressure from within greater per square inch than the tensile strength of a square inch bar of the same material." That is—to take an example—if a cylinder of cast iron, whose breaking strain is ten tons per square inch be subjected to that amount of interior pressure, it will from the very first occasion begin to give way. At first, the inner surface would be ruptured, and the expanding gas or other source of pressure, taking advantage of the fissures, would quickly extend and complete the work of destruction. The inner portions of such a cylinder have much more work to do than those farther out, the same amount of force acting over a greater extent of surface or lamina. It may therefore be roughly estimated that the strains on the successive portions or rings vary inversely as the square of their radii. Dr. Hart, Fellow of Trinity College, Dublin, taking into account the compressibility of the metal, has given the following formula, $\frac{\sigma}{s} = \frac{r^2}{\rho^2} \frac{R^2 + \rho^2}{R^2 + r^2}$, where s is the strain on the inner surface, σ that on a ring of which ρ is radius, and R and r are external and internal radii of the cylinder. To compare the strains on the inside and outside, let S be the latter, and, as $\rho = R$, the formula becomes $\frac{S}{s} = \frac{2r^2}{R^2 + r^2}$. Applying this to the case of a gun of ten inches calibre, with a thickness of side of five inches, $r = 5$, $R = 10$, and S is to s as 50 to 125, so that more than twice as great a strain is borne by the inner surface as by the outer.

This brings us face to face with one of the great objections to cast-iron guns. In all castings the outer part, which cools first, is stronger than the interior. The metal contracts in cooling, and as the heat first leaves the exterior it first becomes solid, and the inner particles successively unite to it in layer after layer of crystals; so that the centre contains metal but imperfectly coherent. In a casting of two or three feet diameter, a central portion of six, eight, or even more inches in diameter, is found consisting of a spongy mass of scarcely coherent crystals of iron, sometimes with cavities visible to the eye when a section is made. This is exemplified in the annexed drawing (Fig. 1) of a 13-inch sea-mortar shown in section with the head of metal with which it is cast remaining attached, the parts to be cut off and bored out being marked by a black line. The shaded portions represent the weak and porous parts of the metal, which extend down through the centre below the bottom of the powder chamber, where it leaves a soft spot, easily hammered and burnt away by the shock and blaze of the discharge. It is plain that in any piece of artillery thus formed, the strength of the sides must gradually decrease from the exterior to the interior, which is precisely the reverse of what is required. To remedy this the Rodman cast-iron guns, used in the United States, are cast round a core or closed tube, which is inserted in the mould and represents the bore of the proposed gun. Into this tube a stream of cold water is kept continually pouring, so that the molten metal first solidifies round it; and, further, to secure this a fire is kept up round the mould for some time after the casting has begun. Here we have the conditions required in a cast-iron gun, viz. the best and strongest metal in the interior round the bore. Fig. 2 represents a 15-inch Rodman cast in this manner. It will be observed that in its shape, which resembles a soda-water bottle, all angles are avoided and a rounded form carefully preserved. By this another source of weakness is avoided, through compliance with an important law of nature. When any substance solidifies under the influence of heat leaving the mass, "the principal axes of the crystals will always be found arranged in lines perpendicular to the bounding planes of the mass, that is to say, in the lines of direction in which the wave of heat has passed outwards from the mass in the act of consolidation." (The Construction of Artillery, by R. Mallet, M.I.C.E., &c., &c. 1856.) This direction is that of least pressure within the mass, being that of the motion of the heat waves; and the law above stated is part of the far more general principle that in nature the line of least resistance is the one invariably chosen. From this law it follows that wherever there is an angle or sudden change in the form of a casting, through that angle there runs a plane of weakness, arising from irregular crystallization, as the crystals arrange themselves perpendicularly to the surfaces. Every abrupt change in the form of the exterior of a casting, every salient or re-entering angle, no matter how small, upon the exterior of a gun or mortar, is accompanied by one or more planes of weakness in the mass. This was strikingly exemplified in the cylinders of the hydraulic press made for raising the tubes of the Britannia Bridge. The first was made with a flat bottom, consequently it had planes of weakness as shown by the lines VV in Fig. 3, and under the enormous pressure to which it was subjected it gave way, the bottom curving out in the direction of those lines. The second cylinder was made with a rounded bottom, as in Fig. 4, and successfully resisted the pressure to which it was exposed. Cast-iron guns give evidence in bursting of planes of weakness in accordance with this law. The usual lines of fracture are shown in Fig. 5. Any visitor to Woolwich Arsenal can, by inspecting the cemetery where the guns burst in proof or for experimental purposes are preserved, verify this law of nature from many examples. By the form of the Rodman gun this source of weakness is avoided. It is a cast-iron gun, made on thoroughly scientific principles, in which the material is used to the utmost advantage, and therefore it may justly be compared with the Fraser gun, in which the same thing is done with wrought-iron, to measure the value of two materials. A casting is always cheap compared with a forging; in this point the Rodman has the advantage. But at its best, cast-iron has only one-third the strength of wrought-iron. Consequently the Rodman gun cannot be safely rifled. It fires heavy round shot, but with a range and accuracy greatly inferior to that of an elongated rifled projectile. Its initial velocity is high, but it is not long kept up. At close quarters its *racking effect* upon armour plate would be very severe, but its penetrative power is low. In the extensive experiments made at Shoeburyness in 1868 the 10-inch Fraser gun of 18 tons weight penetrated fifteen inches of iron (in three 5-inch armour plates), upon which the Rodman 15-inch gun of 20 tons only made a shallow indent. Further, being cast-iron, they are liable to burst explosively without any previous indication, and from the metal being in a state of great tension, being cooled from the inside, they have been known to break up in store.

The facts stated in the comparison of the best and most scientifically-constructed cast-iron with wrought, seem to be very decisive against the former. Two other materials have to be noticed. One is bronze, or gun-metal. It has some admirable qualities for making a gun. A bronze gun is hardly ever known to burst under ordinary circumstances. So great is its tenacity that with continued firing such a gun has been known to swell and change form without bursting.

But there are two objections to its use for heavy artillery that are decisive against it:—It is very expensive. The respective cost of guns of the several materials treated of in this paper may be taken in general as stated in the following table:—

Cast-iron guns . . .	£21 per ton	weight	of gun.
Armstrong wrought-iron	£100	,,	,,
,, ,, Fraser construction	£65	,,	,,
Steel on Krupp's or Whitworth's plan .	£170	,,	,,
Gun-metal	£190	,,	,,

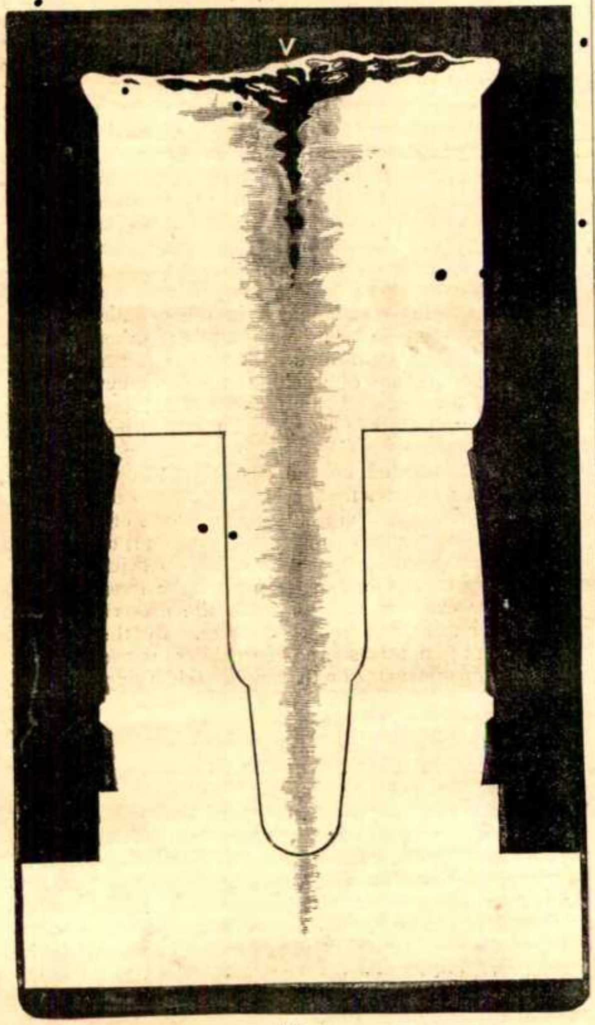


Fig. 1.

But a still more fatal objection to gun-metal for heavy pieces of artillery is its softness, and the rapidity with which it becomes heated. It is therefore quite unfit for the large charges of powder and immense rifled shot, with its severe friction on the bore, which are used in armour-piercing guns. Even small bronze guns, when fired repeatedly, exhibit the curious phenomenon of "drooping at the muzzle." This has been very interestingly explained by Mr. Mallet in the valuable work before referred to, and shown to arise chiefly from the rapidity with which that metal takes up heat. For large guns it is a material out of the question.

The last material to be spoken of is a far more formidable rival to wrought-iron, namely, steel. Of this Krupp's guns and Sir Joseph Whitworth's guns are constructed. The latter are made in a series of cylinders like the Armstrong coils, and which fit very tightly one over the other, and are forced on each other by hydraulic pressure. The former are bored out of huge solid ingots, which have been hammered to give them a fibrous character. Now steel has some admirable qualities as a material for heavy guns. It is very hard; and this makes it very suitable for the grooves of a rifled gun which have to bear such severe friction from the shot. For this reason all our wrought-iron guns are provided with an inner tube of steel. Moreover, it is very strong; the average breaking weight of a bar of steel of an inch sectional area is thirty-one tons, against twenty-five tons for a similar bar of wrought-iron (in the direction of its fibre) and ten tons for cast-iron; and when toughened by being tempered in oil the breaking weight rises as high as forty-five tons to the square inch. The reason why steel is so much stronger when tempered in oil instead of water, is, that the boiling point of oil is so high (600° F.) that it withdraws the heat very slowly from the red-hot mass plunged into it, compared with water, which is turned into vapour at 212° F. The steel for the tubes of the Woolwich guns is always thus tempered. But though steel has these admirable qualities as a material for heavy artillery, there are very grave objections to its use, and, on the whole, good reasons for preferring the wrought-iron guns to any steel ones that have yet been made. One is the greater cost, as shown in the table given above. A much stronger reason is, that though steel bears a gradual pressure so well, such as that of a testing machine for the breaking weight, it is brittle under a very severe dynamic strain or blow. When a steel gun bursts, it does so explosively without previous indications of yielding, and breaks up into fragments like a cast-iron gun. Another objection, and one perhaps even still more weighty, is that it is very difficult to make steel in large masses homogeneous throughout, and free from flaws. It is very much a chance whether it is so or not; and in the case of a gun, this is determined by the fact of its not bursting in service, but cannot be settled by a few proof shots beforehand. A good steel gun is a weapon of wonderful power and very great endurance. But one apparently exactly the same, and made in the very same way, in fact a twin gun, may burst before the hundredth round, or even at the first or second. From time to time, inventions of some process to make steel cheaply and of a homogeneous texture are announced. When some such announcement passes into realisation, it will be time for our Government to abandon a system which arms the country with guns at a moderate price, of a power which weight for weight is not surpassed by those made on any other system, and whose endurance may after proof be relied upon. Our gunners have no reason to be afraid of their own guns. Many of the continental Governments have been supplied with Krupp's steel guns, but very frequently they have burst in an unceremonious and unsatisfactory manner. On one occasion, the director of the Artillery Depôt at Tegel, near Berlin, was killed by the unexpected bursting of a steel 4-pounder gun. The manner of manufacture shows that steel is not calculated to bear a high dynamic strain. At the great gun factory at Essen the steel is hammered with steel hammers of immense weight, as much as 50 tons; but they are single action hammers, lifted up by steam, and allowed to fall by their own weight.* The 12-ton hammer used for forging our wrought-iron guns, which is driven down by steam power, has much greater dynamic force than the far heavier ones with which Krupp forges his steel. But the steel would break and crumble under a swift blow.

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* The greatest achievement of Krupp's Gun Factory is a fifty-ton gun to fire a 100lb shot, which he presented to the King of Prussia; a fitting present to make a monarch. It cost nearly 10,000*l.*, and occupied ten months of labour, night and day, in its manufacture. It was exhibited at Paris in 1867. Alas, that it should now return there under far different circumstances! But the manner of its construction cannot claim the merit of high scientific principles. A steel tube was formed sufficiently strong to resist the discharge; but as this would be so light that the recoil would hurl it into the air, like the toy cannons of our boyhood, it had shrunk round it enormous cast-iron rings or tubes. This was metal used for weight and not for strength. Whereas, the scientific principle very fairly laid down by the late Captain Blakely, R.A., is that "a gun should, if possible, be constructed in such a manner that each part of its mass would do its due proportion of work at the instant of firing."

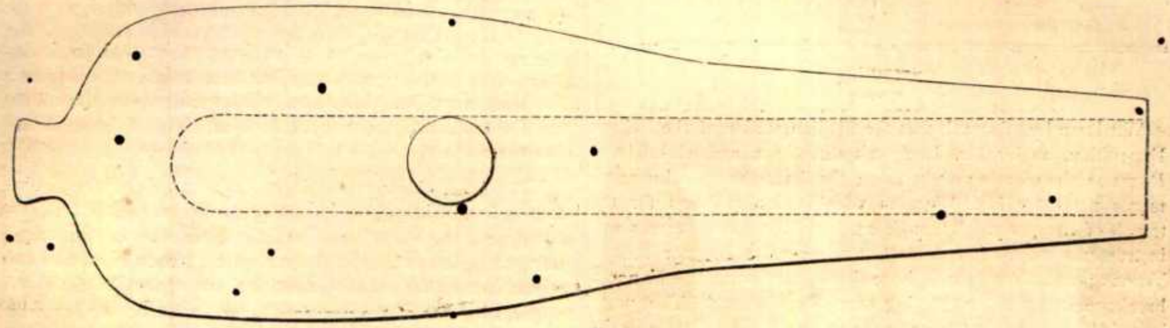

FIG. 2.

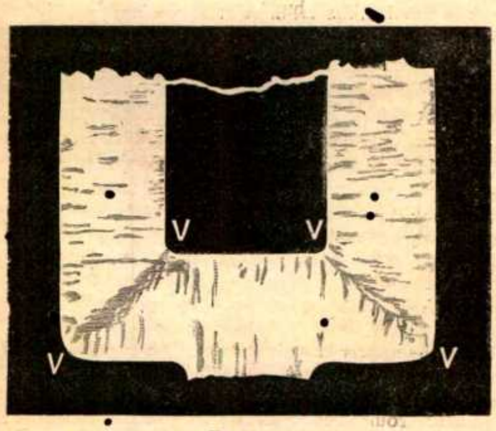

FIG. 3.

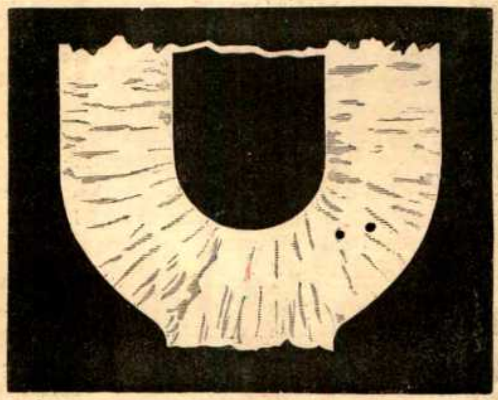

FIG. 4.

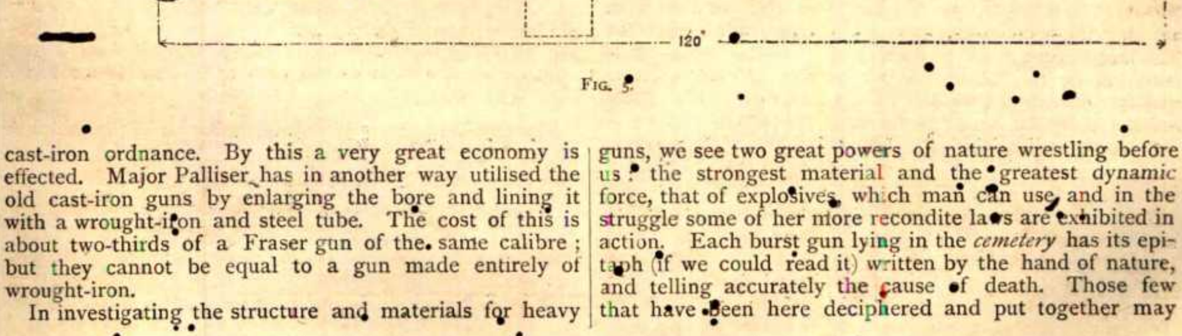

FIG. 5.

From what has been stated, it will be understood that the wrought iron now used for our guns, which was adopted upon Mr. Fraser's recommendation previous to his invention of our present system of construction, is of a soft and cheap character, instead of the hard, steely iron, which is much more expensive, and of which the guns were formerly made. The bars for coils are made in the Royal Gun Factories from scrap-iron, and from the stock of obsolete cast-iron ordnance. By this a very great economy is effected. Major Palliser has in another way utilised the old cast-iron guns by enlarging the bore and lining it with a wrought-iron and steel tube. The cost of this is about two-thirds of a Fraser gun of the same calibre; but they cannot be equal to a gun made entirely of wrought-iron.

In investigating the structure and materials for heavy guns, we see two great powers of nature wrestling before us: the strongest material and the greatest *dynamic* force, that of explosives, which man can use, and in the struggle some of her more recondite laws are exhibited in action. Each burst gun lying in the *cemetery* has its epitaph (if we could read it) written by the hand of nature, and telling accurately the cause of death. Those few that have been here deciphered and put together may form, it is hoped, a not uninteresting page in her note-book.

The authorities for most of the statements in this paper are, besides Mr. Mallet's very valuable work before referred to, two exceedingly interesting papers in the Proceedings of the Royal Artillery Institution, by Captain F. S. Stoney, R.A., Assist. Supt. Royal Gun Factories, entitled "A Brief Historical Sketch of our Rifled Ordnance," and "The Theory of Gun Architecture."

NOTES

WE learn by telegraph that the Sicilian section of the Eclipse Expedition, under Mr. Lockyer's directions, arrived safely at Rome on the morning of the 12th; thanks to the capital arrangements made by Mr. Vignoles with foreign railways for the transit of the party. Through carriages for the observers and instruments were taken from Ostend over the Brenner Pass to Verona; and they would have been sent further had not a breakage occurred in passing over the Brenner. The party intended leaving Naples for Syracuse in H.M.S. *Psyche* in the course of yesterday. All the Governments are aiding to the utmost in their power.

WE are glad to be able again to report favourably of the state of both Sir R. Murchison and Prof. B. Stewart. The former has continued to gain somewhat in strength, and the progress of the latter from day to day has been as satisfactory as could be desired.

WE have to announce that Mr. C. L. Bloxam has succeeded the late Dr. W. A. Miller as Professor of Chemistry in King's College, London, and that Dr. Debus, F.R.S., has been appointed Examiner in Chemistry to the University of London, in the place of the late Dr. Matthiessen.

PRESIDENT WOOLSEY of Yale College, U.S.A., has announced his intention of resigning his office. He has been President of Yale for more than a quarter of a century.

L'ABBE MOIGNO has discontinued the publication of his *Les Mondes* owing to the scarcity of paper and the impossibility of sending his periodical to the subscribers. L'Abbé Moigno, however, attends regularly the sittings of the Institute. M. de Parville is now the scientific editor of the *Journal Officiel*, as in former times. He is the only contributor to that paper who has been kept in office, which is highly creditable both to M. de Parville and to the French Government. M. de Parville's contributions are pirated by almost every French paper published in Paris, the only exception being the *Liberté* when M. de Fonvielle was in Paris. Now, *Liberté* must do like others. Reports on scientific matters are published in the *Journal Officiel* with more regularity and space devoted to them than in former times.

M. DECAISNE, Professor of Agriculture at the Museum, has laid before the French Institute, of which he is a member, a scheme for the early growing of cabbages, radishes, &c., &c., which are to be sown in land richly manured, kept for a fortnight, and used stem and root, as a new description of vegetable. This diet is intended to protect Parisians against scurvy when the use of salt beef becomes still more frequent than it is now. The working of this scheme is superintended by M. Toigneaux, the editor of several agricultural papers. Different pieces of waste land close to the walls were appropriated, and vegetables of that description must be now actually on sale.

AS soon as the investment of Paris was completed, the authorities took measures for preventing the destructive effects of shelling. Bills were printed and affixed to almost every house with directions for stopping the fire set up by the bursting of shells. Large tubs, filled with water, were placed on every floor of the large houses and private buildings. Although covered carefully with canvas, the water, having been left for weeks and weeks, became corrupted and fœtid. Proper instructions were given for stopping the infection by the using of charcoal. Two of these tubs are placed in the hall where the French Academy held its sittings, and two others in the *Salle des pas perdus*, by which visitors and members are introduced to it.

M. BARRAL, well known for the part he took in balloon experiments twenty years ago, has volunteered with his son for the balloon service. M. Barral was for many years the editor of the *Presse Scientifique*, a periodical which was discontinued a few years ago.

THE *Société Chimique* continues its sittings, devoted exclusively to warlike, culinary, and sanitary purposes. This body has offered a gun to the Provisional Government. It was built on new principles, and paid for out of the funds of the society, which do not flow out of the national exchequer, the *Société Chimique* being one of the very few French scientific bodies which are not supported by the State.

M. DUMAS is always in earnest for any improvement of diet. He has patronised the use of gelatine obtained from the carcasses of oxen, sheep, and pigs, by hydrochloric acid. The French Institute appears to have reversed the verdict given thirty years ago against the gelatine as proposed by D'Arcet, the only difference being that gelatine is not now prepared by steam, but by the action of acid on animal products, and that a new name, *osseine*, is given to the substance. He has also supported a scheme for grinding the whole of the corn, except the exterior coating, and the making of the flour so obtained into bread. But there is so large a quantity of corn and flour at Paris that the Government, although approving the scientific principles of this new method of grinding corn, declared the suggestion to be useless for the moment.

THE same answer was given to another suggestion, relating to the roasting of the corn for making a kind of gruel called *bouillie romaine*. It appears that this was the diet of the Roman legions, and that the gruel so obtained is very acceptable indeed. A kitchen for public distribution was said to be opened, but Government interfered, thinking, very properly, that it would be good to delay such steps for many weeks. They will use it only if the siege is prolonged so long that there is some danger of actual starvation.

M. BERTHELOT, although not a member of the French Institute, is the president of a standing committee for using scientific discoveries in the defence of Paris. That committee holds its sittings at the Ministry for Public Instruction, of which M. Jules Simon is the head. Many valuable suggestions have been adopted by that committee, which was closely connected with the Committee of Barricades, presided over by Rochefort.

M. JAMIN, the celebrated Professor of Natural Philosophy at the Sorbonne and member of the French Institute, has enlisted as a private in the artillery of the National Guard, and is doing his duty regularly, although begging for a weekly authorisation from the lieutenant in command to enable him to attend the sittings.

THE two Becquerels, father and son, have left Paris, and their place is filled by supernumeraries. Some papers have remarked very sharply upon it, and asked for the discharge of the younger.

THE following notification has been issued from the Home Department respecting the International Exhibition of 1871, dated Simla, October 11, 1870:—His Excellency the Viceroy and Governor-General in Council has been pleased to nominate the following gentlemen to form a Central Committee to assist the efforts of private individuals and others who may be willing to forward the objects of the Annual International Exhibition of Select Works of Fine and Industrial Art and Scientific Inventions, to be held at South Kensington on the 1st of May, 1871 : President, the Hon. Sir Richard Temple, C.S., K.C.S.I.; Members, his Highness the Maharajah of Jeypore, G.C.S.I., the Hon. B. H. Ellis, C.S.; Major-General the Hon H. W. Norman, C.B.; the Hon. J. Bullen Smith; the Maharajah of Vizianagram, K.C.S.I; Mr. W. G. Romaine, C.B.; Mr. E. C. Bayley, C.S., C.S.I.; Colonel the Hon. F. Thesiger, C.B.; Mr. Allan Hume, C.S., C.B.; Mr. H. Rivett-Carnac, C.S.; Lieut.-Colonel Baigrie, B.S.C.; Major O. T. Burne, Private Secretary to his Excellency the Viceroy. Honorary Secretary, Mr. H. Rivett-Carnac, C.S.

AT the meeting of the Royal Geographical Society, held on Nov. 29, a paper was read "On the Geography of the Sea Bed," by Capt. Sherard Osborn, R.N. The author gave an account of our present knowledge of the configuration of the bed of the ocean, as derived from Admiralty surveys and submarine telegraph expeditions during the last fifteen years. His explanations were illustrated by a number of diagrams showing sections of the North Atlantic and other oceans. It has been definitely ascertained that the greatest depth of the ocean does not reach 3,000 fathoms in any part where telegraphic lines have been laid. The bed of the North Atlantic consists of two valleys, the eastern extending from 10° to 30°, the Western from 30° to 50° West longitude. The extreme depth of the eastern valley is under 13,000 feet, which is less than the altitude of Monte Rosa. This valley has been traced southward to the equator. It is separated from the western valley by a ridge in 30° West long., in which the average depth is only 1,600 fathoms. This ridge terminates to the north in Iceland, and southward at the Azores, so that it is volcanic in its character at both extremities. Its extreme breadth appears to be under 500 miles, and the Atlantic deepens from it on both sides. Explorations carried on in the Mediterranean, the Red Sea, and the Indian Ocean, showed similar uniformity in the level of the sea-bottom; and the general conclusions arrived at by Capt. Osborn were that in the deep sea there is an absence of bare rock, and that there are no rough ridges, canons, or abrupt chasms. Moreover, that the bed of the deep sea is not affected by currents or streams, even by those of such magnitude as the Gulf Stream; but that it rather resembles the prairies or pampas of the American continent, and is everywhere covered with a sort of ooze or mud, the *débris* of the lower forms of organic life. In the course of the discussion, Professor Huxley said that, viewed on a great scale, there would be but slight difference between the large general features of the ocean bed and the dry land; but that the smaller features would be different, as the effects of denudation would not appear in the deep ocean bed. To the naturalist, the observations of the telegraphists were of great importance, as showing the existence of low forms of animal life in the deepest seas; and recent discoveries had shown that the most characteristic organisms of the deep-sea beds, named coccoliths and coccospheres, existed at all depths, even in shallow shore waters, and were also found fossil in sedimentary rocks of all epochs—a discovery of great interest, as confirming the view of the uniform conditions of submarine deposits in all ages of the earth's history. He was opposed to the view that the animals found living in the dark regions of the lowest sea depths depended for light upon the phosphorescence of some of the species, and saw no reason for concluding that they could not, like fungi, exist without light. He also doubted the accuracy of the very low temperatures said to have been found at great depths, and thought that those taken in the Indian Ocean might be explained by the fact that they were taken with thermometers not rectified for pressure.

AT a meeting of the Scientific Committee of the Horticultural Society, held on the 7th inst., a remarkable paper was read by Mr. Andrew Murray, on the subject of Mimetism, especially as exhibited in the instances of the South American butterflies, which have already been discussed in our columns. Mr. Murray adduced a number of arguments which he considered told against the theory that the Mimicry had been produced by Natural Selection, and attributed it to Hybridisation. We hope to be able to publish the paper in a future number.

A CONFERENCE of gentlemen interested in Scientific Education was held in the Royal Institution, Liverpool, on Tuesday night, the 6th inst., and unanimously passed a resolution declaring the advisability of establishing a Science College in that town, the cost of which was estimated at about 50,000*l.*, and a committee was appointed to take steps with the view of carrying out the object. Possibly the recent meeting of the British Association in Liverpool may have given an impetus to so laudable a design, which we hope may be successfully carried out.

A NEW quarterly journal is about to be published by Messrs. Groombridge and Sons, under the title of *The Landowner and Farmer's Note Book*. It will aim at presenting a well-arranged series of notes and suggestions in connection with estate and farm management.

MESSRS. LONGMAN intend to issue early in 1871 a Supplement to "Watts's Dictionary of Chemistry," bringing the record of chemical discovery down to the end of the year 1869. It will form a volume of about 900 pages, and many of the former contributors have consented to furnish additions to their articles.

THE new general Government of Elsass proclaimed by the King of Prussia, embraces, in addition to the departments of the Rhine which constituted the former Alsace, the arrondissements of Saarbourg, Château Salins, Saarguemines, Metz, and Thionville, taken from the departments of Moselle and Meurthe, in Lorraine. With the addition of these districts, the boundary of the new province marks out very nearly the German-speaking part of France. The fortresses of Thionville and Metz in the north, the natural barrier of the Vosges Mountains, and again the fortress of Belfort, in the south, will then protect the frontier of Germany towards France. From an article in the December number of *Petermann's Mittheilungen*, we learn that the new government has an area of 5,825 English square miles. This space is represented in English soil very nearly by the counties of Hants, Surrey, Sussex, and Kent, and it cuts off a thirty-sixth part from the whole of France. The fertility and industries of Elsass, however, support a population of 1,638,500, or a twenty-third part of the inhabitants of France, and the density of its population is comparable to that of the plains of China. In the new government the purely German-speaking area measures 4,425 square miles; the purely French parts, which lie chiefly round the fortresses in the north and south, are together 985 square miles in extent, and the territory of a mixed language, which lies in patches between, makes up an area of 415 square miles.

THE series of botanical diagrams by Professor Balfour, of which we noticed the first some time since, is now issued in a complete form by Messrs. W. and A. K. Johnston, and comprises four large sheets 4ft. 2in. by 3ft. 6in. on rollers and varnished. They supply a marked desideratum for botanical teachers and lecturers, supplementing, but not replacing, Prof. Henslow's diagrams published by authority of the Science and Art Department. In the latter we have delineations of a plant belonging to each of the most important natural orders, with details of their structure. In Prof. Balfour's diagrams, each separate organ is taken, and the different variations of its form and structure illustrated. The first sheet showed the organs of plants generally, the tissues, root, and stem; in the second we have leaves and their modifications; in the third, inflorescence and the whorls of the flower, while the fourth presents us with the pistil, ovule, and seed, and the organs of flowerless plants. Many of the drawings are, strictly speaking, diagrams, that is, artificial representations of typical structure. They are well and clearly drawn, and sufficiently coloured to add to their lifelikeness. With these sheets, and either Henslow's series, or better still, a few hand-made ones on a larger scale, and actually taken from life, the botanical lecturer would be well provided; and the comparatively low price at which they are published ought to insure for them a very large sale. The handbooks which accompany them are admirably drawn up.

FROM China samples of Poyang Lake coal have been forwarded to the Admiralty authorities in the hope that they may be found useful for the navy in that station.

THE Government of India has taken further measures for carrying on coal borings in Central India, but only in Berar, and not in the Nizam's dominions.

THE Indian Government have again deputed Mr. T. W. H. Hughes, of the Geological Survey Department, to prosecute the investigation of the Wurda River coal-beds in the Central Provinces, and to report on the line of railway best calculated to develop the collieries.

OUR Darjeeling hill district of India is likely at length to experience a development of its mineral produce, instead of being left to depend on tea culture. Limestone, copper, and iron have been discovered not only in the Darjeeling territory, but also in the lately annexed Dooars of Bhootan. The Nepaulese have applied to work the iron ore, and the Commissioner of Kooch Behar has been authorised to divide the tract into sections, and to let out the mineral privileges by the year to the highest bidder.

A VERY important discovery of silver is reported from Copiapo, in Chile, which has a large silver district. El Carmen Mine is now producing 16,000 marcs of silver per month; that is 128,000 ounces, worth about 32,000*l*.

MR. JAMES GALBRAITH sends to the *Ararat and Pleasant Creek Advertiser* (Victoria), of Sept. 16th, an account of some huge boulders found near the townships of Hamilton and Coleraine, which he believes to have been deposited there by the agency of ice. He states that the whole of the western district of Victoria is covered with boulder clay. The ironstone gravel called buck-shot, which is found in patches on the surface and at a short distance below the surface, all over the plains to the south of Ararat and Beaufort, is, no doubt, a deposit from floating ice. The "grey stone" on the Ararat and Port Fairy road has been floated to its present position by ice; and a number of granite boulders on the road between Moyston and Ararat, some of them a great deal larger than the "grey stone," must have been brought to their present position by the same agency. No polished or striated rocks have, however, as yet been detected in the colony.

WE learn from the *New York Times* that an ice machine, constructed on Tellier's principle, is now being exhibited in the United States. The material used is gaseous ammonia, which is liquefied by pressure. It is said that the machine will make 100 tons a day, at a cost of four or five shillings per ton; and that the ice made by it is transparent and durable. The cooling effect of the vaporisation of liquefied ammonia may be applied to chambers containing articles of food to be preserved, or refrigerators might be constructed on any scale. The holds of ships could thus be converted into refrigerating chambers with the greatest ease, offering a ready means for the conveyance of meat from one port to another in a wholesome state.

BALLOON ASCENTS FOR MILITARY PURPOSES

II.

BEING detained in England by unavoidable circumstances for some time longer than I expected, I will try to give the British public an adequate view of the action of our French Institute in the matter of balloon navigation; and will confine my criticism to an exposition of M. Dupuy de Lome's own views, which were supported by the Government, so far as to give to this learned man a credit of nearly 2,000*l*. for the construction of his balloon. Perhaps the observations I have published in the *Liberté* and offered to some of his assistants in private conferences, have produced some alteration in the original scheme. It is a matter of which I cannot be made aware by any means, and I must suppose things to be as they were when I left Paris in my own balloon.

M. Dupuy de Lome's balloon was to be constructed out of silk, and I understood that people were engaged in looking after the stuff in different Parisian fancy shops. But it requires a great deal of search to find silk enough to construct a large balloon somewhat larger than Mr. Coxwell's "Research" and having a larger surface besides in consequence of its intended elongated form, the spherical form being, as is known, the one which offers the largest capacity for the smallest surface.

It may be well to remark that balloons are somewhat elongated in the present fashion of building them, the elongation being vertical instead of horizontal as required by M. Dupuy de Lome's scheme. But the elongation of the balloon is a thing of which we will speak more fully in another place.

M. Dupuy de Lome was not afraid to have his balloon shaped like an egg, or, rather, like a fish with two tails and no head, but he did not wish to try it with pure hydrogen gas. It is not because he thinks that hydrogen gas is too expensive or too difficult to prepare, it is only because he supposes that hydrogen gas would escape in spite of varnishing. I cannot agree with him in this respect, not only because my friend Giffard's balloons have proved perfectly hydrogen-tight, but principally because ordinary balloons filled with hydrogen have done good service. Amongst these balloons I may mention the one which conducted the unfortunate Worth to be lodged in a Cologne cell, not by any fault of its gas-holding power, but merely because aeronauts were foolish enough to open their valve when Prussians were firing at them, and preferred trusting to Prussian humanity to relying upon the dark mantle of the night.

Official people engaged in ballooning seem to have strong prejudices against hydrogen gas, as may be noticed from their acts; the battery I had caused to be constructed for filling poor "Liberté," having been wholly disregarded by them as unfit for use.

Many inventors have published descriptions of working aerostats, but very few of them were really professional aeronauts. I shall be justified by facts in stating that scarcely any of them was in a position to form an adequate idea of the most essential features of any really scientific scheme. Almost everyone of them has forgotten that the principal condition of success is an easy working. M. Dupuy de Lome has not avoided that fault, and his balloon is to be shaped, as we have remarked, like a fish, which is to be kept in an horizontal position. That condition is very difficult to accomplish when you have to look to so many other things at the same time. M. Dupuy de Lome is so well aware of the difficulty of having his balloon always progressing horizontally, that he proposes to get rid of it by keeping the balloon always filled either with gas, or with ordinary air by means of a pump. It is an instance of avoiding our old French saying, *Le remède est pire que le mal*. This saying is so much the more justified, that M. Dupuy de Lome is not contented with sending air into his balloon when it is required to fill it. He has constructed ready for the purpose a special balloon, which is to be enclosed within the large one, and which being alternately filled and unfilled according to the requirements of the external pressure, keeps the balloon always in a state of full expansion. The pressure inwards is always a little greater than the pressure outwards, which is in itself a new objection, as this artificial pressure increases the rate of escape for the gas by the small holes which are unavoidably so numerous in the whole surface. Besides, if there are some defective places, they may probably be opened by that pressure.

As you may understand from this explanation, M. Dupuy de Lome was very careful, and his scheme is worked out with every required detail, to show the corollaries which follow from the first assumptions. M. Dupuy de Lome being a very clever ship constructor, is ready to meet the difficulties, but he was not willing to avoid them at once by having a more simple scheme to work out. It is so much the more to be wondered at, if this clever aeronaut has not adopted this policy, inasmuch as he does not profess to go against the wind, but to design a contrivance which may help aeronauts in using the wind for a certain purpose, as returning to Paris from a town located at some distance; as Lille, Le Mans, &c. M. Dupuy de Lome intends to attain the desired result by making a definite angle with the direction of the reigning wind, which supposes on the part of the aeronaut some previous knowledge of the state of atmospheric currents, their change of duration, and different directions at different elevations; the principal feature of his intended directing balloon being the grand idea of having the motive power like an auxiliary implement for giving to the balloon differential motions. It does not, of course, prevent the aeronaut from using the deflections and variations of the wind according to the elevation of his balloon at any moment. The working out of these aerial manœuvres supposes necessarily that aërial navigators can know at any moment the place where they are. It requires constant attention from the aerial travellers, who are supposed to be supplied with every possible instrument for looking over the land and finding the places on the map prepared for that very purpose. It would be of itself a most interesting chapter, the better construction of such maps, as well as the determination of the means by which public authorities could give warnings to the aerial travellers. But in the present state of things, I should not be justified if I did not abstain from giving details which may prove useful to the invaders of my native land. I will be satisfied with saying, moreover, that the taking of the point in sailors' fashion is quite out of the question. The only condition is the view of the land remaining always at the command of the observer, or only being lost for short intervals, during which more than the usual attention is required. I have invented an apparatus called an *aerial planchette*, for helping aeronauts in the determination of their way, but from the experience of my last excursion I have lost every confidence in my nstrument. I think that it is quite useless; the only thing required being good maps and better eyes, helped by powerful opera-glasses. The power of these can be enlarged by a very simple contrivance, which I mention merely to show that I know what is still to be done in this respect.

The question of the motive power to be employed is not of so much importance as was supposed at the first instance; and it s very easy to understand why, admitting that we want only a slow motion. I should not object, of course, to a quick displacement; but I am satisfied it cannot be obtained except by conrivances very difficult to imagine, and even more to realise, and besides it is not required for the special purpose in view, the eturning to Paris from a French city which German armies have not occupied. The rate of motion will be improved by degrees, and will not amount to a large increase, except by the use of steam engines, which requires a great many preliminary steps o prevent the gas of the balloon from being lighted by the fire from the furnace, which would lead to the destruction of the balloon and of the aeronauts. The simplest contrivance will be he best if it proves useful. I should advise to arrange the motor apparatus so that it could be used by hand, and, besides, that it could be very easily thrown overboard like ballast n case of need. These two conditions being of much importance or our purpose, if I start for Paris, which I hope will be the ase, I shall adhere strictly to them.

I have no objection to use a rudder, which may be constructed n a manner very easy to understand, but I should feel very much disposed to dispense with it. I think that a propelling machine may be arranged so that no rudder at all will be put nto operation. I am afraid to give more substantial explanaions, which could hardly be offered without giving a full knowdge of my intended construction, which is not my purpose.

The contributions of M. Dupuy de Lome to the *Comptes ndus*, have been sharply commented upon by the *Aeronaut*, special paper devoted to the aerial navigation worked out by e *plus lourd que l'air* system, as inaugurated by Lalandelle and adar, and many other gentlemen of very little or no scientific ualifications. But every scientific man must confess that these teresting papers constitute by themselves a very valuable acquiion to general knowledge, independently of their special m. M. Dupuy de Lome has given at the same time many lculations to show to what elevation a given balloon can attain nder the conditions he has adopted, viz., constant fulness, and a certain excess of internal pressure for giving it a stability of form and of equilibrium.

Without quoting M. Dupuy de Lôme's paper, and even correcting some parts of it, I will give a rough idea of the analytical questions involved in the calculation of the circumstances of an aërostatical ascent. I suppose, firstly, that the air has no horizontal movement at all, and that the only questions are to ascertain the elevation which the balloon may reach, the time that may be required, and the velocity with which the balloon ascends at the various points of its vertical course, as well upwards as downwards. There are besides two accessory suppositions which are required. The first is, that the balloon does not lose its gas by any *exosmose* during the experiment; and the second is that the temperature of the air, as well as the degree of moisture, is not altered in any degree. These conditions are hardly to be expected, but they are required for mathematically working out the analytical equations.

M. Dupuy de Lome, however, would not have been placed in a position to proceed with his calculations, if he had not very cleverly evaded the consideration of the other conditions, which are insuperable, owing to our imperfect knowledge of the atmosphere, as we shall see hereafter.

W. DE FONVIELLE

PROF. WILLIAMSON'S INAUGURAL LECTURE TO THE FACULTY OF SCIENCE AT UNIVERSITY COLLEGE, LONDON

THE great value of scientific knowledge as a means of culture, a promoter of civilisation, and one of the most powerful levers of national prosperity, seems at least to receive its due acknowledgment in the land of Bacon and Newton, Sir Humphry Davy and Faraday. The recent efforts to introduce science into the public schools of England appear as a consequence of this recognition. A great variety of opinion, however, exists as regards the mode by which scientific knowledge ought to be imparted to the people. Some believe that a young farmer ought to be taught agricultural chemistry, the man at the furnace the chemistry of iron melting, and the maker of colours the chemistry of colours. This is what is called by many "technical education," for the promotion of which great efforts have been made of late. Technical education in this sense would be a mistake. It would not be difficult to show that it is impossible to teach, with any considerable effect, agricultural chemistry, which is the application of certain chemical principles to Agriculture, without a knowledge of these principles. These, with others, form part of the science of Chemistry, and it is clearly absurd to isolate them and teach their application in some particular case. The working classes of England want a knowledge of the elements of pure science; and they are sure to make useful application of this knowledge as soon as an opportunity offers itself. In this sense Professor Williamson expresses himself in his admirable lecture, "A Plea for Pure Science," * which on account of its sound views on some of the most important questions of the day, we recommend to the attention of our readers. On p. 3 Prof. Williamson says, "Now there are in education two great national parties, corresponding to the two most different points of view from which the preparation of any young person for his career in life can be considered. I submit that the progress of education will be proportional to the consistency and completeness with which the functions of these two parties are systematised and developed.

"The first step towards that object is to know and acknowledge their respective characteristics.

"One party looks to the special duties for which a young person has to be prepared and the material difficulties which he is expected to encounter. They see that the success and happiness of each individual are proportional to the efficiency with which he discharges the aggregate of the special duties of his station in life; and they accordingly recommend that each youth be placed in circumstances which may induce him to imitate accurately the doings of some one who is known to be successful in a station such as he is intended to occupy. The other party looks to the general qualifications which experience has shown to be most important for any success in life; and to the means by which they are most effectually acquired. They see that men who have been taught to understand and apply the best-known general principles are able to master a given set of practical details with a facility and completeness which other men do not attain. They know that a general principle of nature is an instrument of thought applicable to the explanation of an infinite variety of phenomena, and they recommend that every one be placed in his youth somewhere where he may best learn such general principles. The first party takes little account of the development of the mental powers as a distinct object to be aimed at in education; the second attends but little to special operations.

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* "A Plea for Pure Science" By A. W Williamson, Ph D., F R.S.

"The former recommends special or technical instruction with a direct view to material success in a particular business, the direct aim of the latter is to educate and strengthen each individual mind. The essential differences between them arise from the fact that they look at the question from opposite sides, and respectively put forward what they see most clearly."

After this sketch of the outline of the characteristics of the two great parties, Professor Williamson describes some results of the arrangements recommended by them respectively. The great aim and object of science is to systematise our knowledge; and the discovery of an idea which helps to arrange any considerable number of facts in such a manner as to facilitate their apprehension, is the highest result of scientific work. We are then led in an admirable manner from the periodical disappearance of the sun to the law of gravitation as a model of scientific work. But it must not be supposed that the application of science to practical purposes is the greatest reward of scientific work. We read on page 12—

"To any one possessing a clear and vigorous mind, the acquisition of an idea which helps to explain things is a source of intense pleasure. He feels that it enlarges the scope of his mind, and gives him new power; and when facts, previously unintelligible, are explained by the aid of such an idea, they immediately acquire vivid interest and special value to his mind; such facts seem to gain life by acquiring an intelligible place in the system of nature.

"I believe that the triumphant feeling of the enlargement of his faculties which is experienced by a real student in the acquisition of any new law or principle of nature, is the most direct and vivid reward of his labours. The best and truest, as well as the most rapid progress in study, is made for the sake of that reward. Whoever has once enjoyed it will gladly seize any opportunities of wrestling, as far as his powers permit, with new difficulties, and mastering new ideas.

"It has been said that the happiness of an individual results from the due exercise of his various faculties, and this is surely not least true of the highest faculties of the mind; certainly those who have the power of understanding the wonders of nature derive great happiness from learning to employ it. It is like the pleasure which a man of healthy and vigorous frame experiences in climbing a mountain peak, and in enjoying, in proportion as he rises, a wider and more commanding view of things below."

Our space does not permit us to follow the author into the study of the conditions under which science flourishes, and does most effectively the good which it has to do, and we must content ourselves with a quotation of his description of the usual results which follow a system of special professional pupilage:

"But it often happens (page 15) that a man learns thoroughly the particulars of a business, as practised in some one successful case, and although he has sufficient capital and industrious habits, fails to realise similar results elsewhere.

"For instance, he has learnt and practised the management of a particular farm, and then takes a lease of one in another district. He purchases implements exactly similar to those which he has been using, and gets sheep and cattle of the same breeds. He adopts the same rotation of crops, and spares no pains to make everything go on precisely in the same way as that to which he has been accustomed.

"His first year is unprofitable; but he looks forward hopefully to better results, when things will have got into better working order. But the second and third year only bring more losses, and he is ultimately compelled to give up the farm

"The next tenant is perhaps a man who has learnt the management of an adjoining farm, which happened to be in size, in soil, &c, very much like it. He uses ploughs and other implements which have been found to suit the soil, and gets breeds of sheep and cattle which thrive in that part of the country. He adopts the same rotation of crops and system of manuring which is customary in that district, and carefully imitates what he had seen to succeed, under conditions similar to his own. The result is that he goes on steadily year after year making a fair profit.

"Both of these men were mere servile imitators of what they had seen, and both had been taught to believe that a practical man ought to be nothing more, and that all theories are dangerous. Yet one failed while the other succeeded. We ought not to be surprised at the failure of the one, so much as at the success of the other, which was due to the exceptional circumstance of his finding a farm which admitted of being profitably managed upon exactly the same system."

Men of business, in the opinion of our author, ought to have not only a knowledge of things, but also of principles; and they must be able to use their knowledge of these things and principles for the purpose of bringing about special results; in fact they must have a knowledge of the laws of nature, and skill in the methods of applying that knowledge to experimental purposes. Their power of bringing about the material results from which they derive profit is proportional to the amount of such knowledge and skill which they possess.

And here we must break off our account of an essay which, no doubt, will greatly help to clear up our ideas about scientific and technical education.

ZOOLOGY

Researches on the Amœbæ

The minute masses of protoplasm termed Amœbæ have been recently examined by M. V. Czerny, in relation to their resistance to reagents, and his results have been published in Schultze's "Archiv fur Mikroscopische Anatomie," p. 158. He finds that the power of resistance to the action of solutions of common salt varies considerably in different individuals. In solutions containing one part to four hundred of water none died, but in those containing one to three hundred many died; whilst others, especially the quiescent ones, still lived in solutions containing one per cent., or more. None, however, survived when suddenly placed in a two per cent. solution. It is interesting to observe, however, that these lowly organised beings possess a certain adaptability to external conditions—a power of acclimatisation, as it were, enabling them, if these conditions undergo slow alterations, to accommodate themselves to their new and modified surroundings. Thus, if the strength of the solution were gradually raised, it was found that some Amœbæ could continue to exist in solutions of four per cent. of salt. M. Czerny corroborates the statements already made by Kuhne, that, on exposure to weak saline solutions, Amœbæ thrust forth numerous extremely delicate processes resembling cilia, and that they undergo fission. It is worthy of remark that the partially double contour of the *Amœba bilabiata*, which has led to the admission of a double contoured membrane in this species, is resolved, when examined with a No. 10 Hartnack immersion lens, into a number of closely aggregated, extremely minute toothlets, which, like the stinging cells, cover the whole surface of the body.

M. Engelmann has made some observations on the electrical excitation of the Amœba of the Arcella in the fifth volume of the "Nederlandische Archiv voor geneesen Natuur kunde," p. 28. His investigations were conducted in a moist gas chamber, with unpolarisable electrodes, the stimulus consisting of a single opening induction shock. In *Amœba diffluens*, as Kühne had showed previously, the results of the excitation differ according to whether the individual is in the active or in the quiescent condition. In the former condition, that is, when the animal is elongated or club-shaped and homogeneous, and its protoplasm seems to be flowing continuously in one direction, a very slight stimulus retards or altogether arrests the current, though it speedily recommences, the period of arrest not exceeding at most five seconds. If, however, the stimulus has been a little stronger, in addition to the arrest of the protoplasmic movements, a condensation and shortening of the whole animal occurs, and at a subsequent period perfectly transparent projections form in the anterior part of the body, into which the highly granular protoplasm streams until the original form of the Amœba, which sometimes moves forward with great rapidity, is re-established. The length of time occupied in these changes may amount with tolerably strong currents to about from one to two minutes. If the excitation be applied to the quiescent animal, the protoplasmic movements first cease, the mass assumes the spherical form, but instead of remaining quiescent, it now begins to move from place to place, in which active condition it may remain for a long time. From these experiments, and from others performed upon specimens of Arcella, containing air-vesicles M. Engelmann draws the conclusion that protoplasm, in consequence of electrical excitation, transitorily assumes the mechanical properties of a fluid.

CHEMISTRY

Constitution of Arbutin

HUGO SCHIFF has made some interesting experiments relating to the constitution of arbutin. This substance splits up into glucose and hydroquinone, just as salicin is resolvable into glucose and saligenin (Strecker). The relations between salicin and arbutin may be represented by the following formula:

$$\begin{array}{r}C_6H_7\\ \\C_6H_4\end{array}\left\{\begin{array}{l}O\\(OH)_4\\O\\CH_2 . OH\end{array}\right. \qquad\qquad \begin{array}{r}C_6H_7\\ \\C_6H_4\end{array}\left\{\begin{array}{l}O\\(OH)_4\\O\\OH\end{array}\right.$$

Salicin (Glycosalicyl alcohol) Arbutin (Glycohydroquinone)

The hydrogen in the oxhydryls of the glucosic portion of arbutin may in fact be replaced by acid radicals, just as the author formerly showed in the case of salicin (Zeitschr. (2), v. 1. 52) Moreover, the hydrogen belonging to the hydroquinone in arbutin is easily replaceable; whereas in salicin the hydrogen belonging to the saligenin is not capable of substitution.

Benzoyl-arbutins are obtained by means of benzoyl chloride; *acetyl-arbutins*, with acetyl chloride or acetyl oxide, which act at 60°–80°. The ultimate products of the reaction separated from the resulting solutions, after cooling, by means of ether, contain five acid radicals, and have this composition:

$$\begin{array}{r}C_6H_7\\ \\C_6H_4\end{array}\left\{\begin{array}{l}O\\(O . C_2H_3O)_4\\O\\O . C_2H_3O\end{array}\right. \qquad\qquad \begin{array}{r}C_6H_7\\ \\C_6H_4\end{array}\left\{\begin{array}{l}O\\(O . C_7H_5O)_4\\O\\O . C_7H_5O\end{array}\right.$$

Pentacetyl arbutin. Pentabenzoyl-arbutin

They are colourless bodies, insoluble in water, slightly soluble in ether, more soluble in hot alcohol, from whence they crystallise on cooling in small shining needles. The acid radicals may easily be taken out again by boiling with weak bases. Together with the pentabenzoylated compound, the author likewise obtained *dibenzoyl-arbutin*, in which the hydrogen might be further replaced by acetyl.

Dinitro-arbutin dissolves easily in acetic oxide, and is converted into

$$\text{Pentacetyl-dinitro-arbutin} \qquad \begin{array}{r}C_6H_7\\ \\C_6H_2(NO_2)_2\end{array}\left\{\begin{array}{l}O\\(O . C_2H_3O)_4\\O\\O . C_2H_3O\end{array}\right.$$

which may be separated from the acetic acid solution by water, and crystallised from hot alcohol in fine needles, insoluble in water, slightly soluble in ether. The alcoholic solution heated with sulphuric acid yields glucose, acetic ether, and dinitrohydroquinone, easily recognisable by the splendid colour which it gives with caustic alkalies. Dinitro-arbutin forms with basic lead acetate a crystalline orange-coloured lead-compound, in which the hydrogen of the oxhydryl is replaced by lead. Arbutin gives no precipitate, even with an ammoniacal solution of lead-acetate.

Recently precipitated silver-oxide is reduced, even at ordinary temperatures, by an aqueous solution of arbutin. On adding freshly prepared silver carbonate to a solution of arbutin heated to 50°—60° as long as carbonic acid is evolved, and heating for short time with excess of the silver carbonate, a yellow solution is obtained, which no longer contains arbutin; but on separating the dissolved silver with a few drops of hydrochloric acid, and filtering, a solution is obtained, from which alcohol precipitates white flocks consisting of a compound formed by the union of molecules of arbutin minus 2 atoms of hydrogen, viz.—

$$\begin{array}{c}\text{Diarbutin}\\ \text{or}\\ \text{Glycoquinhydrone}\end{array} \qquad \begin{array}{r}C_6H_7O\\ \\C_6H_4\\ \\C_6H_4\\ \\C_6H_7O\end{array}\left\{\begin{array}{l}(OH)_4\\O\\O\\O\\O\\(OH)_4\end{array}\right. \left.\vphantom{\begin{array}{l}O\\O\end{array}}\right\} = C_{24}H_{30}O_{14}$$

This compound may be regarded as the glucoside of quinhydrone (green hydroquinone); it is related to arbutin in the same manner as helicoidin to salicin. Glycoquinhydrone is not at all bitter; it forms acetyl-derivatives when treated with acetic oxide, and an orange-coloured nitro-product with nitric acid. The latter, when decomposed in alcoholic solution by sulphuric acid, does not yield any substance that turns violet with potash. By means of zinc and sulphuric acid, hydrogen may be again added; and dinitro-hydroquinone thereby produced.

A solution of arbutin produces with ferric chloride a deep blue colour, which gradually disappears. None of the derivatives of arbutin above described exhibit this reaction.

Schiff also finds that *amygdalin* contains seven, and *phlorizin* five oxhydryl atoms, the hydrogen of which may be replaced by acetyl. Hence he assigns to these bodies the following formulæ:

$$\begin{array}{r}C_6H_7\\ \\C_6H_7\\ \\C_7H_6\end{array}\left\{\begin{array}{l}O\\(OH)_4\\O\\(OH)_3\\O\\O\\CN\end{array}\right. \qquad\qquad \begin{array}{r}C_6H_7\\ \\C_6H_3\\ \\C_8H_8\end{array}\left\{\begin{array}{l}O\\(OH)_4\\O\\OH\\O\\CO . OH\end{array}\right.$$

Amygdalin. Phlorizin

(Zeitschr. f. Chem. (2) v. 519. Ann. Chem. et. Pharn. cliv. 237.).

SCIENTIFIC SERIALS

Poggendorff's Annalen, 1870, No. 7. The contents of this Number are:—(1.) "On the effect of Roughness of Surface on the Radiation of Heat," by G. Magnus. The author shows that the generally accepted explanation of the increased emission of radiant Heat by roughened surfaces, that it depends on a diminution of superficial density, is inadmissible. He attributes it to the refraction which takes place at the surface of emission, whereby the direction of the rays which leave the surface obliquely is changed. (2.) "On the Specific Gravity of Alcohol and of Mixtures of Alcohol and Water," by E. H. Baumhauer. A defence of the author's tables of the specific gravity of alcohol against Mendelejeff's criticisms contained in Poggendorff's *Annalen* for 1869, vol. 138. (3.) "On the Flow of Mercury through Capillary Tubes," by E. Warburg. The author's experiments prove that in glass tubes, whose diameter is a sufficiently small fraction of their length, the quantity of mercury which flows through them in a given time is directly proportional to the difference of pressure at the two ends, to the *fourth power* of the diameter, and inversely proportional to the length, but that it is independent of the absolute pressures at the ends so long as the difference remains constant. He concludes from these results that there is no friction, under the conditions of the experiments, between the mercury and the glass, but that the film of mercury in contact with the glass remains at rest while the inner portions flow through it. (4.) "Continuation of Investigations into the Electromotive Force between Liquids," by J. W. Muller. (5.) "On the Determination of the Proportion of Water in Glacial Acetic Acid," by F. Rudorff. The author gives a table for deducing the proportion of water contained in acetic acid from the freezing point of the mixture. He gives 16·7° C. as the freezing point of pure acetic acid (without water), and finds that the presence of ½ per cent. of water lowers the freezing point by more than a degree. (6.) "On the Determination of the Freezing and Melting Points of Fats and other Compounds," by F. Rudorff. The author points out the untrustworthiness of observations of melting points made, as they often are, by heating the substance to be examined in a capillary tube, or by coating the bulb of the thermometer with it. He recommends the observation of the freezing point, with a thermometer whose bulb is actually immersed in the substance, as a means of establishing its chemical identity instead of observing the melting point. To ascertain whether the observed temperature is the highest at which solidification can occur, he notices whether it is accompanied by rise of temperature, which always takes place if the body has been cooled below the normal freezing point. (7.) "On the Phosphorescence of Rarefied Gases after the passage of an Electric Discharge," by E. Sarasin. The author finds that the presence of oxygen, either free, or combined in a compound which is probably decomposed by the discharge, is an essential condition of the occurrence of the phosphorescence, and shows that this phenomenon is probably connected with the formation of ozone. Sulphuric acid vapour favours the production of phosphorescence in a high degree. (8.) "On the Electromotive Forces due to the contact of different metals," by E. Edlund. When an electric current traverses the *point* of junction of two different metals, a quantity of heat is absorbed or produced per unit of time which is proportional to the strength of the current and to the electromotive force acting between the metals. The author refers on this point to a previous paper (Poggendoff's *Annalen*, vol. cxxxvii.); in the present communication he endeavours to estimate the comparative electromotive forces acting between different pairs of metals by the heating or cooling effects of a current of measured strength. The junction formed of each pair to be examined was enclosed in the bulb of an air thermometer, and the difference between the expansions produced, when the current passed in opposite directions, was measured. The electromotive order of the metals deduced from the results did not agree with the order given by electroscopic observations (*elektrische Spannungsreihe*), but it agreed with the thermo-electric order, though the electromotive forces were not found to be proportional in all cases to the thermo-electromotive forces between the same pairs of metals. (9.) "On the Properties of Pictures formed by Photographic Lenses," by Dr. Hermann Vogel. The author calls attention to certain inherent defects of pictures formed by perfect photographic lenses, that is to say, defects not due to distortion or aberration in the lenses. (10.) "On the Velocity of Light in Quartz," by Victor von Lang, contains very careful measurements of the deviations produced by a quartz prism in the ordinary and extraordinary rays for various angles of incidence. Incidentally, a measurement of the ratio of the two coefficients of expansion of quartz is also given, deduced from the change produced by alterations of temperature in the refracting angle of the prism. (11.) "On the Specific Heat of Saline Solutions and Mixtures of Liquids," by A. Wullner. The author disputes, on the authority of the experiments made in his laboratory by Dr. Schuller, Jamin's conclusion that when two liquids are mixed together, and therefore each of them is uniformly diffused through the whole of the space occupied by the mixture, the specific heat of each increases in proportion to increased space occupied by it. (12.) "On the Fusion of Leaden Bullets by striking against an Iron Plate," by Edward Hagenbach. This paper describes the melting of leaden bullets fired against an iron target, and contains a calculation showing that the kinetic energy due to the velocity assigned by "a competent military authority" is sufficient to account for the result. (13.) "An Experiment on boiling together two liquids which do not mix," by August Kundt. If steam is passed into liquid sulphide of carbon, or if sulphide of carbon vapour is passed into water, the resulting mixture of water and sulphide of carbon boils at 42·6° C., that is to say, at a temperature four degrees lower than the boiling point of sulphide of carbon alone. Also, if water and sulphide of carbon, which have been heated separately to between 43° and 46·6°, be mixed together, the mixture boils until its temperature has fallen to about 43°. These facts are in accordance with the observation of Magnus and Regnault that the vapour-tension of a mixture of two mutually insoluble liquids is equal to the sum of the vapour-tensions of the separate liquids. (14.) "On Microscopic Tridymite," by Ferdinand Zirkel. The author describes the characters of this mineral as seen under the microscope, and shows that it is of frequent occurrence in microscopic crystals. (15) "On Acoustical Attraction and Repulsion," by K. H. Schellbach, contains experimental proofs of the statement that "the sonorous vibrations of an elastic medium urge specifically heavier bodies towards the centre of disturbance, and specifically lighter bodies away from it."

Palæontographica. Beiträge zur Naturgeschichte der Vorwelt. Herausgegeben von Dr. W. Dunker and Dr. K. A. Zittel. Band xvii., Lief. 6, 1870. This new part of the well-known "Palæontographica" contains an interesting contribution to fossil entomology in the description of the species of diptera obtained from the brown coal of Rott in the Siebengebirge. It is from the pen of the distinguished entomologist, L. von Heyden. The species, which are figured, are forty-one in number, belonging to sixteen genera, and all but nine of them belong to the moisture-loving families of the monocerous group (*Tipulidæ*, *Culicidæ*, &c.). Of *Chironomus* there are five well-marked species, and no less than six different forms of larvæ and pupæ, and there is also the larva almost certainly of a species of *Stratiomys.*

The most important article in the *Journal of Botany* for December is a continuation of Dr. Braithwaite's Recent Additions to our Moss Flora, accompanied by two plates. Dr. Seemann continues his Revision of the Natural Order *Bignoniaceæ*, and Mr. Ernst gives Jottings from a Botanical Note-book, relating chiefly to Caracas plants. The other articles belong exclusively to specific British botany. With the new year it is intended to increase the amount of type in the journal by about one-third, without any corresponding increase in price.

SOCIETIES AND ACADEMIES

London

Zoological Society, December 6.—Robert Hudson, F.R.S., V.P., in the chair. The Secretary read a report on the additions to the Society's menagerie during the months of October and November, amongst which particular attention was called to an example of Geoffroy's Cat (*Felis Geoffroyi*), from Paraguay, purchased Oct. 10, and a specimen of the Antarctic Wolf (*Canis antarcticus*), from the Falkland Islands, presented by Mr. H. Byng, Acting Colonial Secretary of that colony.—An extract was read from a letter received from Dr. R. C. Cunningham, giving particulars of the habits of a Manatee, as observed by him in the public gardens at Rio.—A ninth letter was read from Mr. W. H. Hudson, on the Ornithology of Buenos Ayres.—Dr. J. Murie read the second part of his memoir on the anatomy of the Sea Lion (*Otaria jubata*), as observed in the male of this species which died in the Society's Gardens in 1867.—Mr. J. B. Perrin read a paper containing notes on the anatomy of the Smaller Fin-Whale *Balænoptera rostrata*), as observed on dissection of a young female specimen of this species captured at Weymouth in April, 1870.—A communication was read from Dr. G. Hartlaub and Dr. O. Finsch, containing the description of a remarkable new Finch from the Navigators' Islands, proposed to be called *Lobiospiza notabilis.*—A communication was read from the Rev. O. P. Cambridge, containing notes on a collection of *Arachnidea* made by Mr. J. Keast Lord in the Peninsula of Sinai and on the African borders of the Red Sea.—A paper was read by Mr. G. Gulliver, F.R.S., containing observations on certain points in the anatomy and economy of the Lampreys.—Dr. A. Günther read a notice of the hitherto unrecorded occurrence of *Lates calcarifer*, a fish belonging to the Perch family, in Australia.—A communication was read from Dr. J. E. Gray, containing the description of the skull of the adult *Eupleres gondoti.* This Madagascar mammal was previously only known from an immature specimen in the Paris Museum.—A second communication from Dr. Gray contained notes on *Hapalemur simus*, a new Lemur, described from a specimen lately living in the Society's Gardens.—Messrs. Sclater and Salvin communicated descriptions of five new species of birds from the United States of Columbia.—A second communication from the same authors contained an account of the collections of birds recently made by Mr. George M. Whitely on the line of the Inter-Oceanic Railway of Honduras.—Mr. Sclater read descriptions of three apparently new species of Tyrant Birds, of the genus *Elainea*, to which were added remarks on other known species of the same group.—Mr. St. George Mivart read a paper on the myology of a species of Chameleon (*Chameleon parsonii*).—Mr. Gould exhibited and pointed out the characters of two new species of Humming Birds recently collected by Mr. Buckley in Ecuador, which he proposed to call *Chætocercus bombilius* and *Thalurania hypochlora.*

Anthropological Society, December 6.—Dr. J. Beddoe, President, in the chair. Mr. W. R. Cooper exhibited and shortly described two Græco-Egyptian terra-cotta figures from the Hay Collection, showing a remarkable form of the head.—A paper was read by Mr. A. L. Lewis. "Suggestions and Reflections respecting the Peoples inhabiting the British Isles." The author divided the inhabitants of Britain into three leading types: 1st, the Kymric, long-headed, dark-haired, and light-eyed; 2nd, the Iberian, dark-haired and dark-eyed; 3rd, the Teutonic, broad-headed, light-haired, and light-eyed; the first two types being included under the collective name of Celt. After touching on some of the physical racial questions connected with the intermixture of these types, the paper concluded with some remarks tending to controvert certain popular ideas in reference to their mental characteristics, and their respective love of freedom, honesty, and chastity.

Entomological Society, Dec. 5.—Mr. A. R. Wallace, President, in the chair. Mr. Edward Saunders exhibited three new British *Hemiptera*, belonging to the genera *Salda*, *Plociomerus*, and *Hadrodema*. Mr. F. Smith exhibited *Baridius scolopaceus*, a beetle new to Britain, also *Calodera rubens*, both species captured in Kent. Mr. Butler exhibited a dark dwarf of *Vanessa urticæ*. Mr. Pascoe exhibited two new forms of *Longicornia* from the Himalayas. Mr. Albert Muller exhibited photographs of galls caused by several species of *Cynips*, sent by Mr. Bassett, of Waterburg, U.S.A. Mr. S. S. Saunders exhibited a living spider, *Eresus ctenizoides*, from Syra, where it lived under stones and fed on large grasshoppers; it had remained without food since July. The paper read was "A Monograph on the *Ephemeridæ*," by the Rev. A. E. Eaton. Mr. G. H. Verrall was elected a member of the Society.

London Institution, December 1.—Prof. Morris delivered a lecture "On Gems and Precious Stones," in which the characters of the various mineral substances used in jewellery were minutely explained. The diamond, the only representative among the gems of the elementary bodies, received special attention. The lecturer referred to its crystalline form, cleavage, hardness, specific gravity, and refractive power, the characters by which it is distinguished from crystallised quartz and other minerals. He described the dull and unattractive varieties of the diamond known as "carbonado" and "boort," and pointed out their application to steel-engraving, glass-cutting, and rock-boring. The mineralogical and geological features of the diamond-beds of India, Brazil, Borneo, South Australia, and South Africa, were discussed at length, and the frequent association of diamonds with itacolumite, gold, and rutile was referred to as a subject worthy of careful investigation. Other precious stones, such as the sapphire, ruby, emerald, beryl, topaz, jargon, garnet, spinel, and turquoise, were successively treated of, reference being made to their chemical composition, their physical properties, and their application to decorative and industrial purposes. To illustrate the lecture, Messrs. Blogg and Martin contributed a unique series of uncut diamonds exhibiting perfect crystalline forms, diamonds from South Africa, and one remarkable specimen embedded in the "cascalho," taken from a bed in Brazil. Prof. Tennant also contributed a splendid collection of diamonds in the natural state. Through the kindness of Messrs. Hunt and Roskell, the lecturer was enabled to show a fine series of precious stones and models of the great South African diamond before and after cutting. To Mr. James Gregory again, the lecturer was indebted for a collection of minerals used for ornamental purposes, models of celebrated diamonds, and samples of the gravels and rocks associated with the diamonds in South Africa.

December 5.—Dr. Odling gave his sixth lecture "On Chemical Action," and illustrated his remarks on the circumstances which modify chemical action by a series of brilliant experiments, in which the oxy-hydrogen blowpipe was largely used.

Chemical Society, December 1.—Prof. Williamson, F.R.S., President, in the chair. The following gentlemen were elected Fellows:—H. E. Armstrong, Ph. D., R. Barklie, W. L. Carpenter, T. M. Crafts, Prof. of Chemistry in Cornell University, J. Dewas, T. Farries, R. Mallet, F.R.S., and Dr. Ogg. Mr. Perkin, F.R.S., read a paper "On some derivatives of Anthracene." This was a detailed account of some Anthracene derivatives, more particularly of the products resulting from the action of sulphuric acid upon dibrom and dichloranthracene. Dichloranthracene is most conveniently prepared by passing chlorine gas over benzole, holding about one-fifth its weight of purified commercial anthracene in suspension, until the mixture becomes a crystalline mass. The product is then brought on to a linen filter, drained, washed with cold benzole, dried, and then further purified by distillation and subsequent recrystallisation from benzole. Thus obtained it appears in golden yellow needles. The mean of several analyses gave 67·91 per cent. C, 3·34 per cent. H, and 28·70 per cent. Cl, which numbers agree perfectly with the formula of Graebe and Liebermann, $C_{14}H_8Cl_2$. Dichloranthracene, when greatly heated, sublimes in beautiful needles, which may be obtained of considerable size. It is fluorescent in the solid state as well as when in solution. When a boiling solution of dichloranthracene in benzole is added to a similar solution of picric acid, the mixture assumes a dark orange-red colour, and on cooling becomes filled with small bright red needles. These consist of a compound of dichloranthracene and picric acid. A determination of the dichloranthracene in this body gave numbers closely approximating to those required by the formula, $C_{14}\,H_8\,Cl_2,\ C_6\,H_3\,(N\,O_2)_3\,O$. Dibromanthracene. This product was prepared by Graebe's process. It was, however, purified first by distillation and then by crystallisation from benzole. Thus obtained, it is of a golden yellow colour. It gave, on analysis, numbers closely agreeing with those required by the formula

$$C_{14}\ H_8\ Br_2$$

Like dichloranthracene, this body produces a beautiful red compound with picric acid. Action of Sulphuric Acid on Dichloranthracene. Dichloranthracene, when submitted to the action of fuming sulphuric acid, dissolves, forming a bright green solution, and is at the same time converted into a sulpho-acid. To prepare this acid, one part of dichloranthracene is added to about five parts of fuming sulphuric acid, and the mixture heated for a short time in the water bath. It is then gradually poured into several times its bulk of water and treated with carbonate of barium until all the sulphuric acid is neutralised. The acid solution, when filtered off from the sulphate of barium, is evaporated to a small bulk. When sufficiently concentrated, it becomes, on cooling, a shiny mass of minute orange-yellow coloured crystals, which may be drained on a porous tile. This acid has not been analysed, but, from the composition of its salts, evidently possesses the formula

$$C_{14}\ H_6\ Cl_2\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.$$

Mr. Perkin therefore proposes to call it disulphodichloranthracenic acid. It is easily soluble in water from which it is precipitated upon the addition of a little concentrated sulphuric or hydrochloric acid. It possesses a strongly acid taste and character. The acid forms salt with sodium, barium, calcium, and strontium. The barium salt is remarkable for its insolubility in hydrochloric acid.

Dibromanthracene yields with strong sulphuric acid an analogous disulphodibromanthracenic acid,

$$C_{14}\ H_6\ Br_2\left\{\begin{matrix}HSO_3.\\HSO_3.\end{matrix}\right.$$

Its sodium, barium, &c., are similar to the salts of disulphodichloranthracenic acid.

Oxidation of disulphodichloro, and disulphodibromanthracenic acid. These sulpho-acids, when subjected to the influence of oxidising agents, rapidly decompose, exchanging their chlorine or bromine for oxygen, and are thus converted into disulphanthraquinonic acid.

$$\underbrace{C_{14}\ H_6\ Cl_2\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.+O_2}_{\text{Disulphodichloranthracenic acid.}} = \underbrace{C_{14}\ H_6\ (O_2)''\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.+Cl\ Cl}_{\text{Disulphanthraquinonic acid.}}$$

$$\underbrace{C_{14}\ H_6\ Br_2\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.+O_2}_{\text{Disulphodichloranthracenic acid.}} = \underbrace{C_{14}\ H_6\ (O_2)''\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.+Br\ Br}_{\text{Disulphanthraquinonic acid.}}$$

An analogous result is also obtained by treating them with concentrated sulphuric acid, the following reactions taking place:—

$$C_{14}\ H_6\ Cl_2\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.+H_2SO_4=C_{14}H_6(O_2)''\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.+2\ HCl+SO_2$$

$$C_{14}\,H_6\,Br_2\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.+2\ H_2\,SO_4=C_{14}\,H_6\,(O_2)''\left\{\begin{matrix}HSO_3\\HSO_3\end{matrix}\right.+Br_2+2\ SO_2+2\ H_2O.$$

Anthracene when pure in large crystals shows a beautiful fluorescence, and so do many of the anthracene products, though curiously their solutions are comparatively poor in this respect. Anthracene and dichloranthracene in the state of vapour are not at all fluorescent, and moreover, a ray of light sent through the length of about four inches of the vapour of either body, still retains its power of rendering fluorescent bodies luminous. The experiments in this direction are, however, not yet concluded. On sealing up anthracene in a long vacuum tube with platinum poles, and allowing the discharge from an induction coil to pass through the tube, nothing particular is observed except the beautiful fluorescence of the crystals of anthracene. On examination with the spectroscope, the light showed carbon and nitrogen lines, the latter arising from the presence of a little air in the tube. Upon heating the tube, however, somewhat strongly, so as to volatilise the hydrocarbon, the ordinary colour of the discharge changed to a magnificent deep azure blue, and what is remarkable is that this blue light, when examined with the spectroscope, is perfectly continuous, and consists of blue with a little green. Dichloranthracene, when heated in a similar manner, gives an analogous result, but suffers a good deal of decomposition, anthracene changing but little. These curious results do not appear to be due to the fluorescent character of the substances employed, as napthaline produces a similar effect, the blue light, though not so intense, being continuous. It must be observed, however, that this hydrocarbon undergoes considerable change, becoming brown and oily. Anthracene heated in a vacuum tube in the same way gives a greenish blue light, showing faint carbon bands. On exposing a solution of disulphodichloranthracenic acid to the light of one of the recent displays of the aurora borealis it was very strongly illuminated, as might be expected. Moonlight, on the other hand, had no perceptible effect upon it, nor yet an alkaline solution of esculine. Mr. Perkin illustrated his interesting communication by a series of most beautiful experiments.

VIENNA

Imperial Academy of Sciences, October 20.—Dr. L. Manol communicated a memoir on chest and head voice, in which he described the condition of the glottis during the production of these two kinds of sounds.

November 3.—Prof. E. Stahlberger transmitted a memoir on the ebb and flow at Fiume.—Dr. Reuss presented a memoir on the Foraminifera of the Septaria clay of Pietzpuhl, containing the determination of the species figured by M. von Schlicht. Pietzpuhl possesses the richest Foraminiferous fauna of any known locality for the Septaria clay; the author has distinguished 164 species and twenty varieties, the total number found in the formation being 244 species.—Dr. C. Jelinek exhibited and explained a new anemometer, constructed for the station at Lesina, by Hipp, of Neuchatel.—Dr. T. R. von Oppolzer communicated a memoir on Winnecke's periodic comet, in which he endeavoured to show that this comet presents no extraordinary anomalies in its movement. This memoir also contained an account of the author's method of calculating disturbances.

November 10.—Prof. J. Gottlieb transmitted a chemical analysis of the Königsbrunnen at Kostreinitz, in Lower Styria, and a memoir by M. A. F. Reibenschuh, containing the analysis of the Johannesquelle, near Stainz, in Merau.—Prof. Loschmidt communicated a continuation of the results obtained by M. A. Wretschko in his researches on the diffusion of gaseous mixtures.

I. R. Geological Institute, Oct. 30.—Baron v. Richthofen, in a letter dated Pekin, July 20, gives a notice of his recent geological explorations in China. On the first of January he started from Canton and travelled through the provinces of Kwangtung and Hunan to Hankan, and then, through Hupe, Honan, and Shansi, to Peking. The most important result of this journey is the discovery of the enormous extension of coalfields and iron-ores in the province of Shansi. The southern half of this province, about 1,500 German square miles, and probably also the northern half, is an almost continuous coalfield, containing anthracite of the best quality, in layers of from twelve to thirty feet in thickness. The anthracite district is much more extensive than that of Pennsylvania, and offers incomparably more favourable conditions for working. Together with the coal, iron ores of very good quality are found in abundance. —M. Th. Fuchs gives a sketch of different discoveries in the tertiary basin of Vienna which he made last summer, in company with M. F. Karrer. The building of the new aqueduct for Vienna has caused denudations near Baden, which prove clearly that the marine clay (Tegel) of Baden overlies the Leytha limestone. Between the Cerithium (Sarmatic) beds and the overlying Congeria beds, they discovered in many localities a thin stratum, which contains the fauna of both these formations mixed, without any sensible difference in the form or size of the various species. M. E. Tietze has explored the Jurassic and Liassic strata in the southern Banat, in the environs of Bersgasyka. He found that large masses of white and red limestones, which belong to the tithonic age, immediately cover the famous Ammonite bed, near Swinitga, which has long been known as belonging to the middle Jurassic formation. Farther down are developed different members of the lias, which contain considerable layers of coal.—M. G. Stache, during the summer, was occupied with the exploration of the central crystalline masses in eastern Tyrol, chiefly in the environs of the Ziller Valley. He brings full evidence that metamorphic stratified rocks, partly even with traces of organic remains, play a considerable part in the composition of the large mountain masses of that country.

GOTTINGEN

Royal Academy of Sciences, October 19.—M. W. Krause read a paper on the termination of the nerves in the tongue of man; and M. P. Gordan a memoir on the partial differential equations, of which the resultant R satisfies a form of the n^t degree and a form of the m^t degree.

November 12.—M. R. Lipschitz communicated contributions to the theory of the reversal of a function system.—A paper was also read by Dr. R. von Willemoes-Suhm on a *Balanoglossus* from the North Sea. This paper contained the description of a third species of the genus discovered by the author in the Oeresund near Helleback, in Iceland. He names the worm *B. kupfferi*, and dredged it up from a depth of 12 to 16 fathoms in a bottom of fine mud.

November 16.—A paper on asymptotic lines, by M. A. Enneper, was read.

BOOKS RECEIVED

ENGLISH.—Use and Limit of the Imagination in Science: Prof. Tyndall (Longmans and Co.).—The Intelligence and Perfectibility of Animals: G. G. Le Roy (Chapman and Hall).—The Wild Garden: W. Robinson (J. Murray). Lessons in Elementary Physics: Prof. B. Stewart (Macmillan and Co).—Chemical Problems: T. E. Thorpe (Macmillan and Co).—The Modern Men of Letters: J. H. Friswell (Hodder and Stoughton).—One Thousand Gems: H. W. Beecher (Hodder and Stoughton).

DIARY

THURSDAY, DECEMBER 15.

ROYAL SOCIETY, at 8.30.—Report on Deep-Sea Researches carried on during the months July-September, 1870, in H.M. Surveying Ship *Porcupine* (conclusion): Dr. Carpenter, F.R.S., and J. Gwyn Jeffreys, F.R.S.—On the Constitution of the Solid Crust of the Earth: Archdeacon Pratt, F.R.S.—Actinometric Observations made at Dehra and Mussoorie, in India: Lieut. Hennessey.

SOCIETY OF ANTIQUARIES, at 8.30.—On the Pre-Christian Cross: Mr. H. M. Westropp.

LINNEAN SOCIETY, at 8.—On Sabadilla from Caracas (*Asagræa officinalis* Link.): A. Ernst.—A letter on the Californian Pitcher-plant (*Darlingtonia*), W. Robinson, F.L.S.

CHEMICAL SOCIETY, at 8.—On some New Derivatives of Coumarin: Mr. W. H. Perkin.

LONDON INSTITUTION, at 7.30.—On Count Rumford and his Philosophical Work: Mr. W. Mattieu Williams.

MONDAY, DECEMBER 19.

LONDON INSTITUTION, at 4.—On Chemical Action: Professor Odling, F.R.S.

TUESDAY, DECEMBER 20.

ANTHROPOLOGICAL SOCIETY, at 8.—Archaic Structures of Cornwall and Devon: Mr A. L. Lewis.—Objections to the Theory of Natural Selection: Dr. Muirhead.—The Manx of the Isle of Man: Dr. Richard King.—The Anthropology of Lancashire: Dr. Beddoe.

STATISTICAL SOCIETY, at 7.45.—On Wool Supply: Mr A. Hamilton.

WEDNESDAY, DECEMBER 21.

GEOLOGICAL SOCIETY, at 8.—On the older Metamorphic Rocks and Granite of Banffshire: Mr. T. F. Jamieson.—On Lower Tertiary Deposits recently exposed at Portsmouth: Mr. C. J. A. Meyer.—On the Chalk of the Cliffs from Seaford to Eastbourne, Sussex: Mr. W. Whitaker.—On the Chalk of the Southern Part of Dorset and Devon: Mr W. Whitaker.

SOCIETY OF ARTS, at 8.—On a Method of Lighting Towns, Factories or Private Houses by means of Vegetable or Mineral Oils: Mr. Albert Silber.

ROYAL SOCIETY OF LITERATURE, at 8.30.—On a passage in *Othello* (by the late Rev. W. W. Berry): Dr. C. M. Ingleby, For. Sec. R.S.L.—On the Great Seals of William the Conqueror: Mr. Walter De Gray Birch.

THURSDAY, DECEMBER 22.

ROYAL, at 8.30.

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THURSDAY, DECEMBER 22, 1870

NATURAL HISTORY SOCIETIES

II.

It must not be supposed that we expect, from Societies which have been but recently established, works of the importance or the completeness of those which we have mentioned* as emanating from the naturalists of Northumberland and Durham; or even such volumes as those annually issued by the Woolhope Club. It must be borne in mind that it is by no means necessary, or even advisable, that a young society should do much, or even anything, in the way of publication. The experiment of a local magazine has been tried in connection with more than one body, but in each case the results can scarcely be considered satisfactory. The Liverpool naturalists issued monthly such a publication—at first in lithograph, afterwards in print—which contained papers on subjects of general as well as of local interest. The High Wycombe Natural History Society started a similar magazine, on a similar plan, but issued quarterly instead of monthly; this continued for four years, but is now among the "things that are not." The Folkestone naturalists tried a like publication and with a like result; one year was sufficient to bring it to a close.

The failure of these periodicals is scarcely a thing to be regretted. The only scientific value of such local publications, it cannot be too often repeated, lies in the prominence given to local Natural History. It is not to be supposed that people even now, much less in the future, would refer to them for information—say on the Darwinian theory, or any other matter of general importance and interest—which would be found in fuller detail in magazines of wider circulation. Nor are popular descriptions of plants or insects of any greater value; and a physiological discovery of any importance should be communicated to some one of the many journals now open to naturalists. If such a discovery is recorded in a local publication, the chances are that it will be overlooked by the majority—simply because it is not in its proper place. The plea that Natural Science may be rendered more popular by such periodicals is worth but little; there are other magazines which, while wisely excluding local lists, are at once readable and scientific, and to them people really anxious to learn will turn for information.

The Folkestone Natural History Society has made a step in the right direction by publishing this year "A List of Macro-Lepidoptera occurring in the neighbourhood of Folkestone." This list occupies twenty-four pages, and is sold in a wrapper for 6*d.* As far as we know, this is the first of its kind; other bodies have issued local lists with their annual reports, but we have none published separately at a low price, like this of the Folkestone Society. The example is one which we hope will be followed; the funds of even a small field-club would be adequate to cover the expenses of printing, and the actual value to science of such a list is far greater than that of any number of local magazines containing papers of "general interest." The same society has in hand a list of the flowering plants of the district on the same plan, which will probably appear next year. The Belfast Naturalists' Club has this year issued with its report the first of a series of local lists; this has already been noticed in NATURE.

The system of dividing the labour of list-making, which is now coming into general use, may be regarded as a modification of the mode of working by sections, which we noticed in a previous paper. One or more members engage to superintend the making-out of some one list of the mammals, birds, lepidoptera, coleoptera, mollusca, fossils, phanerogams, or cryptogams. Each of these branches is, of course, capable of subdivision; and, where there are many workers, such subdivision is advisable. The Westmoreland naturalists have deputed one of their number to collect and examine the Batrachian Ranunculi of the district; and the investigation of certain "critical" genera and species might well occupy a careful observer for a considerable period. But we would lay especial stress upon the necessity of posting up regularly every scrap of information; books should be kept for the purpose, in which every discovery should be entered. By this means, and by this means only, can the actual state of the knowledge of any branch be ascertained; and it is easy to arrange for publication when required notes thus conveniently brought together. Whether for publication or not, however, and no matter how few and incomplete the records may be, it is the first duty of every Society, great or small, to register all its observations, not only for present benefit, but for the use of those who come after.

Although we would not urge upon every Society the "rushing into print," which is nowadays too common, we think we have reasonable grounds of complaint against one or two bodies, which have been established for some years, and number many members, in that they have not published more regarding the results of their labours. We may instance as an example the Manchester Field Naturalists' Society; and, as a notice of one or two of the more popular clubs falls within the scope of this paper, we may take this as a favourable specimen of them. Established ten years ago, chiefly through the energy of Mr. Leo H Grindon, assisted by a few other Manchester naturalists, this Society soon counted its members by hundreds. At one time about five hundred and fifty names were enrolled, and, at the present date, between two and three hundred appear on its list. The work of the Society consists in the investigation, by means of excursions during the summer session, of the Natural History of the neighbourhood. These excursions are largely attended, and after the ramble the members assemble at tea, when addresses on scientific subjects are given. During the winter occasional *soirées* are held, which are rather too much like evening parties, with a little dilettante science thrown in; the room being decorated, extensive "toilettes" abundant, and instrumental music performed during the evening. The scientific "halfpennyworth of bread" to this "intolerable deal of sack" is provided by an exhibition of objects on a large scale, and the delivery of a paper or two, or a lecture, at some time during the proceedings. Of course, neither excursions nor *soirées* can be got up on this large scale without a proportionate outlay; and the result of this is that, although the annual subscription is 10*s.* 6*d.* —much too high, by the way, for such a body—the balance in hand, when all expenses are paid, is inconsiderable. We do not wish to be misunderstood when we express our opinion, founded upon personal observation, that a large proportion of those who attend the summer excursions look upon them as merely pleasant afternoon rambles, and that of those who patronise the *soirées*, many go as they would to any other entertainment, without the slightest interest in Natural Science. There are good workers at Manchester—men, who from early youth have been strongly imbued with a love of nature, and who have done, and are doing, good service to science; and to their influence must be attributed the good effects which such a body certainly produces in spite of drawbacks. But, under the circumstances, it appears to us that instead of the somewhat lengthy, though interesting report, which is issued annually, containing detailed notices of the localities visited and papers read, which will be of little value to posterity, lists of the natural productions of the district, similar in plan to that published by the Folkestone Society, should be issued. One such list—that of the Mosses, by Mr. G. E. Hunt—appeared in 1864; and if only on the ground of showing that some real work is done, and of allowing the workers "fair play," a portion of the funds should certainly be devoted to the placing on permanent record, for the benefit of future observers, complete lists of the flora and fauna of the district. More especially at Manchester is such a record needed, inasmuch as the Committee advocate the establishment, "in places where they are likely to become permanently established," of plants foreign to the district, and of freshwater mollusca. This suggestion appears to us, under any circumstances, unadvisable; and unless some record is kept of such introductions, it must, if carried out, prove both misleading and injurious.

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* See NATURE, Vol ii. p. 469.

Those who contemplate the establishment of a field-club on a small scale will do well to content themselves with moderate beginnings. The subscription should be low—say 2*s*. 6*d*.; patronage should not be sought, but rather discouraged; and every pains should be taken to show that no class distinctions would be considered of the slightest importance in matters connected with Natural History. In one society, with which we were intimately connected, we well remember the difficulty we had to convince an intelligent working man, with a genuine love for science, that he was a welcome addition to our ranks; and it is only by judicious management that such can be induced to co-operate with those who are considered their superiors.

The High Wycombe Natural History Society, another of those which has aimed at popularising science, holds monthly evening meetings during the winter at the houses of some of the principal members. These meetings partake somewhat of the nature of a *conversazione*; tea and coffee are handed round, papers are read, objects displayed, and the evening concludes with an exhibition of the microscope. Despite all care, however, the intention of these gatherings has been somewhat lost sight of, and they have grown to be looked upon as mild forms of dissipation. To remedy this, the plan is to be adopted of holding fortnightly, between the general meetings, instruction classes, which only those who are willing to work are expected to attend. Each of these will last from an hour to an hour and a half, and be devoted to some one special point; and the benefit resulting from this arrangement seems likely to be considerable.

A great point to be remembered by those who are engaged in organising a local Society is that it is by no means to be desired that a large number of members should be enrolled; in fact, when once the body is established, and its existence generally known, we would solicit no one to join it. Those who really care for the thing, and are therefore likely to be useful members, will come forward readily enough to lend a helping hand; but those whose assistance has to be sought will probably be of but little use, even if it be obtained.

The subject of local museums in connection with Natural History Societies is now exciting some attention; and we have intelligence of the recent establishment of one at Folkestone upon what seems to be a satisfactory basis.

THE PHYSIOLOGICAL LABORATORY AT LEIPZIG*

SINCE in England we have absolutely no physiological laboratory open to students, an account of the best in Germany (there are many others) will be interesting to the public. Perhaps some day the University of Oxford may think it desirable to erect such a laboratory to match that recently provided for Experimental Physics. There is plenty of money which the colleges could use for this object, if once they were freed from the old restrictions by the aid of the Government.

"The physiological laboratory, where I am at present working, owes its existence to the energy of Prof. Carl Ludwig, and to the liberality of the Government of Saxony. As it is universally acknowledged to be the most complete establishment of the kind in Europe, it seems to me to merit a somewhat detailed description. The building, as may be seen by the annexed plan, has somewhat the form of a horseshoe, with a small projecting portion in the middle, where the lecture-room is situated. The dimensions are—on the north and south sides, 119ft. 2in.: on the east side, 121ft. 11in. The right wing of the building constitutes the microscopical department, the left the chemical department of the laboratory, while the central portion is devoted to the study of experimental physiology in the narrower sense of the word.

"To describe the rooms more minutely. Room A is arranged for the accommodation of beginners in the study of microscopy, and is furnished with boxes that contain the microscopes, and a large ground-glass tablet, by means of which the lectures on microscopy are illustrated with drawings in coloured chalk. Room B is the private study of the assistant in microscopy. Room C is intended for more advanced students in microscopy, and contains an injecting apparatus, by means of which three different fluids can be injected simultaneously under any required pressure and for any length of time, while the injection mass and the tissue to be injected are heated over a water bath. Room D contains a small library, consisting of such books as are most needed for constant reference. Room E is furnished with glass cases, in which physiological apparatus is kept when not in use. As a rule, no experiments are performed in this room. Rooms F, G, and H are devoted to experimental physiology, and are furnished with operating tables, with bellows attached for keeping up artificial respiration on curarised animals, registering apparatus of various sorts for recording the pressure of the blood, water baths where any required temperature may be kept up indefinitely, an injecting apparatus like that in the microscopical department, evaporating closets, glass cases for apparatus, &c. Between Rooms G and H is a small closet arranged for observations with the spectroscope. Room I is the chamber where all experiments are performed which require the use of large quantities of quicksilver. It contains two quicksilver pumps for extracting gases from fluids, instruments for measuring the activity of the respiration in man and the lower animals, &c. Room J is divided into two portions, one of which is used for a weighing room, and the other for experiments in acoustics. Rooms K and L contain, besides the ordinary furniture of chemical laboratories, the ingenious air-pump of Bunsen, by which the process of filtering is so greatly accelerated. The lecture room, M, accommodates about one hundred students. Tables running on a small railroad in front of the seats, enable the lecturer to demonstrate his experiments very conveniently. The room is lighted from above as well as from the side, and if necessary, can be darkened completely for optical experiments. In the basement of the building is a small gas-engine of about one-horse power, which drives the respiration apparatus, registering instruments, &c. In the basement are also the rooms where the animals are kept (one room being devoted entirely to frogs), a chamber furnished with refrigerators for performing chemical experiments, where a low temperature is required, a chamber containing furnaces for fusion, a workshop, store-rooms, &c.

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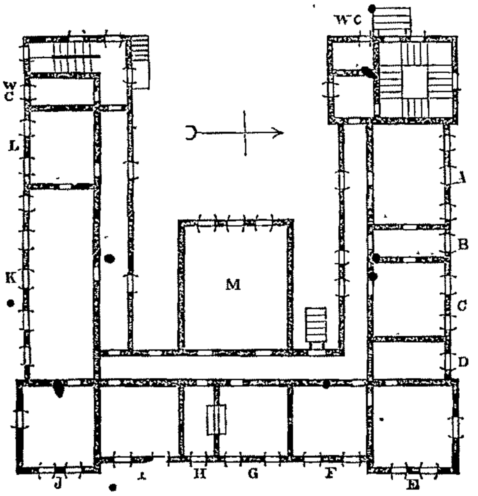
* From the *Boston Medical and Surgical Journal*. Letter from Dr. H. P. Bowditch.



"The second story of the building contains the rooms of Prof. Ludwig and his family, and those of other persons connected with the laboratory. In the court-yard is a small building containing the necessary arrangements for experimenting on horses and other large animals. Here, also, are an aviary and a small fish-pond.

"Besides the permanent and stationary apparatus already described, the laboratory is well supplied with all sorts of instruments for physiological experiments, and new apparatus is constantly ordered for special investigations. There is also a very skilful mechanic living in the laboratory, whose duty it is to make alterations or repairs in the apparatus as circumstances may require.

"Prof. Ludwig directs personally all the work done in the laboratory, devoting his whole time to the superintendence of his pupils, and making no independent investigations. Each of the pupils, at present nine in number, makes, under the direction of the Professor, a series of experiments with a view of settling some special point in physiology. The results arrived at are published at the end of the year, sometimes under the names of the Professor and pupil together, and sometimes under that of the pupil alone. The whole work of the laboratory forms every year a pamphlet of 150 to 250 pages.

"Prof. Ludwig lectures five times a week on physiology, and his assistants, viz., Prof. Schweigger-Seidel in microscopy, Dr. Hufner in chemistry, and Dr. J. J. Muller in physics, also lecture on their specialties, besides superintending the work done in their respective departments.

"It will thus be seen that abundant facilities are here offered, not only for learning the existing state of physiological science, but also for becoming familiar with the manner in which physiology is at present studied in Germany. The patient, methodical, and faithful way in which the phenomena of life are investigated by the German physiologists not only inspires great confidence in their results, but encourages one in the hope that the day is not far distant when Physiology will take its proper place as the only true foundation of Medical Science.

"H. P. BOWDITCH."

Dr. Bowditch adds to this in a private note that *all* expenses, even down to the frogs used for experiments, are borne by the Saxon Government; so that the institution is absolutely free of charge to the student. Professor Ludwig welcomes to the laboratory any student—provided there is room for him—whether German, English, French, or Russian, who is desirous and capable of original investigation.

PALÆONTOLOGY OF MAN

Précis de Paléontologie Humaine. Par le Docteur E. J. Hamy. 8vo. (Paris, 1870. London: Williams and Norgate.)

M. HAMY'S Palæontology of Man, written with the view of bringing the results of recent discovery to bear on the antiquity of our species, is a most important contribution to the rapidly increasing literature of prehistoric archæology. It is intended to serve as an appendix to Sir C. Lyell's great work on the subject, and treats only of palæolithic man to the exclusion of the three newer prehistoric ages. M. Hamy has classified his materials with judgment and caution, and has collected into a small compass most of the statements on record of the existence of man in the geological past, with a running criticism, which sometimes admits, and at other times rejects, the testimony. He stands almost alone among his countrymen in attaching no importance to the reputed discovery of the famous Moulin Quignon jaw, and in allowing that the circumstances under which it was found were, to say the least, very equivocal. His book, in a word, is so good that I propose to draw attention to a few of the weak rather than the strong points. Among the latter, the first chapter, which treats of the employment of stone implements in the religious ceremonies of the ancients, and gives the history of "thunderbolts," is perhaps that which especially demands the notice of the English reader.

The evidence adduced by M. Bourgeois of the discovery of flint flakes and scrapers in the Miocene strata of Thenay, along with remains of the hornless rhinoceros and mastodon, proves, according to M. Hamy, that man was an inhabitant of Miocene Europe. It is, however, rejected by most of the French and English *savants*, because M. Bourgeois has not shown that the implements in question may not have been derived ultimately from the surface of the ground, where they are very abundant. While M. Hamy acknowledges this to be the case, he does not see its full bearing on the value of the testimony. The implements probably are of Quaternary, or even of post-quaternary age, and certainly cannot be considered decisive of the sojourn of man in Europe during the Miocene epoch, although the climate at the time was almost tropical, and the conditions of life easy. Nor can the evidence of the grooved bones of Halithere, found by M. Delaunay at Puancé in Maine-et-Loire be accepted, because it cannot be proved that the grooves may not have been caused by some other agency than that of man. The proof of the existence of man in Europe during the Pliocene epoch derived from the striæ in the fossil bones found at Saint Prest and in the valley of the Arno, accepted by M. Hamy, is equally unsatisfactory. The flint "arrow-head" (fig. 25) and other rude fragments said to have been obtained at the former place from the same horizon as the bones of *Elephas meridionalis*, by M. Bourgeois, the stout champion of Miocene man, do not afford the precise and exact testimony which is demanded for the establishment of the case. The presence, indeed, of man in Europe in the Miocene and Pliocene epoch is as yet non-proven, and we must be content to await future discoveries. The results of the labours of archæologists and geologists throughout Europe during the last ten years has not placed the advent of man further back than the river gravels of the Somme, and the epoch of the caves, both of which are post-glacial or post-pliocene, or quaternary, in other words posterior to the great submergence and refrigeration of northern Europe, through which many of the Pliocene mammalia were destroyed.

M. Hamy has done good service to the students of the Quaternary epoch by refusing to allow the validity of M. Lartet's divisions into the age of extinct animals, as distinguished from that "of those which have migrated." The intimate association of the remains of the two groups in the caves and in the river-deposits, renders such a division untenable. He also modifies the divisions of the Quaternary invented by M. Lartet—(1) the age of the cave-bear, (2) that of the mammoth and woolly rhinoceros, (3) that of the reindeer, and (4) that of the aurochs, by running the first and the last two into two groups, connected together by a series of transitions. In other words, in the application of M. Lartet's system, he finds it necessary to admit that the "ages" are more or less connected together, and have no very great value in classification. M. Lartet was undoubtedly correct in the view that the post-glacial or Quaternary mammals did not arrive in Europe *en masse*, but he has not shown us the order of their appearance, which is the very corner-stone of his system. So far as the geological evidence goes, the aurochs was probably living in the Val d'Arno in the Pliocene age, and the reindeer is found as abundantly in France, Germany, and Britain with the cave-bear as with the mammoth and woolly rhinoceros. Since, indeed, the Quaternary epoch succeeded the great lowering of temperature, it might, *a priori*, be inferred that the reindeer was one of the first animals to invade the then almost arctic regions of Central and Northern Europe. The caves, however, and the river-deposits, reveal nothing on this point; they merely prove beyond a doubt that all the Quaternary mammals were living here at the same time. It is very hard to understand why M. Lartet should have expected to find all the species of animals in one locality, and should have based his classification on the absence of some, and the presence of others, since in every living fauna the animals are unequally distributed. Nor is there any intelligible cause why some few animals should be picked out of a large fauna to the prejudice of the rest, for classificatory purposes. The Essay on the Post-glacial Mammals of Great Britain (Quart. Geol. Journ. 1869) demonstrated that the system will not apply to the British Fossil Mammalia, and M. Hamy's book implies that it is equally inapplicable to those of France, for which it was intended. It is not too much to say that our present knowledge forbids any attempt to subdivide the Quaternary epoch by an appeal to the animals living at the time. Archæologists may perhaps be able to classify the different forms of implements, but naturalists are as yet unable to learn the order in which Quaternary mammalia invaded Europe. The reindeer is quite as likely to have preceded as to have succeeded the mammoth.

W. Boyd Dawkins

COOKE'S CHEMICAL PHILOSOPHY

First Principles of Chemical Philosophy. By Josiah P. Cooke, Jun., Erving Professor of Chemistry and Mineralogy in Harvard College. Pp. 533. (Macmillan and Co., London and Cambridge, 1870.)

THIS book is intended to be used by students who have attended lectures on experimental chemistry, or after a course of laboratory instruction; hence it deals merely with the theoretical principles of the science and their application for the solution of many practical problems of chemical research.

Every chapter and section is followed by a series of problems and questions which the student is recommended carefully to work out, and anyone who has mastered all the problems set forth will have attained a very considerable proficiency in chemical science.

In the introductory chapter we have definitions referring to volume and weight, and the author has adopted here, as in several other instances, different kinds of type in order to represent different relations. Thus, Sp. Gr. in italics means specific gravity referred to water as unity; the same symbol in ordinary Roman letters signifies that hydrogen is taken as the standard; and when printed in Old English type that air = 1. The distinctions between chemical and physical forces are here pointed out.

This chapter is concluded by twenty-three problems and questions referring to weights and measures and specific gravities. Throughout the book temperatures are expressed in centigrade degrees, and the metrical system is employed. Next, we have chapters on fundamental chemical relations and molecules, and the relations of the latter to heat. Chapter IV. treats of Atoms, and Chap. V. of Chemical Notation. Under this head we observe several definitions and modes of formulation which are not usual in this country. Thus chemical reactions are divided into three classes, analytical, synthetical, and metathetical, the latter including double decomposition, displacement, and re-arrangement. The symbols are usually printed in italics, but solid bodies are formulated in "full-faced" type, and gases in skeleton type; and when reactions take place in aqueous solution this is expressed in the equation by including the dissolved bodies in brackets with the symbol Aq: thus, $(Ca\ Cl_2 + H_2O + Aq)$ shows that the calcic chloride is in solution, and that the quantity of water represented by H_2O is a result of reaction. In the chapter on Stochiometry (though Stoichiometry seems more correct) are a number of modes of calculation of formulæ, and of the relations between weight and volume; and under Chemical Equivalency is a kind of constitutional formulæ which somewhat differs from those in general use. Professor Cooke follows the principle laid down by Dr. Frankland that chemical formulæ should represent, as far as possible, the relations existing between the atoms of a compound; but he has thought it advisable to place the grouping element or radical at the end of the formula instead of the beginning; this, we think, is to be regretted, as it adds another to our already numerous modes of formulation. Short strokes are placed at the side of the grouping element or radical to indicate the direction of the attractions; thus the formula for Ethylacetamide is $H, C_2\ H_5, C_2\ H_3\ O \equiv N$, showing that the hydrogen, ethyl, acetyl, are united directly to the nitrogen, the commas indicating that they are not united among themselves: Diethylurea $H_2, (C_2\ H_5)_2, \equiv N_2 = C\ O$ is another instance. When these short strokes become numerous they are rather confusing, as in Turquoise, $O_3 \equiv [Al_2]_2 \equiv O_6 \equiv (P\ O_2)$. Graphic formulæ of Kekulé's and Crum Brown's form are explained, and students are advised to make frequent use of them, but not to abuse them. The nomenclature employed is that which is frequently used in England, and which Mr. Cooke designates as that of the London Chemical Society, though we think that some of the Fellows disapprove of such terms as zincic sulphate, &c. The physical relation of chemical bodies, as crystallography, electrolysis, spectrum analysis, and heat of chemical combination, are treated at some length. The elements are divided into sections according to their atomicity, the perissads being taken first and afterwards the artiads. This arrangement places oxygen beyond the middle of the book, instead of near the commencement as usual. The occurrence, mode of preparation, and properties, of the elements, and of their more important compounds, are very concisely given. Carbon is described as occurring in three forms, diamond, graphite, and *coal*. This last term seems an unfortunate one for amorphous carbon, as some coal contains as little as 73 per cent. of the element. Under the compounds of carbon is a section on Organic Chemistry which extends over only sixty pages. Although it contains a vast amount of information, and organic compounds are mentioned throughout the book, yet it seems rather a meagre account of the enormous developments of this branch of Science. At the end of the book are tables of French measures and weights, of elements, of the specific gravities of gases and vapours, and of logarithms and antilogarithms. We wish this book success, as it indicates great vitality in Transatlantic chemistry.

OUR BOOK SHELF

Elementary Treatise on Natural Philosophy. By Professor A. Privat Deschanel, of Paris. Translated and edited, with extensive additions, by Prof. Everett, D.C.L., of Belfast. In 4 parts. Part 1—Mechanics, Hydrostatics, and Pneumatics. (London: Blackie and Sons.)

THIS translation of Prof. Deschanel's "Traité de Physique," will, we believe, be found extremely useful. An elementary treatise of moderate size on Physics has been long wanted in our schools. Atkinson's translation of Ganot, or Brook's Natural Philosophy, is too expensive for general use, and of smaller books, none, so far as we know, are good. The issue of Dr. Everett's translation in parts at the moderate price of 4*s.* 6*d.*, will enable it to be largely employed. The engravings with which the work is illustrated are especially good, a point in which most of our English scientific works are lamentably deficient. The present part contains Mechanics, Hydrostatics, and Pneumatics. The clearness of Deschanel's explanations is admirably preserved in the translation, while the value of the treatise is considerably enhanced by some important additions. Thus, to Deschanel's description of the pendulum is added a short account of the condition of isochronous vibration, moment of inertia, momentum, and kinetic and potential energy. In the section on Pneumatics, Deschanel's extremely good description of the air-pumps of Hawksbee, Bianchi, Kravogl, Geissler, and Deleuil, is supplemented by an account of Sprengel's mercurial pump. It is possible to point out defects, but they are few. The conception of "mass" is always a difficult one for a beginner. Deschanel gives a very clear explanation, which is not reproduced, the term being employed without explanation. Nor do we understand why, instead of Deschanel's statement that the co-efficient of absorption of ammonia in water at 0°C. is 1050 (exactly it is 1150), it is said to be only 600. But these are minor defects, and we repeat that we believe the book will be found to supply a real need. W. M. W.

The Wild Garden; or, Our Groves and Shrubberies made beautiful by the Naturalisation of Hardy Exotic Plants. By W. Robinson. (London: Murray.)

THIS little volume forms a fitting sequel to Mr. Robinson's admirable book, "Alpine Flowers for English Gardens," which we noticed some time back.* It does not contain nearly such a large amount of novel information, but will nevertheless be of great value to all lovers of their gardens. The book is a protest against the practice which commenced some twenty years since, and reached its height a year or two back, of throwing the whole energies of the gardener into producing large masses of colour by the use of what are termed "bedding plants," to the exclusion of the cultivation of the individual flower. This somewhat barbaric taste has resulted in the gradual disappearance from our gardens of many flowers which had been "household words" since the time of Shakespeare, to the great detriment, as Mr. Robinson considers, of gardening as a real art, and to the enormous increase of its expense.

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* NATURE, vol. i. p. 603.

Our great traveller and naturalist, Mr. Wallace, says, that "during twelve years spent amidst the grandest tropical vegetation, he has seen nothing comparable to the effect produced on our landscapes by gorse, broom, heather, wild hyacinths, hawthorn, and buttercups." Mr. Robinson's aim is to make our gardens as beautiful as our hedgerows and woods; and to this end he would not have his favourite plants placed together indiscriminately in a bed; but, as far as possible, he would imitate the natural *habitat* of each species, and for this he gives full instructions in each case. Half the volume is occupied by a list of hardy exotic plants suitable for naturalisation in our woods, semi-wild places, shrubberies, &c., with directions for their cultivation; and we hope it may assist in again bringing the public taste to the culture of flowers beautiful not only in themselves, but from the historic associations connected with many of them. A. W. B.

Die Kleinschmetterlinge der Umgegend Munchens und eines Theiles der bayerischen Alpen. Von August Hartmann. 8vo, pp. 96. (Munich, 1871: E. Lotzbeck.)

THIS is a catalogue of the Micro-Lepidoptera of the neighbourhood of Munich, and of a portion of the Bavarian Alps, with indications of the plants on which the larvæ of the different species have been found feeding and of the times and places at which the species have occurred. In his Introduction the author describes the method which he adopts for killing and preparing the delicate little moths which form the subject of his book, and from this the collector of Micro-Lepidoptera may gain some important hints. He also notices especially the curious moths belonging to the group of Psychidæ, and those Tineidæ which resemble them in habits; and he fully confirms the statements of Von Siebold as to the occurrence of parthenogenesis in *Solenobia triquetrella* and *lichenella*. W. S. D.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Eozoon Canadense

IT is now about five years since a series of communications to the Geological Society of London by Logan, Dawson, Carpenter, and Sterry Hunt, announced the discovery of organic remains in the Laurentian rocks of Canada. They were decidedly interesting, not only as attempting to show that the belts of limestone interpolated in the great beds of Laurentian Gneiss were organic in their origin, but also from the *art* which the authors displayed in the mode of placing their views before the public. The realistic manner in which the fossil *Eozoon canadense* is drawn as it were from the *life*, coupled with the fixed belief in most men's minds that limestone is necessarily organic in its origin, predisposed many to accept the theory without much inquiry. The reputation of Dr. Carpenter as a physiologist was alone considered sufficient to settle the matter. These views did not, however, long remain unchallenged, for in the following year Professors King and Rowney, in a communication "On the so-called Eozoonal Rock," detailed the elaborate investigations by which they arrived at the conclusion that the presumed fossil was purely a mineral production. The replies that naturally followed were literally little more than reiterations of previous statements, excepting in the important admission from Dr. Carpenter, that the several features (that is chamber casts, canal system, and proper walls) could be separately paralleled elsewhere. He, however, took his stand upon the combination of the whole found in the Canadian specimens. To the fatal objection that all had been obtained from metamorphosed rocks Dr. Dawson replied by producing a specimen from Tudor, Ontario, which Sir W. E. Logan goes no further than to declare is from *comparatively* unaltered limestone, but which Dr. Dawson considers furnishes a conclusive answer to all arguments drawn from metamorphism. Since then I am not aware that any further evidence in favour of the organic hypothesis has been made public. On the other hand, Professors King and Rowney announced in a paper, read before the British Association at the Liverpool meeting, that they had discovered the features of the so-called organism in the Ophite of Strath in the Isle of Skye, an altered sedimentary deposit of the Liassic age, in which evidence of its mineral origin was conclusively proved. Here, at present, the matter rests; but in my opinion ample materials exist for forming a judgment, not by reliance on authority but by independent reasoning. With this object in view, and with your permission, I will proceed to detail a few of the facts of the case. Before doing so I would, however, call attention to the strange absence of any allusion to obvious objections which characterises the first series of papers, and to the persistent begging of the question involved in constantly speaking of the specimens as undoubted fossils. The adoption of this objectionable practice under authority of such eminent names is prejudicial to an impartial judgment, as it indirectly influences the mind. I am quite willing to admit that there existed sufficient reasons for suspecting them to be fossils, but I submit that it is not philosophical to state so distinctly before a thorough examination of all the objections. For evidence of this having been done I search these papers in vain. How then can any one, accustomed to scientific methods of investigation, help suspecting that under all this scientific and pictorial use of the imagination there exists or lurks a fallacy?

First, then, the specimen from Tudor has to be disposed of; nor will this be difficult, for it is altogether a lame affair. It is admittedly not from an unaltered rock, so it is difficult to see even how it bears on the question. The distinctive features are also obscure, and the chambers not of the usual form and proportion. To call this *Eozoon canadense*, and then bring it forward as closing the discussion, is an amusing piece of controversial skill. When it is more certainly co-ordinated with the original specimens, it will be time to discuss it. I simply ask, would it have been pronounced organic had it been the only variety discovered? I think not.

The broad fact then remains unshaken that in unaltered rocks no Eozoonal structures have yet been discovered. On the other hand, in metamorphosed rocks such structures are abundant, and even Dr. Gumbel, of the Bavarian Survey, a believer in Eozoon, has been much mystified by finding its features in impossible places. Not only do we find it in the Laurentians, but in rocks of a much later date, but curiously only in those that have undergone alteration. If it be an organism, then hydrothermal action, it seems, is necessary to its development, not as one would suspect during life, but ages after its entombment in sedimentary deposits.

The prevailing infilling material of the "chamber casts" representing "the sarcode body of the animal," is also admittedly serpentine or some analagous mineral—a mineral that forms the basis of no known fossil, consequently we are to assume that at each period of the animal's existence, either the conditions were different to those under which others were fossilised, or that the original infilling has since been replaced by serpentine; and this, be it said, must always have happened in those rocks afterwards selected for metamorphism.

Again, as minerals of this description are never found in unaltered rocks, we must be prepared to believe in the curious coincidence of the same rocks, and only these, having, at periods widely separated in point of time, been selected for the preservation of the organism and the deposit of these minerals. Either we must do this, or be prepared to show that metamorphism must necessarily change the infilling of the "chamber casts" to serpentine. Which supposition is the wildest?

Still further, we must believe that not only has Nature so miraculously preserved her pet animal, but that she has also imitated the fossil organism in the same minerals in an altered rock, in a manner to justify such acute observers as Professors King and Rowney in considering the imitation identical with the thing itself. For in the altered portion of the rock at Strath, before referred to, we have the features of the Eozoon, while the unaltered portion, which it gradually shades off into, teems with characteristic Liassic fossils. Have these fossils been obliterated in the altered portion, or have we the Eozoon again conterminous with the metamorphism?

And, in conclusion, we must further admit that all these conditions have been fulfilled over wide areas and at periods remotely separated with unerring regularity whenever the Eozoon has made its appearance. Is not this an improbability amounting to the impossible? For my part, this negative evidence far outweighs the "determination of its foraminiferal affinities by a point no larger than a pin's head," and I feel assured that whenever impartial geologists take the question up the fossil itself will become extinct. T. MELLARD READE

Blundellsands, Liverpool, Dec. 12

The Difficulties of Natural Selection

MR. WALLACE'S frank acknowledgment, for which I thank him, that he had in his two previous letters misunderstood my line of argument in what I consider one of the most important points at issue between us, absolves me from the task of again defending myself from charges of error and self-contradiction. As, moreover, Mr. Wallace has not accepted my challenge "to explain the nature of the intelligence which was operative in the creation of man, and which is a principle unknown in the rest of the organic world," it is impossible to pursue further this branch of the question. All naturalists will look forward with the most intense interest to Mr. Darwin's long-promised work on Natural Selection as applied to Man. There are, however, one or two subsidiary points raised in the discussion, to which I shall be glad of the opportunity of briefly referring. Mr. Stebbing, objecting to my attempted parallelism between mimicry and instinct, says that "it can hardly be said to be proved" that the extraordinary resemblances occasionally found in the vegetable kingdom are not protective or mimetic. I certainly think it can be. When we find an almost absolute identity between the foliage of a plant belonging to Africa and another growing in South America,* we are certainly justified in saying that one has not imitated the other, and that it gains no protection from the resemblance. Mr. Carvalho again makes merry over what he calls "my" argument, that imperfect imitation is, to all appearances, not beneficial in the cases published by Mr. Weir. The argument is not mine. I simply recount the observations made by practical entomologists, undertaken at the suggestion of Mr. Wallace himself. Mr. Carvalho's argument, which follows, is an instance of how, when a theory is once adopted, every conceivable fact may, by its too zealous advocates, be twisted to support it. Had these twig-like caterpillars been rejected by birds, it would have been considered a triumphant proof of the theory of Natural Selection; the fact that "they are eaten with great relish," we are told is equally "really in its favour"!

Westminster Hospital, Dec. 17 ALFRED W. BENNETT

Is Mimicry Advantageous?

THE discussion of mimicry among butterflies, in the recent numbers of this Journal, has brought to my mind some considerations which seem to have been overlooked by those who have treated the subject.

Of the fact of mimicry there can be no possible doubt, and in some instances it is even more striking than has been asserted. For instance, in North America, Messrs. Walsh and Riley have pointed out the resemblance between *Danais Archippus* and *Limenitis Misippus*; they might also have shown that in the extreme southern states where *L. Misippus* occurs, and *D. Archippus* is replaced by *D. Berenice*, the colour of the mimetic Limenitis deepens nearly or quite to the tint of the southern Danais.

But of how much actual benefit to the mimetic species is this so-called "protective" resemblance? It seems to occur where it can be of the least possible advantage to the species. The great sources of destruction here, as in all groups of animals, are in early life. How large a proportion of the eggs that are laid by butterflies ever finally produce imagines? Let those answer who have attempted to follow their history in their native haunts. My experience leads me to believe that, at the very least, nine-tenths—perhaps ninety-nine hundredths—never reach maturity. Hymenopterous and dipterous parasites beset them at every step; the eggs, although so small and often heavily ridged, cannot escape the ovipositors of the tiny Pteromali; while in attempting to breed caterpillars taken in the field, the chance is so greatly against the evolution of a butterfly, that hymenopterists actually choose this method of supplying *their* cabinets. "Of two hundred larvæ of *Pieris Brassicæ*," Mr. Drewsen, of Denmark, writes to me, "I obtained only twenty pupæ; all the rest were attacked by *Microgaster glomeratus*," and my own attempts with the larvæ of *Pyrameis Atalanta*, both in America and Europe, have been even more unavailing. These caterpillars seem to be peripatetic banquetting halls of Microgasters and Tachinæ.

Now it is a curious fact that while the globular egg of *Limenitis Misippus*, with its deeply-pitted shell, defended by long filamentous spines, is constantly attacked by parasites; and the grotesque, hump-backed, strangely-coloured caterpillar of the same species is likewise infested to an extraordinary degree, I have been unable to discover by very careful search any evidence that the egg or larva of *Danais Archippus* is ever pierced by a parasite; yet the egg is not small and only lightly ribbed, and the caterpillar large, fleshy, smooth-skinned, and gaily banded, living on the widely-separated leaves of Asclepias, with no attempt at concealment. The abundance of the imago of the Danais is then due quite as much to the immunity of the egg and larva from the attacks of parasites, as to any freedom it may itself enjoy from pursuit by insectivorous birds.

Although I have hunted butterflies for fifteen years, I confess I have never seen one in a bird's bill, and my faith in that method of lessening their numbers is very slight. Birds, too, must be their greater foes in earlier life; and the chances of living, which are certainly against them before they take wing, seem afterwards rather in their favour, at least, until they have accomplished their mission.

If, then, such an extraordinary element as Mimicry is to be summoned to the aid of Natural Selection, and can perform its task in such a masterly manner, why has it been made to waste its energies upon unimportant material? If the object of the resemblance be protection, why does not the unfortunate caterpillar of the Limenitis mimic the more favoured larva of the Danais?

I cannot now consult the writings of Messrs. Wallace and Bates, nor do I remember their statements respecting the abundance of the mimetic species compared to that of its normal congeners. In my own country *Limenitis Misippus* is, as a general rule, more common than *L. Ursula*, but the difference in their numbers is not very marked. It is by no means as great as one would expect had Mimicry in the imago state so strong a protective power as has been assumed. Two closely allied species,* occupying the same geographical area, do not often occur in the same abundance, whatever be the cause; and the disparity in numbers in these two species of Limenitis is no greater than occurs in many instances where mimicry plays no part.

Cairo, Egypt, Nov. 9 SAMUEL H. SCUDDER

Nepenthes

THE allusion to Nepenthes in Mr. Buckton's interesting article in a late number of NATURE, on the liquid secreted by this and other plants, prompts me to place on record a few facts regarding that genus, at which I have just arrived, after monographing the Pitcher-plants for the "Prodromus Systematis Vegetabilium" of De Candolle; a work of which the publication is suspended, owing to the siege of Paris.

The genus *Nepenthes* extends from Madagascar on the west to N.E. Australia, the Louisiade Archipelago, and New Caledonia on the east; embracing within these limits, thirty species, most of which have well marked characters in the pitcher, but which, with only two exceptions, present a wonderful uniformity in the structure of both flower and fruit. It has two foci of maximum development; the Malay Peninsula (including Sumatra), and Borneo, in both of which localities the species are not only more numerous, but more gigantic than in any other country. No fewer than twenty-one species inhabit these two countries, of which thirteen are common to both; but, what is very remarkable, the intervening island of Java contains but one representative of the genus, and that a totally different species from either the Bornean or the Malayan; thus confirming the fact first brought to light by the Dutch naturalists, of the close biological relationship between the two former localities, to the exclusion of Java. Only one species has a wide range, the *N. phyllamphora*, which extends from Sumatra to Borneo, Amboyna, China, &c., but is absent from the island of Java.

Proceeding from the Malayan islands westwards, we find one species in east Bengal; more allied to the Javanese than to any other; another in Ceylon, the old *N. destillatoria* of Linnæus (a name long usurped in our gardens by the Bengal plant), which presents the first departure from the typical structure of the genus, having a spreading paniculate inflorescence; a character shared by those in Madagascar and the Seychelles. Proceeding further west to the African islands, we find still further deviations from the type, which now extend to the structure of the seed and fruit; for whereas all the eastern species have very long appendages to the seed, which are no doubt instrumental in its dispersion, these appendages are very short in the Madagascar species, and are wholly absent in the Seychelle one; which thus presents a case analogous to that of the prevalence of wingless insects on oceanic islets. Lastly, the Seychelle Islands species further differs from all others in the structure of its ovary and capsule.

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*See NATURE, Vol. ii. p. 70.

*L. Misippus and L. Ursula can with difficulty be separated in their earlier stages, although so unlike in their perfect forms.

To sum up, deviation from the type of the genus commences on the western confines of the principal centre of its distribution, namely in Ceylon; and the initial deviation, that met with in the Ceylon species, is the slightest, but is propagated (so to speak) westwards, equally characterising the two African islands Pitcher-plants, which again deviate still further from the type; the maximum deviation, however occurs, not in the great sub-continental Island of Madagascar, where the endemic species has a considerable range; but in the very small oceanic Archipelago of the Seychelles, where the only native species is confined to the one mountain summit of one island of the group!

The only other fact that struck me as bearing upon this subject of distribution is, that though present in the Seychelles, the genus Nepenthes is absent from the Mascarene group (Mauritius, Bourbon, and Rodrigues). This is only one instance of the broad distinction that exists between the vegetation of these Archipelagos, and which is in some way connected with the fact that the Mascarene group is volcanic, the Seychelles group formed of granite and quartz. Coincident and perhaps co-ordinate with these phenomena of plant distribution, geographical position, and geological structure, are the facts that the flora of the Seychelle Archipelago is more Asiatic, and the florulæ of its several islets very uniform; whilst the florulæ of the islets of the Mascarene Archipelago differ wonderfully, and in their totality are more African than Indian. The flora of the Mascarene group may hence be regarded either as a very ancient outlying province of the African, or as consisting of a more modern assemblage of plants, derived at various periods from Africa, but subsequently much altered by causes operating in the several islets; or more probably its peculiarities are attributable to both causes. Long as the Mascarene and Seychelle islets have been colonised, under Dutch, French, and English rule, their floras are still very imperfectly known; so much, however, of Mascarene botany is known, as to show that its relations with those of the Seychelle group and Madagascar, and the relations of all these with India and Africa, are most complicated, and present one of the most puzzling problems in Phytogeographical Science.

Royal Gardens, Kew, Dec. 18 J. D. HOOKER

THE author of the notice which appeared in a recent Number of NATURE is probably unaware that a minute analysis of the "water" found in the pitcher of Nepenthes was made a few years since by Dr. Volcker. For full particulars I will refer your correspondent to "Annals and Magazine of Natural History," 27, 4, 128, and "Phil. Magazine," 3, xxxv., 192; but I may perhaps be allowed to give the results of the analysis. My extract is from Liebig and Kopp's "Annual Report, &c." "The liquid was generally clear and colourless, rarely yellowish, and reddened litmus. That which was collected from different plants gave respectively 0·92, 0·91, 0·87, 0·58, 0·62, and 0·27, per cent. of residue, which contained in 100 parts 38·61 per cent. of organic matter, consisting chiefly of malic acid with a little citric acid, 50·02 of chloride of potassium, 6·36 soda, 2 59 lime, 2·59 magnesia."

During the early part of the present year I was led to suspect the presence of some form of tanno-gallic acid in the tissue of the stalk, and the kindness of a chemical friend enabled me to verify my conclusions; but no quantitative analysis has, to my knowledge, been made beyond the one I have referred to.

Hull, Dec. 1 H. POCKLINGTON

Cockroaches

THE facts mentioned by your correspondent, Mr. Arthur Nicols (in your number of Dec. 8), are notorious to all West Indians. A friend of mine was marked for life by these things on board a ship coming home from Jamaica.

As for their scent, if you crush one in England it smells evil enough; and I don't doubt Aristophanes's sharp Greek nose had found that out. I have known bread, &c., in the West Indies uneatable from being run over by the small dark Cockroach of England, *Blatta orientalis;* while the great pale species, *B. occidentalis*, is utterly unbearable. C. KINGSLEY

EARED SEALS AND THEIR HABITS*

THIS paper, which forms the first number of the second volume of the "Bulletin of the Museum of Comparative Zoology at Harvard College in (Transatlantic) Cambridge," is one of great zoological importance, and likewise of much general interest. The Eared Seals, a group of marine Carnivora, which form a well-marked division of the Pinnipedia, distinguished by the possession of a small external ear-conch and other peculiarities, are still very imperfectly known, although of late years they have attracted the attention of several eminent naturalists. Unfortunately, however, the great variations which occur in the sexes and different ages of these animals, have not been sufficiently appreciated by those who have studied the few specimens of them preserved in European museums. The consequence has been that numerous artificial species have been manufactured upon stray skulls and imperfect skins, which have exhibited what were really only individual differences. Moreover, what is worse than this, under the prevailing mania for coining new generic names, more genera of Eared Seals have been established than the number of species which actually exist in nature. Foremost amongst these offenders, we regret to say, has been one of our own countrymen, who, in a recent article published in the "Annals of Natural History," has subdivided, on the most trivial characters, the family *Otariidæ* into four subfamilies and ten genera! We shall see how much more reasonable and consonant with nature are Mr. Allen's views on the arrangement of these animals.

Mr. Allen commences his paper by an Introduction, in which he discusses at some length the writings of preceding authors on this subject. He then proceeds to set forth his own views, distinguishing first of all the Eared Seals from the two other families of the Pinnipedia (the true seals and the walrus), and afterwards the different genera and species of *Otariidæ*, in a very neat and perspicuous manner. Mr. Allen is only able to recognise eight species of these animals, and considers two of these rather doubtful. Four of them belong to the "Hair-seals," or "Sea-lions," which have no under fur, and four to the smaller "Fur-seals," or "Sea-bears," which have a dense under coat, and furnish the seal-skin cloaks so much now in fashion with English ladies. The well-known "Sea-lion" in the Zoological Gardens belongs to the former group—being a female of the Southern Sea-lion (*Otaria jubata*).

Mr. Allen next begins to treat at great length of the North Pacific species of Eared Seals, of which he is able to give us a full and excellent account from the specimens in the Museum to which he is attached, together with those in other American collections. These North Pacific species are the Steller's Sea-lion (*Eumetopias Stelleri*), Gillespie's Hair seal (*Zalophus Gillespii*), and the Northern Fur-seal (*Callorhinus ursinus*). Of these three animals such full particulars are given that it seems scarcely possible that there can be any more confusion respecting them. But the most remarkable part of the present memoir is perhaps the account of the extraordinary habits and customs of the Northern Fur-seal, given from Captain Bryant's observations of these animals, on the Pribyloff Islands, off the Northern part of Alaska Territory. As is the case in other known species of Eared-Seals, there is an enormous discrepancy in the size and weight of the two sexes, the weight of the female being rarely more than one-fourth of that of the full-grown male.

The Fur-seals resort to the Pribyloff Islands during the summer months for the purpose of breeding, and in St. Paul's Island, where Captain Bryant made his observations, occupy at this season a belt of loose rocks along the shore, varying in width from five to forty rods. Twelve miles of shore line at least are taken up by what is called their "*breeding rookeries*" in this island, and are tenanted by not less than 1,152,000 breeding males and females, according to Captain Bryant's estimate. Each male seal stations himself in a particular spot, usually the same as he has occupied in former years, and keeps about a square rod of ground free around him to afford space for the reception of his ten or fifteen wives. By the 15th of June all the males have arrived, and have stationed themselves each in his own domain, not without constant growlings and fightings with his neighbours for what he considers the best station. The young males are not allowed to take a place in the "rookeries," but are driven by the patriarchs back into the sea, or compelled to resort to the high rocks above. After the middle of June, the females arrive; in small numbers at first, but increasing as the season advances, until the middle of July, by which time they are so crowded together that they often overlap one another. The old males who are nearest the shore seize upon the females at once, and of course fill their harems first. But the males who are higher up on the rocks select the time when their more fortunate neighbours are off guard to steal their wives, taking them up in their mouths, and carefully carrying them off to their own dominions, as a cat would her kittens. Struggles often occur between two males for the possession of the same female, and both seizing her at once, terribly lacerate her with their teeth. When his harem is full, the old male struts complacently around reviewing his domestic circle, and fiercely driving off all intruders. Two or three days after landing and taking up her abode, the female brings forth her single pup, after which she is ready to associate with the male. By the middle of August the young are all born, and the females are again pregnant. The old males having been constantly in their stations for four months *without food*, now leave the females and young to the company of the younger males, and go off-shore to feed. At the end of October the whole body of seals leave the island and journey southwards.

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* "On the Eared Seals (*Otariidæ*), with detailed Descriptions of the North Pacific Species" By J. A. Allen Together with an Account of the Habits of the Northern Fur Seal (*Callorhinus ursinus*). By Charles Bryant. With Three Plates. 8vo. 108 pp. (Cambridge: University Press. 1870)

The greatest care is taken by the hunters never to disturb the breeding places of the seals in any way, and the only seals killed for the sake of their fur are the younger animals (principally males) that resort to the higher rocks above the rookeries to pass the night. A party of men armed with clubs surround a portion of the herd and drive them off sometimes six or seven miles across the island, to the place selected for killing and skinning them. By this plan the rookeries are less liable to be alarmed, and the seals are made to carry their own skins to the salting houses, which would otherwise be a work of much labour. At the present time the annual yield of seal-skins from the Pribyloff Islands is estimated to have reached 100,000, and the killing yearly of this number is believed in no way to check their increase, but rather to augment it.

This short sketch will serve to give an idea of Captain Bryant's account of the extraordinary habits of this animal, and of the way in which the large annual supply of the much-valued seal-skin coats of civilised life is produced. Many other details of the highest interest are added, for which we must refer our readers to the original article. Although several accounts have been already published of the habits of other species of this group, none, we believe, is so full and perfect as the present, which forms a valuable appendix to Mr. Allen's excellent essay already spoken of. In short, it may be truly said that, by this single memoir, more extensive knowledge has been gained concerning this little-known group of mammals than by the half-dozen different systems of arrangement of them which have lately emanated from the British Museum, and the publication of an indefinite number of (so-called) new genera and species founded upon stray skulls and imperfect skins.

P. L. S.

SCIENTIFIC TEACHING IN ELEMENTARY SCHOOLS

THE following address, signed by Prof. Huxley, as President of the British Association, has been presented to the Vice-President of the Council by a deputation, consisting of the President of the Association, the General Secretaries, and the Treasurer; Sir Charles Lyell, Bart.; Sir John Lubbock, Bart., M.P.; Dr. Lyon Playfair, M.P.; and Mr. Francis Galton:—

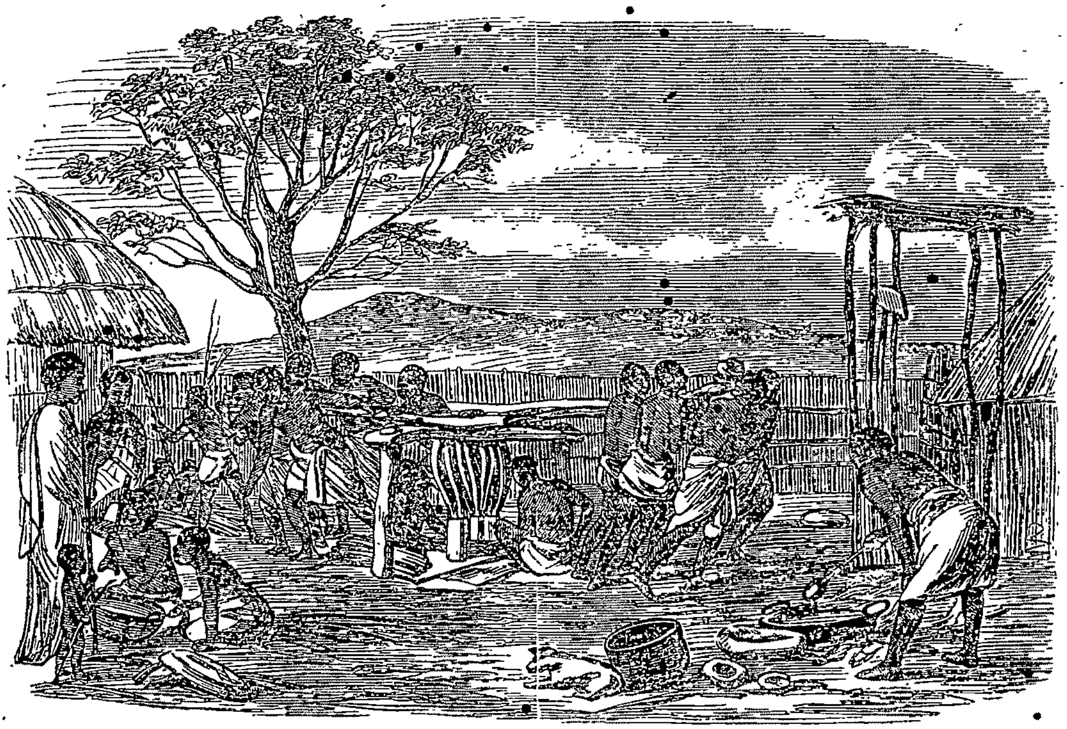
"The deputation from the Council of the British Association for the Advancement of Science waits upon you for the purpose of urging the advisability of including elementary Natural Science among the subjects for which payments are to be made under the authority of the Revised Code. We have asked you to receive us at the present time because we understand that you have announced your intention of making certain modifications in the Code. Our reasons for requesting you to give direct encouragement to the teaching of Natural Science in elementary schools are three. Firstly, we conceive such teaching to be one of the best instruments of education in the sense of intellectual discipline, and in many respects better calculated to awaken intellectual activity than other studies; secondly, we think that a knowledge of the elements of Natural Science has a high value as information; and thirdly, we are of opinion that scientific training and teaching in the elementary schools will afford the best possible preparation for that technical education of the working classes which has become indispensably necessary to the industrial progress of the country.

"We take the liberty of pointing out to you that, in asking for the introduction of scientific teaching into the elementary schools, we are not seeking for the creation of a new system or even of new executive machinery. The Science and Art Department does already provide for elementary scientific instruction; and all that is necessary to fulfil our desire is, that the system of the Science and Art Department and that of the Revised Code shall be brought into harmonious co-operation. In preferring the request that instruction in the elements of Science shall be made part of the regular course of instruction of all elementary schools, we desire carefully to guard against the supposition that we are seeking for such an amount of this kind of instruction as would interfere with the teaching of reading, writing, and arithmetic, and the other essential constituents of primary education. On the contrary, we think it very desirable that systematic instruction in elementary Science should be given only to those scholars who are able to read and write fairly; that it should be limited to certain well-defined subjects, such for example as elementary physical geography, elementary physics and chemistry, elementary botany, and, in consequence of its relation to the public health, elementary human physiology; and that care should be taken to make the instruction, so far as may be, real and practical.

"Finally, we desire to point out that such scientific instruction in the elementary schools as we pray for, would afford a means by which any child of exceptional aptitude for scientific pursuits might obtain the education suited to its capacity in the higher schools, and that in this way advantages similar to those which are offered by the scholarships and exhibitions of grammar-schools to the children of the well-to-do classes of society, would be extended to the poor and necessitous. In other countries in which well-organised systems of secondary education for the working classes exist, it has been found necessary to give a taste for Science in the elementary schools, so that the youth of the country may be induced to take advantage of the more advanced schools. While, therefore, we look with pleasure to the introduction of Science into the endowed schools of the country, we still believe that it will be necessary to link them to the elementary schools by commencing instruction in Science in the latter."

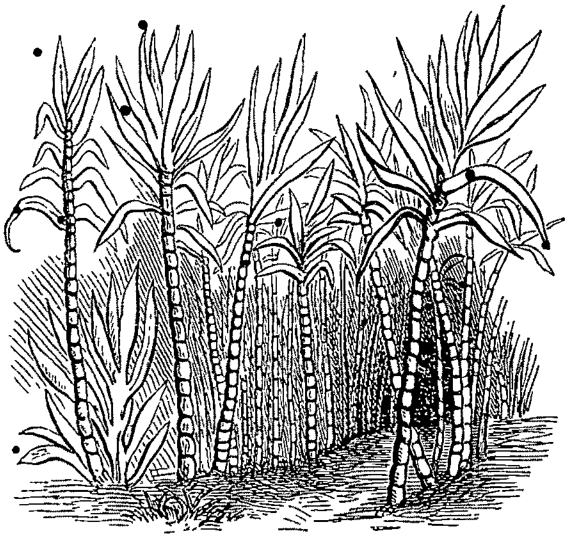
SUGAR

IN considering the subject of sugar, its produce, supply, uses, and adulteration, we enter upon a much wider field of inquiry than in either of our former articles, though the present has an intimate connection with our previous subjects; for neither coffee, tea, nor cocoa is usually considered properly prepared for table without the addition of sugar; it is used more or less in every part of the globe, for in the widest sense of the word, sugar is contained in most vegetable juices, indeed it is the principal food of young plants. In the rising sap of some trees in spring it is very abundant, as well as in the young stems of grasses. The starch stored up in many seeds at the time of germination is converted into sugar. The process of malting consists in forcing the seeds of the barley to germinate, and just at the time when most sugar is found, to stop their growth, so that the sugar is preserved for our use and not consumed by the growing plants. Sugar is extracted for the use of man from many distinct plants. Chemically considered, there are two kinds of sugar; one called cane sugar, which is obtained from the sugar-cane, the beet-root, the maple, &c.; the other called grape sugar, or glucose, which is chiefly found in grapes and various fruits. The bulk of the sugar used in this country is the juice of the sugar-cane (*Saccharum officinarum*, and perhaps allied species), a gigantic perennial grass, growing usually ten or twelve feet high, but in some situations attaining fifteen or sixteen feet; it has a jointed stem, somewhat similar to that of the bamboo, the upper part having a series of long, narrow leaves, and the flowers produced in large, feathery panicles. Some doubt exists as to the true native country of the sugar-cane, though it is not at all improbable that it came from Southern China and India. The plant is now very extensively cultivated in the East and West Indies, China, the Mauritius, S. America, and other parts.



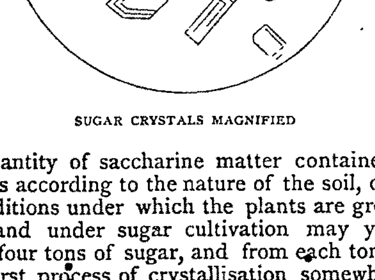
SUGAR MANUFACTURE AT KATIPO, A VILLAGE IN EASTERN TROPICAL AFRICA

The use of sugar dates back to a remote age; its introduction into Europe is said to have taken place in the 9th century, when it was brought from the East into Sicily by the Saracens, and the first European plantations were established about 200 years later in Sicily and Valentia. In the early part of the 15th century its cultivation began in Madeira, the Canary Islands, Granada, &c.; and at the close of the same century Columbus introduced it into one of the West Indian Islands. Barbadoes sent large quantities of sugar into England so long ago as 1646. The sugar-cane does not ripen its seeds, and is, therefore, propagated by cuttings. The canes after planting require, according to the situation and soil in which they are grown, from ten to twelve, or even twenty months before they are ready for cutting; they are taken off near the base, and the stem is then divided into equal lengths, put up into bundles and carried to the mill. These lengths are submitted to pressure between heavy rollers, by which the saccharine juice is squeezed out, and is collected in a cistern; it is next filtered and clarified, and the feculent matters separated by lime, a rapid system of boiling now throws off the watery particles by evaporation; and the sugar is brought to such a thickness or consistency that after boiling in one pan, its bulk is so reduced that it is removed to a smaller or medium sized pan, where it is boiled again and skimmed until it is reduced to sufficient bulk and thickness to enable it to undergo a similar operation in the next, or smallest size pan, where it is again boiled till it has assumed the consistency of a thick syrup, which partially granulates upon cooling. It is, however, still in the form of a soft mass, the crystallised portion being imbedded in a thick juice, which is known as molasses. To remove this the whole mass is put into loosely made casks or hogsheads, through which the molasses drains, leaving the crystallised portion more or less dry. After being kept a few weeks, during which time molasses continues to drain from it, it is packed in bags or hogsheads, and is ready for shipment. In this state it is known as raw, brown, or Muscovado sugar. The process of extracting and preparing the juice for export is not exactly similar in all countries; though the principle is the same, the practice varies according to the amount of intelligence brought to bear upon it.



FIELD OF SUGAR CANE

The drawing on the opposite page, which has been copied by permission of Mr. T. Baines from one of his paintings in the Kew Museum, represents the whole process of sugar making at a village in Eastern Tropical Africa. Under the tree, in the left-hand corner, is a native cutting the cane into lengths, and others collecting them into bundles which are afterwards passed between the rollers of the press, as seen in the centre of the picture. Boiling and crystallisation are shown in the right and left hand corners respectively.



SUGAR CRYSTALS MAGNIFIED

The quantity of saccharine matter contained in the cane varies according to the nature of the soil, climate, or other conditions under which the plants are grown. An acre of land under sugar cultivation may yield from one up to four tons of sugar, and from each ton of sugar after the first process of crystallisation somewhere about seventy gallons of molasses will drain; so that when we consider the labour required to keep a sugar plantation in full working order, such as tending the plants, expressing the juice, evaporation, crystallisation, and the subsequent waste both of sugar and molasses, as well in the draining on the voyage home, as during the time of its storage in the docks, and above all, the duty demanded upon sugar by the British Government, it does seem rather surprising that the article can be retailed at the price it is, though many are apt to call out that it is not so cheap as it ought to be. Sugar in its most common use cannot now be considered a luxury; to rich and poor alike it has become a necessity, indeed, amongst the lower classes a larger proportion is consumed than amongst the middle and upper section of the community, the average annual consumption per head of the whole population of the United Kingdom being about forty pounds. The East and West Indies, Mauritius, and Brazil now supply the bulk of the sugar brought into the English Market. It is imported in hogsheads and bags, the latter vary much in size and the sugar is also of various qualities, some of it indeed as it arrives has much the appearance of black muddy gravel or sand; most of this undergoes a further process of purification and recrystallisation in our own sugar refineries, which abound at the East end of London, as well as at Greenock, Glasgow, Liverpool, and Manchester. From these refineries the sugar comes out in the forms known in trade as lump or loaf sugar, crushed lump, pieces, and bastards.

The quantity of unrefined sugar entered for home consumption during the year 1869 was 11,188,081 cwts., and this exclusive of 1,025,929 cwts. of refined sugar and sugar candy, and 741,771 cwts. of molasses. The sugars best known in the British grocery trade are Demerara, Berbice, Barbadoes, Porto Rico, and Mauritius. These are sufficiently refined and crystallised in the colonies producing them as to suit the requirements of the retail trade. Those from Antigua, Cuba, Madras, Penang, &c., usually find their way into the hands of the refiners, brewers, and confectioners. Molasses is, as we have before said, the drainings from raw cane sugar. It is used for many purposes; large quantities of rum are distilled from it, often on the plantation where the sugar is produced; and where it is not so used it is exported, the sugar refiners in England buy it up largely, and produce a quantity of crystallised sugar from it. Rum, however, is usually made from the skimmings taken from the last boiling of sugar, which are mixed with proportionate quantities of molasses and water. Though molasses and treacle are often spoken of as identical, they appear to be different in their origin, for while the former is the drainings of raw sugar, the latter is the drainings of refined sugar. We have hitherto spoken only of cane sugar, or, to speak more correctly, of that obtained from the sugar-cane, but a large portion, indeed the bulk, of the sugar used on the Continent, is obtained from the beet-root (*Beta vulgaris*), the manufacture of which forms a separate article of industry.

JOHN R. JACKSON

NOTES

INTELLIGENCE has been received of an accident which occurred to H.M.S. *Psyche*, carrying the Sicilian section of the Eclipse Expedition from Naples to Catania; but, we are happy to announce, without injury to any of the passengers or crew, or loss of any of the instruments. The following are all the telegrams to hand at the moment of going to press:—"The *Psyche*, with the Eclipse Expedition on board, has struck on a sunken rock near Catania. All hands have been saved, and also the scientific instruments. The captain, who has behaved most nobly, is still on board, and, with a view to save the ship, has telegraphed to Malta for assistance." The following telegram has been received at the Admiralty:—"*Pysche* has struck, while running by chart, on a sunken rock near Catania. All saved. Instruments sent into Catania. Commander Fellowes has acted nobly, and hopes to save ship, if assistance comes at once from Malta, where he has telegraphed. *Royal Oak* sent for." We may congratulate ourselves that the expedition was in the hands of trained men belonging to one of Her Majesty's ships. We learn that nothing has occurred which need in the least imperil its success, and trust that even the ladies of the party will have suffered nothing worse than temporary inconvenience and alarm.

MR. J. R. HIND, writing to the *Times*, states that, although the Eclipse of the Sun, which takes place to-day, will have been exceeded in magnitude by more than one of the eclipses which have been visible in this country during the last thirty years, it will yet be the greatest eclipse that can be witnessed in England during the remaining thirty years of the present century, and on this account possesses a degree of interest which does not always attach to partial eclipses. If we take successive intervals of thirty years, commencing with 1781, and note only those eclipses visible in London, in which the moon has covered more than half the sun's diameter, we find between 1781 and 1810 two eclipses, between 1811 and 1840 six eclipses, between 1841 and 1870 seven eclipses, while between 1871 and 1900 there will be one eclipse only of similar magnitude. This single eclipse in the ensuing interval of thirty years will not occur until the last year of the century, or till the 28th of May, 1900, and the magnitude will not then quite reach 0·7 of the sun's diameter. The next solar eclipse visible in England is a small one on the morning of May 26, 1873.

THE French Eclipse Expedition may not have proved a failure, as has been feared. We have received intelligence from Paris that on the 1st December M. Janssen left in a balloon, assisted by a sailor. No letter was given him in order to prevent the Prussians detaining him as a prisoner of war. A balloon reached Brittany about that time, and was styled a private balloon. Telegrams and notices were sent to France from Mr. Lockyer, to invite M. Janssen to join the English Eclipse Expedition, but it appears that none of these communications have reached him, owing to the interruption of regular postal telegraphic service in France. M. Janssen was intending to proceed to Medeah, an Algerine town.

SOME time ago we announced that Sir Roderick Murchison had offered the munificent sum of 6,000*l.* for the endowment of a Chair of Geology and Mineralogy in the University of Edinburgh, on the understanding that the annual proceeds of this sum would be supplemented by a grant from Parliament. We are happy to state that Government has consented to this proposal, and has agreed to recommend an annual grant of 200*l.* This desirable result (for which the University, we believe, is largely indebted to the earnest co operation of its member, Dr. Lyon Playfair) will be welcomed as another evidence that our authorities are not so indifferent as they have been supposed to be to the claims of scientific education.

DR. HUREAU DE VILLENEUVE, secretary to the French Aëronautical Society, and editor to the *Aeronaut*, a paper devoted specially to aëronautics, has established at his residence an ambulance for aëronauts. He had to attend to a few cases from accidents of different descriptions in the management of captive balloons, and fewer of soldiers who had drawn the ropes for their aerial observations. M. de Villeneuve is to open a regular course of lectures on aëronautics at the Petite Sorbonne. It is the third time that a regular course of lectures has been delivered in Paris on this subject: the first time by Dupuis Delcourt, at the Athénée about forty-five years ago, when Comte was delivering his lectures on Positive Philosophy; the second time by M. W. de-Fonvielle, at the Lecture Hall of the Boulevard de Capucines, three years ago.

THE new Meteorological Observatory established by the city of Paris in the well-known Palais du Bey de Tunis, which was a part of the great Champ de Mars Exhibition, is no longer in operation. It was put into requisition to be used as a barracks. M. Sainte-Claire Deville, the elder, who was the director, has protested against such a requisition, but his protest was wholly disregarded.

M. MARIE DAVY is now in Paris, and engaged in meteorological observations, as well as M. Chapelas Coulvier Gravier, who keeps his watch from the Luxemburg Palace for falling stars. He publishes regularly the records of the observations in the *Comptes Rendus* and the *Journal Officiel*. He has published a description of several magnificent auroral displays, which were described by several witnesses in different papers.

A VERY influentially-signed memorial from heads of houses, professors, tutors, lecturers, and fellows of colleges in the University of Cambridge, has just been sent to the Lord President of the Committee of Privy Council on Education. The memorial prays that women may be appointed to the office of Inspector of Schools, and points out some of their qualifications for that responsible office. The memorial is signed by seventy-two resident graduates, all of whom are fellows of colleges or hold college or university offices in the University of Cambridge. Among those who sign are the Vice-chancellor and five other heads of houses, including the Masters of Trinity and St. John's; nine university professors; the senior tutors of every college, with three exceptions; all the tutors, assistant tutors, and lecturers of Trinity College, with two exceptions; and of the remainder all except eight are engaged officially in teaching in the Colleges or University.

THE death is announced, in his seventy-first year, of the Rev. Joseph Bancroft Reade, President of the Royal Microscopical Society. He was educated at Caius College, Cambridge, where he took his B.A. degree in 1825, when he was thirty-sixth senior optime in the mathematical tripos. In 1839 he was presented by the Royal Astronomical Society to the vicarage of Stone, near Aylesbury, and at the time of his death was rector of Bishopsbourne, near Canterbury.

WE shall be much obliged to any of our correspondents who will oblige us (for the purpose of drawing up a tabular account) with particulars of any hitherto unrecorded meteorological phenomena which may have occurred during the present year, as auroras, earthquakes, large sun-spots, meteors, storms, volcanic eruptions, haloes, &c. Similar information will be very acceptable from the continent of Europe, America, or the Southern Hemisphere.

THE Royal Horticultural Society has been compelled, by the pressure on its funds, to dispose of a portion of its gardens at Chiswick, the most valuable portion of its property in a scientific point of view. Among the articles sold were a number of fine trees, Pyramid Pears, Dwarf and Trained Apples, Filberts, Wellingtonias, Cupressus, Piceas, Araucarias, &c., in all no less than 12,757 plants, fetching about 600*l.* The portion of the Gardens which still remains covers about 33 acres; and the orchard will be reorganised on a smaller scale, and the trials and experiments, practical and scientific, will still be carried on. The Gardens were originally founded in 1821.

THE galleries of the Royal Albert Hall, which are to be used for the display of architectural drawings and models in the forthcoming International Exhibition of 1871, are approaching completion. Architects have been invited to inspect the Hall; and to see the galleries appropriated to their works, on Wednesday, December 21, at 11.30 A.M. Some trials of the acoustic properties of the building were to be made between 12 and 12.30.

THE *Journal of the Society of Arts* states that the Council of the Society has sanctioned a plan for establishing a National Training School of Music. For promoting this purpose it is proposed that a musical section of the society be instituted, with a separate fund, in order to give concerts annually in the Royal Albert Hall; these concerts to consist of vocal and instrumental music of the highest character. After paying the expenses of the concerts, the profits are to be applied to the establishment of a National Training School for Music.

THE Perthshire Society of Natural Science proposes starting a Quarterly Magazine, to be called the *Scottish Naturalist, and Journal of the Perthshire Society of Natural History*. It will be specially devoted to recording observations and discoveries made in the northern part of this island; and it is intended that it shall contain (in addition to the "Proceedings" of the Society), reports of the meetings of Scottish Natural History Societies; a record of Scottish captures; observations and discoveries both zoological and botanical, scientific jottings; notes and queries; lists of species for distribution in exchange, &c. As the carrying out of this scheme depends entirely upon the encouragement and support received by the Society, it is hoped that those interested will at once send in their names as subscribers to the honorary secretary, Mr. A. T. Scott, Clydesdale Bank, Perth.

THE *Saturday Review* of December 10 refers to "the gigantic Lycoperdon, or Puff-ball, which in one night acquires the bulk *of a child of ten years old*, and produces about 96,000,000 cells a minute!" We have both seen and heard of Puff-balls as large as a child's *head*, but one of this "bulk" is, we imagine, somewhat rare.

THE Folkestone Natural History Society has issued the following programme of its proceedings for the present (its third) winter session:—Nov. 8: Public lecture on "Solar Eclipses," Rev. C. L. Acland, M A. Nov. 23: Microscopical Conversazione. Dec. 13: Public lecture on "Food, and the Process of Digestion," F. Fagge, Esq, F.L.S. Jan. 10: Public lecture on "Fossils and their Teachings," Henry Ullyett, hon. sec. Feb. 1: Annual meeting Feb. 7: Public lecture on "The Natural History of Language," by W. J. Jeaffreson, Esq., M.A. Feb. 22: Ordinary meeting, paper, and discussion. March 7: Public lecture on "Iceland and Spitzbergen," by C. E. Fitzgerald, Esq, M.D., president. March 22: Ordinary meeting, paper on antiquities, and discussion. March 28: Public lecture on "The Materials for Antiquarian Research in S. E. Kent," by the Rev. Canon Jenkins. A class in Botany is held every Wednesday. at 4 P.M., by the Rev. C. L. Acland, and one in Geology every Monday at 8 P.M., by Mr. Ullyett. The Museum is open free on Thursday evenings and Saturday afternoons, as well as on the occasion of meetings; and a library of reference is in course of formation.

MR. EDWIN C. REED has been employed by the Director of the Museum of Santiago, in Chili, to arrange the insects. He has classified the foreign insects, which had hitherto remained unpacked. Mr. Reed is preparing a Chilian collection to send to California in exchange for a collection from that country.

THE United States Government commenced on the 1st of November the publication of a daily record of the state of the weather in various parts of the country. The following extract from the *New York Journal of Commerce*, dated Nov. 5th, shows the view entertained in the States on this subject:—"The Federal Government has lately done one thing which men of all parties will agree in commending heartily. It has made arrangements to furnish daily reports of the markings of the barometer and thermometer, the direction, velocity, pressure, and force of the wind, and the state of the weather, at points in different parts of the country. The shipowner, master, or merchant, reading this journal, has before him every day trustworthy advices of the weather all over the United States only a few hours old, and sufficiently fresh to apprise him of the danger or safety of sailing or making shipments from this port. Much depends on the accuracy of the reports, and for that we find good assurance in the fact that they are prepared by army officers, most of whom have received an education scientific and practical enough to qualify them for the work. The Associated Press have made an appropriation to have an observer among the party who will watch through the winter on the top of Mount Washington, White Mountains. From that point also we expect to receive daily bulletins, which will not only be serviceable to commerce, but in our coldest weather here may make people more comfortable to think how much colder it is away up on that windy peak. Prof. Hitchcock will be in charge of the party, which comprises a number of able scientists. The Chamber of Commerce is making a move to supplement the action of the Government at this point with a little more money." The places at present contributing "weather reports" are Augusta (Ga.), Boston, Buffalo, Cleveland, Cincinnati, Chicago, Cheyenne, Detroit, Duluth, Lake City (Fl.), Milwaukie, Montgomery, Mobile, Nashville, New Orleans, New York, Oswego, Omaha, Rochester, St. Paul, St. Louis. Toledo, and Washington.

A ST. PATRICK, says the *Pall Mall Gazette*, is evidently wanted in India as much as ever he was in Ireland. During the year 1869 no fewer than 11,416 persons in the Bengal Presidency died from the effects of snake bite. The return giving us this information has been carefully compiled; all the merely sick and wounded have been omitted, as well as those sudden deaths which in India are often attributed to snake-bites by heirs to property unduly eager for their inheritance. Such a mortality from such a cause is sufficiently startling to the sophisticated mind of a stay-at-home Englishman, but the more surprising fact remains that this destruction of human life goes on year by year, and that no efficacious means are adopted to check its ravages.

The *Pall Mall Gazette* states that we owe our supplies of Indian cotton to the American war, and we may have to refer the cultivation of tobacco in our colonies to the present campaign. A good deal of the leaf employed in the manufacture of cut tobacco comes to us from the Continent, and of course that source is now closed; but the smoker may be consoled by hearing that India, Jamaica, and Natal are all engaged in cultivating the plant. Latakia from the West Indies has been received in London and reported upon favourably, and the samples of tobacco from Natal are said to be remarkably good. What the Indian Government has already done for the cultivation of tea, cinchona, and ipecacuanha, it is now doing for the tobacco plant. Seeds of the best varieties have been distributed in suitable districts, and the time may speedily come when the Bengal cheroot may be a production of the Presidency.

FROM the report of the cotton department of India it seems that the crops in the several districts have, during the past year, yielded a satisfactory return. We also learn that a very extensive system of adulteration exists by mixing two or more qualities in one bale, to which a stamp, indicating a superior quality, is affixed. With regard to the use of English ploughs which have been introduced, it is said that "the cohesion and tenacity of the richer black cotton soils are evidenced by the manner in which they get rent and cracked into deep fissures instead of becoming pulverised by the rapid contraction they undergo when exposed to the fierce rays of the sun after the crops have been removed, and from the same cause the upper surface bec [illegible] baked and hardened into a crust, which is about a [illegible] to the plough as if it were a pavement. Th [illegible] accordingly to wait until the advent of the mons [illegible] and removed this crusty impediment, and, if [illegible] that the first crusts are very heavy and con [illegible] further delayed from an opposite cause, n [illegible] his fields having become too soft for his [illegible] that he has to wait for a favourable breal [illegible] moisture." In this state of the land [illegible] and the animals have not strength to move the instrument, so that "both ploughs and ploughmen succumb, and the antediluvian implement of the ryot is found to be the only feasible one."

SOME idea of the damage done to vegetation by locusts in tropical countries may be gathered from the following account of a raid made by them in an East Indian cotton plantation. The means adopted to repel them was by recourse to the discordant sounds of native music—horns, tom-toms, and pipes—aided by the waving of flags and branches of trees. These measures, undoubtedly, saved the produce; for, judging by the performance of the very small number that succeeded in gaining admission to one of the finest fields unobserved, had a full complement effected a lodgment, one hour would have sufficed to strip every tree of its leaves, though the foliage was abundant, and the plants in one field from five to six feet high. The immunity which the native Indian cotton enjoyed from the attacks was considerable, considering the avidity with which they devoured the exotic descriptions, and, true to their early traditions, the Egyptian was evidently an especial favourite. Some of the swarms that passed over the country at that time were exceedingly numerous. The arrival and settlement of one mighty mass was a remarkable sight. What was first observed was a sort of haze on the verge of the horizon, in a long line, as if a steamer had passed, and its smoke was rising into vapour; this was some hours before the insects arrived. The cloud gradually thickened, and rose higher as they approached. When they got fairly overhead the air became darkened as if night was setting in, it being yet mid-day, and the peculiar sound which accompanied their flight resembled that of the rustling of the leaves of the peepul tree when agitated by light winds; but it is not until they have settled down that any idea can be formed of the immensity of their numbers, and the early dawn, before sunrise has warmed them into life and motion, is the time to witness this most extraordinary sight. In the instance now referred to the appearance the face of the country wore would be best described by supposing that a tolerably heavy fall of snow had taken place, only that the colour of it was a light brown, and this extended for miles, as far, indeed, as the eye could reach. Trees were favourite perching-ground for the night, and the manner in which they contrived to crowd upon them, piles over piles, concealing every vestige of leaf and branch, gave the trees a singular appearance. At one spot a stout and wide-spreading branch of a banyan tree had snapped at its stem from the incumbent weight of the insects.

RICH mines of gold and silver are being daily discovered in the State of Tulima, in Columbia, according to late advices.

A RECENT number of the American *Journal of Chemistry* contained the following story of the first introduction of the stereoscope to the *savants* of France. The Abbé Moigno took the instrument to Arago, and tried to interest him in it; but Arago unluckily had a defect of vision which made him see double, so that on looking into the stereoscope he saw only a medley of four pictures. The Abbé then went to Savart, but he was quite as incapable of appreciating the thing, for he had but one eye. Becquerel was next visited, but he was nearly blind, and conse[illegible] cared little for the new optical toy. The Abbé, not [illegible] called next upon Puillet, of the Conservatoire des [illegible] He was a good deal interested in the descrip[illegible]atus, but unfortunately he squinted, and there[illegible]othing in it but a blurred mixture of images. [illegible]ied, but Biot was an earnest advocate of the [illegible] light, and until he could be assured that [illegible]id not contradict that theory, he *would* not [illegible]ler the circumstances, the wonder is that [illegible]airly into France.

MIMICRY AND HYBRIDISATION [*]

SOME time since I had occasion to study with care, for the purposes of a work on which I am engaged, the phenomena of mimetic analogy made known by Mr. Bates, which have lately formed the subject of discussion at the British Association, and in the pages of NATURE, in which I observe with pleasure that one of our body, Mr. A. W. Bennett, has borne an honourable part. Neither he nor any of the gentlemen who have written on the subject, have, however, so far as has come under my notice, brought the point to its real issue. They have accepted battle on the field on which Mr. Bates has placed it, and although they may have achieved a victory over him, they have not succeeded in rescuing the subject from its obscurity. He may be wrong without their being right. I am not surprised at their having been led to accept his premises; when I first approached the subject I did the same; but the longer I live and the more extended my experience becomes, the more surely do I find that when a theory looks shaky and unsound, the place to look for the flaw is not in the upper story, but in the basement. It is in the foundation that the crack will almost invariably be found. I am sure it is so here.

Mr. Bates found in the Valley of the Amazons a number of species of a Northern tribe of butterflies, wearing the colour and form of a Brazilian tribe, and so like in their varieties and strains that they obviously represent some different phenomenon from the ordinary one of mere difference in species. To account for this he devises a theory on the Natural Selection plan. The Brazilian tribe has a bad smell, and birds and insects of prey consequently do not feed upon them, and the Northern tribe, in the course of their variation in the dark, accidentally produce one something like the Brazilian one, which produces others in the same direction by Natural Selection, until the mimics are brought to perfection. Every inch of the ground he goes over here is mined and unsound—the bad smell has not been observed in North America where similar mimicry occurs—birds and insects of prey hunt by sight and not by smell, and the various communications on the subject in NATURE point out a variety of other insuperable objections. But my object is not so much to show that a friend and entomological brother has been seduced by a "bad smell" to go on a wrong scent, like a good dog after a red herring, but to find out the true explanation of the phenomenon.

The explanation seems to me to be simply Hybridisation; but before committing myself to it, as there were one or two points on which I was not sure how far the phenomena corresponded with those of hybridisation in plants, I applied to my friend Mr. Isaac Anderson Henry for information upon them, and he has sent me a paper (for the Scientific Committee of the Horticultural Society), as well as some other information, which enables me now to say that there is not a phase or a fact in the mimicry in question, for which I cannot produce the exact counterpart in the hybridisation of plants.

In the first place the mimicked and the mimickers are always found together, and even the mimickers of varieties are only found beside the varieties that they mimic. Now, it is plain that if the resemblances be due to hybridisation, it is inevitable that the two must always be found together, at least in the first instance. It may be that after the hybrids are established and advanced into the position of actual species, the species (*i.e.* the parent and offspring) might diverge from their primary locality, the one to the right and the other to the left, and so cease to be found together; but this must be an after act, and consequently an exception. The natural condition is to find both together, and so they are always found together. But this would not be the natural condition if the mimicry were produced by Natural Selection. The same enemies are found over thousand of miles, and the same kind of enemies over tens of thousands; and there is no advantage to be gained by mimicking one variety of Danais more than another. The same advantageous results would be obtained by mimicking in the east the form that prevails in the west, or in the north the form that prevails in the south, but the imitation of each variety is limited to the district which it inhabits, however narrow and restricted it may be. Natural Selection, therefore, fails entirely to account for the localisation of the mimickers of varieties.

In the next place the mimicked occur always in overwhelmingly greater numbers than the mimickers. Mr. Bates says:—"The Ithomiæ (Danaids) are all excessively numerous in individuals, swarms of each kind being found in the localities they inhabit. The Leptalidæ (mimics) are exceedingly rare; they cannot be more than 1 in 1,000 with regard to the Ithomiæ." This is quite what we should expect if the resemblance is due to hybridisation. Hybridisation is not the normal mode of producing either species or individuals. It is not the plan laid down by Nature. Being exceptional, it is, of course, comparatively rare. But there is no reason for rarity if it be the result of Natural Selection. That operation is going on equally upon all, and under that hypothesis mimicry is just as powerful an influence in modifying and producing forms as any other; and there is no reason whatever why it should have less conspicuous results; indeed, it should have more, if we judge by the long-continued persistence of influence which must have been in operation to produce such exact resemblances; and which, indeed, seems very much thrown away when confined to the 1 in a 1,000 mentioned by Mr. Bates.

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[*] This paper was originally presented to the Scientific Committee of the Horticultural Society (Dec. 7, 1870), but has not yet been published elsewhere.

Although mimicry occurs between various tribes or genera, it has been observed most frequently in connection with the most common species of the country. This is what would naturally be the case with hybridisation, supposing all to start fair and to be equally liable to hybridisation. But this is an assumption which we are scarcely warranted in making, and I therefore do not press this inference further than as of some conditional value.

After the second generation of hybrids in plants, it was first shown by M. Naudin, and is now well known to all hybridisers, that those which do not revert to type break out into an overflow of irregular variation, which supplies many of his most remarkable sports to the horticulturist, and many of his most puzzling difficulties to the systematic botanist. On the assumption that the mimicry in question is the result of hybridisation, we should therefore expect to find a marked degree of variation among the mimicking species. And so we do. Mr. Bates figures no less than fifteen varieties of *Leptalis Ithomiæ*, one of his mimics, which itself mimics seven different species (all very close to each other, however, and perhaps scarcely deserving the name of independent instances.) Mr. Trimen figures six varieties of *Papilio Merope*, which supplies four of his instances of mimicry, and Mr. Wallace's imitating Papilios were in like manner remarkable for their variations. It seems a fair inference that when the mimicking species are not variable they have been established before the second generation of hybrids, and where they are variable they have been established subsequent to the second generation, and have experienced the usual shock to stability occasioned by such repeated loosening of the fetters of specific identity.

Mr. Bates's list of mimics and mimicked species shows, too, that when a species is mimicked by one species or genus it is often mimicked by more, a fact which, applied to the idea of hybridisation, simply means that that species had a readiness to take to itself wives of more than one of the nations round about. Out of twenty-eight Danaoid species cited by him, which had been mimicked or had families from strange husbands, fourteen had families from one each, three from two each, and six from three each. It is only what we find in plants, that some are more open to hybridisation than others; or perhaps, analogous to our moral experience, that where scope is allowed to our own passions, license soon degenerates into libertinism.

Another feature, familiar to all hybridisers, occurs in these mimicries. Notwithstanding the statement of Wichura to the contrary, it is now perfectly well known that in attempting to obtain a cross between two species we often fail when we work with the male of one species and the female of the other, while we succeed when we reverse the process and take the male of the latter and the female of the former. In plants the cases where this capability of crossing in only one direction occurs are beyond number. Mr. Isaac Anderson Henry cites many of them in his late Presidential Address to the Botanical Society of Edinburgh, and in the paper which I have now the pleasure to lay before the Committee. The very same thing has occurred with the mimicries recorded by Mr. Bates. They are all on one side of the house. According to my view (indeed if hybridisation is once allowed to have been the motive power, it must be according to every one's view), the parents were the Danaids on the one side, and the cabbage whites (Pieridæ) on the other, for all the mimicked are Danaids with their special characters, viz., only four apparent legs, while all the mimickers, like the whites, have their special characters, six legs apparent. If they had been hybridised from both sides, we should have had some Danaids with the form and colour of the whites, as well as whites with the form and colour of Danaids; but we have not. The case which so often occurs in plants has obviously occurred here. The cross was taken only from one side. Which is it? I apprehend, from other examples, that it should be on the side of highest organisation—that is, that the male parent has been of the lower organisation, and the female parent (the actual bringer forth) of the higher. Now, which is the side of highest organisation in the Danaids and Pieridæ? Is it that of greatest strength? If it were so, it would then be the Danaids, for they are larger, finer, and more powerful than the more northern whites. But organisation is a higher test than mere strength. This, too, seems to be on the side of the Brazilian tribe. Mr. Bates so considers it, and his reason is that, the essential quality of butterflies being flight, the type which has most attention paid to its wings and least to its legs, must be highest of its order. Others think differently, and say that a type which has had two of its limbs (its anterior legs) almost atrophied, cannot be so perfect an animal as one which has them all in perfection. But I agree with Mr. Bates on this point (at all events in his conclusion). The greater number of legs cannot be any indication of higher organisation, or a centipede might dispute supremacy with ourselves, and push us from our stools. Multiplicity of sub-division or repetition of parts is acknowledged by all physiologists to be an indication of inferiority of organisation. The fewer limbs, that is the simpler the apparatus that a creature can do its work with, the higher the perfection of the machine. Therefore, doubtless, Brazilian Danaids are the higher type, and if (as I believe to be the case) in crosses of difficult accomplishment, the female is the higher parent, then the cross from which these mimics resulted was one by the males of the whites upon the females of the Danaids.

In what I have above said as to one-sided crossing, I have assumed that in plant-hybridisation the fact would be admitted; but as it is in contradiction to the statement of so eminent an authority as Wichura, I shall remove all doubt from the subject by quoting Mr. Anderson Henry. He says:—"I regret to differ from so great an authority as Wichura (who has maintained that 'the products which arise from reciprocal crossing in plants, unlike those which are formed among animals, are perfectly alike'), and must venture to demur to the doctrine in more decided terms than Mr. Berkeley does. I have had so many instances of hybrids taking sometimes to one side and sometimes to another, *but most frequently to that of the mother*, that to those who, like me, have tried their hand with many genera, it would be a matter of supererogation to give instances. I have had them by the score."

But the mixed product also corresponds with another fact observed in hybridisation. Mr. Henry informs me that in some of his crossings of plants he has only succeeded in altering the flowers, the foliage continuing persistently the same as that of one of the parents. He has not succeeded in distributing the union through all parts. That is exactly parallel to what we see in these mimicries. The number of legs and the nervation of the wings (in other words the more structural portions of the animal) remain special as in one parent, while the colour and form of the wings, &c., is taken from the other. In the butterflies it is the more structural parts (legs, nervures of wings, &c.) of the male parent which are observed in the offspring, while the form and general appearance only of the female parent is adopted. In plants it may be a question whether we should consider the flower or the foliage as the more structural parts—for my part I should take the flower as the more important, and therefore equivalent to the structure of the legs and wings; and the foliage and habit of the plant in Mr. Anderson Henry's case as equivalent to the colour and form of the wings and general appearance of the insect. Another phase of the mimicry, which I have no doubt will be found to have also its parallel in the hybridisation of plants, although I am not able to cite any instances exactly in point, is that in species which have dissimilar sexes, it sometimes extends to both sexes, the males being like the males and the females like the females, but in other instances is confined to the females. I believe that the reason why I have no case in point to cite in plants is that it can only be had in diœcious plants, and the hybridisation of diœcious plants has hitherto been scarcely at all attended to. Mr. Henry has some coming forward, but they have not yet flowered.

The last point to be noticed is one of some importance, as being the only one furnishing a shadow of objection to the explanation of the mimicries in question by hybridisation. It is that the nearest natural allies of both the mimickers and mimicked are *not* always to be found in the same district. This deserves the more attention, since it appeared so strong to Mr. Bates as to lead him to relinquish the idea of hybridisation as an explanation after it had crossed his mind. "The explanation," says he, "that the whole are the result of hybridisation from a few originally distinct species cannot at all apply in this case, because the distinct forms, whose intercrossing would be required to produce the hybrids, are confined to districts situated many hundred miles apart."

Before I proceed to show how simple the explanation of the absence of one of the parents is, I must beg to note in passing the admission that there are distinct forms whose intercrossing would produce the hybrids. That granted, I would remind the reader of what Mr. Bates has obviously overlooked, that we are dealing with a phenomenon probably of a very ancient date, and that one side of the parental stock may have disappeared in the course of time. I have elsewhere suggested, in regard to hybridisation as a possible originator of species, that it must be a necessary accession to such an event that the hybrids should have opportunity of isolation, such as might be obtained by thinly peopled districts where they might settle, spread, and establish themselves. Now, certainly, the Valley of the Amazons, the Malayan Archipelago, and many parts of the South of Africa (lands whence these mimetic analogies come) have at different periods all been at one time unoccupied land; for all of them have been raised from the bottom of the sea, and been peopled by the influx of the inhabitants of neighbouring lands. No one knows better than Mr. Bates that at one time Brazil was unconnected with New Granada or the Andes. The Danaids were then inhabitants of it, but not inhabitants of the countries about it; while the Pieridæ, or cabbage whites, were what I have elsewhere denominated a microtypal tribe from more temperate climes, and were present in the Andes and the mountain countries, as Columbia, connected with them. In the natural course of things, therefore, when the Valley of the Amazons was changed from the bottom of a sea to dry land, the Danaids would spread into it from Brazil, and the Pieridæ from the north and west, and meeting in an open, as yet, unpeopled country, hybridisation might take place under one of the few circumstances where I have thought it possible that it could retain its place and establish its products as species. The objection that frightened off Mr. Bates is, in reality, no objection at all to the hypothesis of the mimicry being due to hybridisation, that we are not always, or even that we should not at all be able to identify the probable parents of the mimickers as inhabitants of the same country as their supposed descendants. One of the parents we know to be present (the so-called mimicked), but there are excellent reasons why the other parent should not be present. It is of a northern type, suited for our temperate regions, but not adapted to the tropics except at a higher elevation and a cooler temperature than the damp, hot valley of the Amazons. Although, therefore, it might descend into that region, it is not only a natural but almost a necessary inference that it would not find it congenial or habitable, and although it might live long enough in it to found a dynasty of mimickers, it would soon die off from unsuitable conditions, while its hybrid offspring bred from the tropical Danaids might, from the black blood so imparted to them, find it sufficiently well suited for them.

There is yet another phenomenon connected with Mimicry, which possibly may also be connected with hybridisation, viz., the occurrence of what Mr. Wallace has called dimorphism in insects among the mimicking or mimicked species. We must not, however, confound this dimorphism with Darwin's dimorphism in plants. The two are totally different things, and, as it seems to me, have no relation or analogy to each other. In plants the dimorphism is always confined to the reproductive organs, in insects it has apparently nothing to do with them. Moreover, it seems to me that all the instances of so-called dimorphism in insects that have yet been recorded, are nothing but examples of variation, perhaps complicated by hybridisation. M. Reinhard, of Bautzen, has shown that this is the case with regard to Mr. Walsh's conclusions respecting the dimorphism of certain gall-flies, for he had found that the galls of various species appear to be so transitional between other forms, that they can only be known with certainty when the perfect insect appears. It appears to me to be also the case in all those instances where the dimorphism is confined to particular districts, as in the *Papilio Turnus* of North America, where all the females are yellow in the New England States and in New York, while in Illinois, and farther south, they are all black, and in the intermediate region, both black and yellow females occur in varying proportions. And the case is not open to any doubt, because in the intermediate district, both yellow and black insects have been bred from the same batch of eggs. Now, if the case had been that *both males and females* equally varied, and that in the south all were black and in the north all yellow, with intermediate gradations in the districts between, we scarcely suppose that any one would have thought of calling it a case of dimorphism. If they did, then all climatal variations (and their name is legion) would come under the same category. It is only dimorphism, because the change is limited to the female. But is this a good ground? Physiologists are unanimous in holding that neither the male nor the female is the species, but both; and if that be the case, in what does a variation in the female and not in the male differ from a variation in both but in degree? Most of Mr. Wallace's instances of dimorphism are of this character—the male being the same in a number of islands in each of which the female differs. All these I regard as mere instances of climatal variation, in which the variation shows itself only in that part of the species called the female. An occasional case of variation from some other cause, as from hybridism, may possibly come to complicate this phenomenon; but it appears to me to be sufficiently explained by variation, and the circumstance above mentioned is significant that where mimicry occurs in species having dissimilar sexes, it too is often confined to the female.

A. MURRAY

SCIENTIFIC SERIALS

Silliman's Journal, September 1870.—The opening article of this number is by Prof. E. Loomis, and is entitled "Comparison of the mean daily range of the Magnetic Declination, with the number of Auroras observed each year, and the extent of the black spots on the surface of the Sun." The author first discusses the observations of sun-spots, and points out some corrections that should be made in the numbers obtained by astronomers in the last century; he points out that the period is one of ten years and is influenced by the heliocentric conjunctions of Jupiter and Saturn, but affected by the conjunctions of the Earth and Venus. By a series of tables and curves the coincidences of periods of the maximum number of sun-spots with the maxima of magnetic disturbance and auroral display are elucidated, from which it appears that the present year is a period of maximum.—In a letter to the editors, Mr. J. W. French proposes *a new period in chronology called the Precession Period*, of 25,782 years, being the time for the precession of the equinoxes. The author prefers this period, since it is founded solely on astronomical facts.—The third article is by F. W. Clarke, "On the atomic volumes of solid compounds," in which are discussed the relations of the volumes of analogous and similarly constituted bodies.—The next article, "Considerations on the apparent inequalities of long periods in the mean motion of the Moon," is by Simon Newcomb, and, after a long discussion on the observations on this subject, and the theories proposed to explain them, the author attributes the phenomenon to an irregularity in the rotation of the crust of the earth, caused by the motion of its fluid contents.—The following is a very interesting article by Dr. A. M. Mayer on "Researches in Electro-magnetism." The author has devised a very accurate method of determining the relative values of electro-magnets to replace the one usually employed, which consists of measuring the deflection of a magnetic needle which is produced by the action of the electro-magnet. The author found that this process was liable to error in consequence of the difficulty of keeping the current absolutely constant, resulting in a continual motion of the needle. These difficulties were obviated in the following manner: A line eight feet long and divided into fractions of inches was drawn on a table, the latter being so placed that the line was at right angles to the magnetic meridian; a compass, with a needle nearly six inches long, was placed on this line, and a helix was fixed at each extremity of the line. These helices were traversed by the same current, a tangent galvanometer being placed in the circuit. In this way the needle was influenced by two magnets acting in opposite directions and excited by the same current, and if any deflection of the needle was observed, it must have been due to a difference of power of the magnets. If this occurred the needle might be brought to 0° by moving it from the stronger magnet. A series of experiments was made to determine the variation of the intensity of the force with a change of distance, by placing the needle opposite an electro-magnet and noting the deflection produced when the instruments were at different distances from one another: it was found that in the apparatus employed the intensity varied inversely as the 2·7404 power of the distance from the core. Dr. Mayer has determined the power of cores made of insulated and of non-insulated soft iron wires, and finds that the insulated core is slightly the weaker. He has also measured what thickness of tube is equal to a solid core of the same diameter, and has found that a solid cylinder ten inches long and 1·68 in diameter may be replaced by a tube of the same length and of a thickness of $\frac{1}{8}$ of the diameter. This relative size does not appear to be constant for cores of all dimensions. A longitudinal slit in the tube does not diminish its power; in fact, Dr. Mayer seems inclined to think that it facilitates its magnetisation. By placing a helix inside a soft iron tube a magnet is produced with poles the reverse of those of the coil, or of a bar placed within the helix; this supports Ampère's theory of magnetic currents. Numerous other experiments are detailed in this paper, and the author promises to employ his apparatus for the determination of the force of magnets of different sizes.—Mr. G. F. Barker contributes an abstract of the second series of Professor Meissner's researches on electrised oxygen, in which are detailed the author's experiments on the substance formed simultaneously with ozone which possesses the property of producing a white mist in contact with water. The original paper was published by the Gottingen Royal Society of Sciences.—Mr. A. E. Verrill describes a new species of Entozoön from the Hog. This is followed by some notes on the structure of the Crinoidea, Cystidea, and Blastoidea, by E. Billings, F.G.S.—The next article consists of contributions to chemistry from the laboratory of the Lawrence Scientific School, the first paper of which is by W. G. Leison, on the precipitation and determination of the metals of the magnesium group in the form of oxalates. For this purpose the solutions containing the metals are mixed with oxalic acid and alcohol, the precipitated oxalates washed, dried, and dissolved in hydrochloric or sulphuric acid, and the quantity of oxalic acid present estimated by means of a standard solution of potassic permanganate. A number of examples show the accuracy of the process.—J. H. Talbott describes the precipitation of zinc and manganese as sulphides, and the quantitative separation of tin and tungsten by fusing with potassic cyanide, by which the tin is reduced to the metallic state.—A new mode of treating gelatinous precipitates is suggested by T. M. Chatard, which consists in evaporating the liquid containing the precipitate to dryness, and stirring until the mass becomes a dry powder, which is then readily washed on a filter.—S. P. Sharples points out that antimonious sulphide precipitated by sulphuretted hydrogen in boiling solutions is granular and easily washed. Arsenious sulphide does not behave in a similar manner.—In the fifth section B. Godwin advises the repetition of quantitative analyses with the same quantity of material, the mean of the results being taken.—The next article is by Professor W. A. Morton on the corona seen in total eclipses. He calls the attention of astronomers to the importance of determining the positions of the more prominent portions of the corona with reference to the equator of the sun.—Dr. Finlay contributes observations on prehistoric archæology in Greece.—The remainder of the journal consists of extracts from other journals principally European.

THE *Geological Magazine* for December (No. 78) opens with a curious paper on Earthquakes, written about the year 1798 by Sir John Prestwich, an ancestor of the President of the Geological Society. This paper is interesting not only as showing the absurdities which passed as science not much more than seventy years ago, but also as giving a list, derived from old historical works, of the occurrence of earthquakes in England.—The most important article in the number is a continuation of Mr. H. Woodward's Contributions to the Knowledge of British Fossil Crustacea, containing descriptions of species of the curious genus *Cyclus* from the British Carboniferous rocks. Many of the species are described as new, and most of them are well figured. There is also an interesting article by Mr. George Maw, on Recent Changes of Level in the Coastline of the Mediterranean; and Mr. J. F. Walker describes and figures some *Brachiopoda*, from the Lower Greensand of Upware in Cambridgeshire, two of them as new species. Other papers are: "On the Age and Position of the Blue Clay in the West of England," by Miss C. Eyton; and "On the Dispersion of Granite Blocks over the Plain of Cumberland," by Mr. D. Mackintosh.

THE most important paper in the *American Naturalist* for November is one presented by Prof. Agassiz to the Troy meeting of the American Association for the Advancement of Science, "On the former existence of local glaciers in the White Mountains." He conclusively shows that whatever may have been the number of its higher peaks which at any given time during the glacial period rose above the great ice-sheets that then covered the country, this mountain range offered no obstacle to the southward movement and progress of the northern ice-fields. To the north of the White Mountains, as well as to the south, the northern drift consists of a paste more or less clayey or sandy, containing abraded fragments of a great variety of rocks, so impacted into the minutely comminuted materials as to indicate neither stratification, arrangement, nor sorting, determined by the form, size, or weight of these fragments. Large boulders and pebbles of all sizes are found in it throughout its thickness, and these coarser materials have evidently been ground together with the clay and sand under great pressure, beneath heavy masses of ice; for they have all the characteristic marks so unmistakeable now to those who are familiar with glacial action, scratches, grooves, furrows, &c. We have also articles "On the Habits and Migrations of some of the Marine Fishes of Massachusetts," by J. H. Blake; "What is the Washington Eagle?" and "On the Distribution of the Moose in New England," by J. A. Allen; "Notes on certain Inland Birds of New Jersey," by Dr. C. C. Abbott; and two reprints, "On the Cultivation of Alpine Flowers," and "Acclimatisation of Foreign Trees," both from the *Quarterly Journal of Science*, by Mr. A. W. Bennett. Further abstracts are also given of papers read at the recent meeting of the American Association for the Advancement of Science.

SOCIETIES AND ACADEMIES

LONDON

Royal Geographical Society, December 13.—Sir H. Bartle Frere, vice-president, in the chair. The following new fellows were elected:—Daniel David Dymes; Colonel T. G. Glover, R.E.; R. M. Gordon; Captain L. W. Longstaff, Edward Masterman, jun.; Don Pompeio Moneta (Chief Engineer Argentine Republic), Charles Pannel; Alfred Robinson; G. S. T. Scobell; and C. A. Winchester.—Lieutenant G. C. Musters, R.N., read a paper on his recent journey through Patagonia, from the Straits of Magellan to the frontier of the Argentine Republic. The author, having determined on this journey, landed at the Chilian penal settlement of Punta Arena, in the Straits, on the 15th April, 1869, and, having procured the goodwill of the governor, was permitted to accompany a party who were despatched across the country to recover some runaway convicts at the mouth of the River Santa Cruz. Here he made a friendly arrangement with Orkeke, the cacique of a tribe of Patagonians, to traverse the country with them as far as the Rio Negro. He studied their language and manners, and joined them in their hunting parties: the country abounding in game, chiefly guanaco, the three-toed ostrich, and the puma, or American lion, the latter of which was eaten as well as the rest. Frozen rivers and heavy snow-falls prevented their starting from Santa Cruz before the 12th of August. They travelled at first in a westerly direction, until reaching the foot of the Cordilleras, along which they marched for upwards of 700 miles to the upper waters of the Rio Negro, making a short, but important, detour across the River Limay, in the Cordillera due east of Valdovia. The author described the streams crossed throughout the route, the physical nature of the country, and its chief productions, and gave also long and most interesting details of the manners of the wild tribes, including an account of hostile encounters with other tribes. He stated that, when not excited, the Patagonians manifested a good-tempered and generous disposition, and that they were remarkable for their affection to their wives and children. The women have the whole charge of the tents, constructed of poles and guanaco skins, and the march of many months was an almost continuous chase after the game of the country. Every morning the chief gave his orders for the day in a set speech. The men, on starting, spread themselves over a wide space in the plains, in a crescent form, the more advanced of whom on each side, travelling fastest, as the whole cavalcade moves on, meet in front, and thus enclose the game in a circle; the women and children, with the baggage-horses, forming the base line of the crescent. In the earlier part of the journey four such marches were made in succession, averaging eight or ten miles each; then followed a rest of several days, in places where pasture was abundant. Lieutenant Musters was altogether more than a year with the tribe, who had come to look upon him as one of themselves. In May 1870 he crossed the country again from west to east, and on the 21st of that month arrived at the Argentine settlement of Patagonia, near the mouth of the Rio Negro. The climate of the country, in which he reached north of 40° S. lat., he describes as cold and ungenial; snow fell at midsummer, and the greatest heat experienced in the warmer months was only 65°.

Geological Society, December 7.—Mr. Joseph Prestwich, F.R.S., president, in the chair.—1. "On Fossils from Cradock and elsewhere in South Africa." By Dr. George Grey. From the Karoo-beds, Dicynodont fossils and the jaw of a reptile (*Estheriæ*), and some coal and coal-plants (*Lepidodendron*, *Sigillaria*, &c.), were the chief specimens noticed by the author. Some *Stigmariæ* from the Old Coal of Lower Albany, and gravel and miscellaneous minerals from the diamond fields, formed part of the collection. 2. "On some Points in South African Geology," Part II. By Mr. G. W. Stow. This paper commenced with a detailed account of the forest zones, coal, and other strata of the Karoo formation, as seen in sections in the Winterberg and Stormberg. The author particularly pointed out the position of the fern-beds at Dordrecht, of the Reptilian remains found on the Upper Zwartkei, and of the coal on the Klass Smits River. He next referred to the climatal changes of South Africa, as indicated by its geology and fossils, particularly the Karoo-beds, the *Enon* conglomerate, the *Trigonia*-beds, the several Post Tertiary shell-beds, and especially the present surface conditions, which he regarded as due to ice-action, as evidence of which he adduces *roches montonnées*, moraines, basins, and striæ, both north and south of the Stormberg, in British Kaffraria, and even in Lower Albany. He concluded with remarks on the probable succession of periods, and on the former existence of a great southern continent. Prof. Ramsay expressed a hope that the author at some future time would discuss the numerous subjects of which he treated at greater length and under separate heads. He was not surprised at the finding of Carboniferous plants in the Dicynodont beds which appeared to be of Triassic age, inasmuch as the same was the case to some extent in our own later beds of Oolitic date. He agreed in the view of the probability of a vast continent having formerly existed in the southern part of the world, and considered that the denudation of Southern Africa had been so great, that it was no wonder the boundaries of the old freshwater lakes were no longer easy to find. It was also by no means surprising to him that a recurrence of glacial phenomena should be found in Southern Africa, as it had been in Europe. He did not, however, think it necessary to call in the action of ice for the excavation of valleys such as some of those described, as rain and running water appeared to him sufficiently powerful for the purpose. At the same time he would not deny the possibility of ice having been the agent in these cases. Mr. R. Tate had seen evidence of similar effects being produced by aqueous force to those resulting from glacial action, and cited instances of moraine-like deposits having been formed by running streams in Central and Southern America. Mr. H. Woodward suggested that it would be desirable to wait for further particulars of the sections before asuming the actual association of the *Lepidodendron* and other plants. He added that the *Stigmaria* lately said to have been obtained from the Kimmeridge clay, had really come originally from Newcastle. Prof. T. Rupert Jones remarked that Mr. Stow, like other South African geologists, had had ample experience of the effects of violent rain. With regard to the mixture of Palæozoic plants, such as the *Lepidodendron*, &c., sent by Dr. Grey with *Palæozamia* and *Pecopteris*, he thought it somewhat analogous to the mixture of palæozoic and mesozoic fossils in Australia. 3. "On the Geology of Natal, in South Africa." By Mr. C. L. Griesbach. The author commenced by describing the physical geography of Natal, and then indicated the characters and distribution of the rocks which occur in that country. He stated that the granitic and gneissic rocks do not form the most prominent elevations, but they appear chiefly in the lower parts of the river-valleys, and sometimes in small hills. Mica-schists and slates are found associated with the granites. The great plateaux consist of an undisturbed sandstone, which the author identifies with the Table-mountain sandstone, and which lies horizontally upon the granites and old slates. The tops of many of the table-mountains in Natal are crowned by beds of dark basaltic greenstone. The Karoo formation, which lies in part upon the Table-mountain sandstone, consists of a vast series of sandstones and shales, some of the latter containing beds of coal. The author agreed with Mr. Tate in regarding these beds as of Triassic age. At the base of the Karoo formation the author described a boulder-bed, which he was inclined to identify with the rock described by Mr. Bain as "Claystone porphyry," and through this greenstone has forced its way. On and near the coast of the southern part of Natal, some sandy marls and sandstones belonging to the Cretaceous series were said to occur; the author gave lists of fossils obtained from these deposits, which he identified with the Trichinopoly series of India. Several of the fossils were described as new species. The author considered that the evidence adduced indicated that, after the development of the Table-mountain sandstone, Africa and India formed parts of one continuous continent, afterwards covered by the Cretacous sea. The area now covered by the Indian Ocean was the basin of a large series of lakes; and this condition persisted through a long period of tranquillity, lasting through the Triassic to the Upper Jurassic age. The greater part of this continent was then depressed and covered by the shallow Cretaceous sea. The economic mineral products of Natal were mentioned by the author, who referred to the occurrence of graphite, coal, gold, and copper. Prof. T. Rupert Jones commented on the importance of the paper as throwing so complete a light on the geology of Natal, and proving the geological sequence to be similar there to that in other parts of Southern Africa. He remarked that the author had done special service by the great increase of information furnished by him regarding the Cretaceous rocks of Natal, and their equivalence to those of India. He also pointed out that Mr. Griesbach had proved that the Karoo formation was continuous to the other side of the great dividing range, and formed the floor of the Orange and Waal Valleys, and that as Mr. Stow had indicated glacial action on the south side of the Orange Valley, it was quite possible that the gravels containing the diamonds were of local origin, as Dr. Grey had suggested. 4. "On the Diamond-Districts of the Cape of Good Hope." By Mr. G. Gilfillan. Mr. Gilfillan described his going through Colesberg to Hopetown, and thence across theOrange River to Backhouse; and then, after crossing the Vaal, up its right bank as far as Lekatlong. He noticed such diamonds as he saw or heard of, and described the locality as being thickly coated with sand, diamond-bearing gravel, and tufa; hard blue shales occurring here and there in protruding hills. Prof. Tennant stated that he had lately seen as many as 500 diamonds from the South African fields in the possession of one person, some weighing as much as 50 carats. He had seen another fragment of a stone which must have originally been at least as large as the Koh-i-noor.

Ethnological Society, December 13.—Professor Huxley, president, in the chair. Mr. E. Rowley Morris was announced as a new member. Mr. Grove, Q.C., exhibited a dozen skulls from a large collection in the crypt of Rothwell Church, in Northamptonshire; and Professor Busk, F.R.S., made some remarks upon their anatomical peculiarities. The skulls are, on an average, smaller than those of the existing race, and many exhibit an extreme lowness of forehead. Some of them are referable to Prof. Huxley's "river-bed" type. The discussion on these remarks was sustained by the President, Mr. Galton, and Mr. Evans.—Sir John Lubbock, Bart., read a paper "On Stone Implements from Africa," in which he described some implements of the spear-head type from the Cape of Good Hope, and some small polished celts brought by Mr. Reed from near Accra on the Gold Coast. He also exhibited a small, but exquisitely-worked flint implement found in Syria. Mr. A. W. Franks, Mr. W. Blackmore, Mr. E. B. Tylor, Mr. J. W. Flower, Mr. Hyde Clarke, and Mr. E. B. Pusey, took part in the discussion on Sir John Lubbock's paper.—A collection of stone implements was exhibited by Dr. Hooker, C.B.—Some notes from Mr. Edgar Layard were read relative to some stone spear-heads, hammers, flakes, cores, &c., from South Africa.—A second report "On the Present Condition of the Prehistoric Antiquities of Dartmoor," was presented to the Society by Mr. C. Spence Bate, F.R.S. The author described in detail several stone circles, bee-hive huts, and avenues on the south of Dartmoor, especially in the neighbourhood of the Avon and the Erme. On Trowlsworthy Tor is a curious circular enclosure, with two entrances so constructed as to admit only a single man at a time, which the author regards as a specimen of early military engineering.

Linnean Society, December 15.—Mr. Bentham, president, in the chair. Dr. J. Lindsay Stewart was elected a member of the council in the room of the late Dr. Anderson.—"On Sabadilla (*Asagræa officinalis* Lindl.) from Caracas," by A. Ernst. A large quantity of this drug is exported from Caracas and Venezuela, amounting to from 3,000 to 3,500 quintals annually, almost the whole being sent to Hamburg. The plant is a very common one by the roadsides in Caracas, but the greatest part of the drug (obtained from the seeds of the plant) comes from the hilly regions in the south, where it grows at an elevation of from 3,500 to 4,000 feet. It was originally discovered in the Mexican Andes, and is not known elsewhere. Although not mentioned by Humboldt, it is, however, apparently indigenous in Venezuela, growing in places where it is most unlikely to have been planted, and having been known long before the seeds were first exported by German druggists. The Caracasian plant differs slightly, but hardly specifically, from the typical form of Mexico. The bulbs contain numerous raphides of oxalate of lime.—"On the Pitcher-plant of California (*Darlingtonia*)," by W. Robinson, F.L.S. The Californian Pitcher-plants grow in the Sierra Nevada, at an altitude of 5,000 feet above the sea, in small sloping bogs along with *Sphagnum* and other true bog-plants. At a distance the pitchers have the appearance of jargonelle pears, holding their larger ends uppermost, at a distance of from 10 in. to 24 in. above the ground. This resulted from the pitchers being quite turned over at the top so as to form a full rounded dome, and the uppermost half of the pitcher being of a decided ripe pear-yellow. They are all twisted spirally, especially in their upper portion. Each pitcher had at the bottom a layer of from two to five inches of the remains of insects closely packed into it; from those of minute beetles to large feathery moths. What it is that attracts the insects is by no means clear. Pass a sharp knife through a lot of brown pitchers withering round an old plant, and the stumps resemble a number of tubes, densely packed with the remains of insects Within the pitcher the surface is smooth for a little way down; then isolated hairs appear; and soon the chamber becomes densely lined with needle-like hairs, all pointing down, so decidedly indeed, that they almost lie against the surface from which they spring. These hairs are very slender, transparent, and about a quarter of an inch long, but have a needle-like rigidity, and are perfectly colourless. The poor flies, moths, ladybirds, &c., seem to travel down these conveniently arranged stubbles, but none seem to turn back. The pitcher, which may be a couple of inches wider at the top, narrows very gradually, and at its base is about a line in diameter. Here, and for some little distance above this point, the vegetable needles, of course, all converge, and the unhappy fly goes on till he finds his head against the thick firm bottom of the cell, and his rear against myriads of bayonets; and here he dies. Very small creatures fill up the narrow base, and above them larger ones densely pack themselves to death in the hope of fighting their way out. When held with the top upwards; sometimes a reddish juice, with an exceedingly offensive odour, drops from them. The plant is closely allied to the *Sarracenia*, and would no doubt be easily grown in this country.—"On Carnivorous and Insectivorus Plants," by Mrs. Barber (Cape Town).

NORWICH

Norfolk and Norwich Naturalists' Society, Nov. 29.—The Rev. J. Bates delivered a lecture on "Sun-spots," illustrated by numerous diagrams. After explaining the various and changing appearances presented by sun-spots and faculæ, and the electrical disturbances in the earth's atmosphere which accompanies them, Mr. Bates briefly reviewed the theories which have been advanced from time to time to account for these remarkable phenomena, and concluded by explaining the wonderful light spectrum analysis has thrown, not only upon the composition of the sun itself, but even upon the atmosphere by which it is surrounded, and the stupendous commotions by which it is constantly agitated.—Mr. Barrett read a paper "On certain coast insects found near Brandon." In June last Mr. Barrett captured at Brandon several species of moths, whose habitat was essentially coast sand-hills, and which he believes were not suspected to exist at any considerable distance from the sea. The nearest sea-coast to this locality would be upwards of twenty miles distant, and delicate insects such as the tiny *Gelechia desertella* and the weakly constructed *Anerastia lotella* would not voluntarily undertake so long a flight, and it is impossible, considering their frailness and the nature of the intervening country, to admit of their having been "blown across" to their present locality. Mr. Barrett therefore submits that as this tract of country was undoubtedly a range of coast sand late in the post-glacial period, when the great valley of the Fens was still submerged. These little insects are probably the descendants of an ancient race which has survived the physical changes of ages, and that possibly their very weakness has preserved them in a locality now far removed from what is considered their natural habitat. The specimens exhibited were identical in appearance with duplicates obtained from the coast, and the only difference observed in their habits was their earlier appearance, about a fortnight before the coast specimens, doubtless due to the warmer and more sheltered locality. Mr. Barrett's paper was followed by a discussion, in which some curious facts bearing upon his theory were elicited, amongst others the nesting of a coast species of bird (*Charadrius hiaticula*), in the same tract of country, these birds having, as it were, two distinct places of existence, one portion nesting on the sea beach far away, the other frequenting the ancient coast line which may have formed their breeding place countless ages ago. Seals found living in the Caspian Sea, in which the waters are only one-fifth the saltness of the open sea, are identical with those found in the North Sea and probably in the Mediterranean. Another species found in Lake Baikal, which is fresh water, is also found on the American coast. These, it was contended, were descendants of the oceanic seals, left by the subsidence of the water after the glacial period.—The President read some notes on the birds of New Zealand, from a letter recently received by him from his brother at New Plymouth, Taranaki. Speaking of the disappearance of small birds, which has been attributed to injuries inflected upon them by the bees on which they are supposed to have fed, and which have increased enormously, his correspondent altogether discountenanced the idea, attributing their disappearance to the disturbances of war, bush fires, and perhaps climatal changes. He adds: "I suspect that terrestrial commotions have altered our climate; since our last great earthquake, our winds have altered in their intensity, frequency, and direction. Report states that there has been a great disruption of Antarctic ice, which to me explains the frequency of penguins, mostly young, being among the rocks here, and the capture of two sorts of seals, the common seal and seal lion, both young, quite close to the town, though they have never been seen here since this was a settlement. The natives say they were common enough before the Europeans came, and they still call certain rocks on the beach by the names of the sort of seal that once frequented them. Some of the birds, however, appear to be returning to their former haunts.

GLASGOW

Geological Society, December 1.—Mr. John Young, vice-president, in the chair. *Bituminous Striped Sandstone.*—The chairman exhibited a block of carboniferous sandstone from Gilmore-hill quarry, about nine inches in thickness, showing in that space thirty-two well-defined alternate white and dark-brown stripes, which gave the specimen a beautifully stratified appearance. Mr. Young stated that the brown stripes were due to the particles of sand having become mixed with bituminous matter previous to their deposition. *Carboniferous Fossils.*—Mr. Thomas Naismyth exhibited several drawers of fish remains, principally from the coal-fields around Glasgow, upon which Mr. Young offered a few remarks illustrative of their generic characters and their range in the carboniferous strata. The collection contained a number of fine large teeth of *Rhizodus Hibberti*, from the iron-stone pits at Possil; jaws, scales, and teeth of *Megalichthys Hibberti* and *Megalichthys rugosus*, besides a number of fin-spines and other fragments of fishes, from the Airdrie coal-field. Among the specimens were also to be noticed a few fragments of reptilian remains, consisting of portions of crania, vertebræ, &c., which had been found near Airdrie, and at Quarter, near Hamilton. *Oil Shale.*—Mr. D. C. Glen, C.E., laid before the meeting several slabs of oil shale from near Collingwood, on Lake Huron, Canada; and also some samples of the petroleum distilled from it. The slabs were from the Silurian formation, which is of great extent in North America, and remarkable for the regular succession of its strata. When examined, these blocks of shale were found to be stratified horizontally with layers of Trilobites, Entomostraca, and other marine organisms. It was from the prodigious abundance of these crustaceans over this track of ancient sea-bottom that the shales now referred to had received their bituminous ingredients.

PERTH

Perthshire Society of Natural Science, December 2.—Dr. Buchanan White, president, in the chair. Mr. W. Herd exhibited two specimens (♂ and ♀) of *Dasypolia templi*, recently found by him near Perth. Intimation was given that the first number of the *Scottish Naturalist*, a quarterly magazine of natural history, published under the auspices of the Society, and supported by most of the leading Scottish naturalists, would appear early in January.—Mr. J. Sadler, F.R.P.S.E. (of Edinburgh), read a paper "On the Geographical Distribution of Plants in Perthshire." He traced the range of various characteristic plants from the sea-level up to the summit of Ben Lawers, the highest mountain in the county, and pointed out the distribution of the rarer species in other parts of Britain. In reference to *Saxifraga cernua*, which in Britain has only been found on the summit of Ben Lawers, Mr. Sadler said that in the opinion of some botanists the Ben Lawers plant was only an Alpine form of *Saxifraga granulata*, but in his opinion it was a good species. The paper was illustrated by an interesting series of diagrams, formed of dried specimens of the plants. One of these diagrams showed the altitude attained by the various plants found on Ben Lawers from its base to its summit.—The President read a paper upon "A Naturalist's Work in Winter." He divided his subject into two divisions, "Out-door Work" and "In-door Work," and pointed out what could be and should be done in the various branches of natural history during the winter months.

PHILADELPHIA

Natural Sciences Society, July 5.—The president, Dr. Ruschenberger, in the chair. Mr. Meehan exhibited some specimens of *Rumex obtusifolius*, a naturalised dock from Europe. He said that so far as he could ascertain from European specimens, and the descriptions of Babington, Bromfield, and other English botanists, the plant was there hermaphrodite; but here, as correctly stated by Dr. Asa Gray, it was monoeciously polygamous. He thought the fact that plants hermaphrodite in one country becoming unisexual in another, was worthy of more attention by those engaged in the study of the laws of sex than had been given to it. This *Rumex* did not stand alone; *R. crispus* and *R. patienta* exhibited the same thing. *Fragaria* was another instance well known to horticulturists, although the fact scientifically had not received due weight. The average tendency of the strawberry in Europe was to hermaphrodism—here to produce pistillate forms. He also called attention to the fact that in these American specimens unisexuality was in proportion to axial vigour. This law he had already explained in times past to the Academy, and new instances were scarcely necessary. Here, however, the moderately weak plant had more hermaphrodite flowers than the strong one; and in both classes of specimens the number of male flowers gradually increased with the weakening of the axis, until the ends of the raceme were almost wholly of male flowers. The first flowers on the strong verticels were usually wholly pistillate. Prof. Leidy remarked that the interesting communication of Mr. Meehan had recalled to his mind a result of his experience, which he thought would accord with that of others, viz., that species viewed as common to both Europe and America frequently exhibit slight peculiarities, which are distinctive of those of the two countries. It is what might be inferred even if we admit the evolution of existing species from a common remote ancestry. A wide separation, with a considerable lapse of time and a modification of circumstances, are sufficient to account for the slight and acquired differences. Even where differences are not observed in form and structure, they may exist in the habit of the species. Thus the common wolf of Europe and America, viewed by many naturalists as of the same species, differs strikingly in character in the two countries. In the former it is a more fearless animal, not hesitating to attack man; in the latter, it is said never to attack man. At an early period observers saw, or thought they saw, many of the same species of plants and animals indigenous to America that occur in Europe, and hence the common names of European species were applied to those of America. Gradually the list of species common to the two countries was much reduced, and now is comparatively small.

August 2.—Mr. Vaux, Vice-president, in the chair. Mr. Thomas Meehan called attention to the arrangements of some plants for preventing fertilisation through any other than insect agency, as discovered by Darwin. The *Salvia* family of plants had the most elaborate arrangements for insect agency, but it had been objected to Darwin's theory that insects made no use of them. Bees bore holes through the tube from the outside for the honey, and do not enter by the mouth of the flower, as they ought. In the same way, in the *Petunia*, bees bore for honey from the outside. He had discovered that in these cases, where day insects failed to make use of these apparatuses, fertilisation was carried on by night moths, so that the objections to Darwinism were removed. He also referred to the common sweet chestnut, as bearing two classes of male flowers, only one of which probably aided in fertilisation. The first class appeared ten days before the other, and are those which give whiteness to the trees. They appear in the axils of the weak shoots. The female flowers appear on the apices of strong shoots, according to his theory of the laws of sex. The second class of male flowers appears at the ends of the vigorous shoots bearing the female flowers. Whatever affects the vigour of the tree interferes with the production of female but not of male flowers, and this was the reason why some seasons had short crops.

BOOKS RECEIVED

English.—Text-books of Science; Inorganic Chemistry: W. A. Miller, M.D. (Longmans and Co.).—Method and Medicine, an Essay: B. W. Foster (Churchill).—Science, Creeds, and Scripture, and the Mystery of God (Blackwood and Sons).—A Laboratory Text-book of Practical Chemistry: W. G. Valentin (Churchill).

Foreign.—(Through Williams and Norgate)—Theoretische Astronomie: Dr. W. Klinkerfues.—Handbuch der allgemeinen Himmelsbeschreibung: H. J. Klein.

PAMPHLETS RECEIVED

The Education and Status of Civil Engineers (published by the Institution).—Spectrum Analysis: a Lecture by W. Huggins.—Spectrum Analysis: a Lecture by Prof. Roscoe.—Coral and Coral-reefs: a Lecture by Prof. Huxley.—Proceedings of the Annual Meeting of the Natural History Society of Montreal.—Proceedings of the Cleveland Institution of Engineers.—Science Education abroad: a Lecture by Principal Dawson.—Description of New Fossil Shells of the Upper Amazon: T. A. Conrad.—On the Heat developed in the combination of Acids and Bases: Dr. Thomas Andrews.—New Remedies. Dr. M'Elroy.—Provisional Catalogue of Transactions of Societies, Periodicals, and Memoirs in the Radcliffe Library.—Essay on the Comparative Efficiency of Spectroscopic Prisms of different Angles: E. C. Pickering.—Abstracts relating to the Preservation of Food: W. H. Archer.—Experiments on the Transpiration of Watery Fluid by Leaves: W. R. McNab, M.D.—Applicazione della teoria Darwiniana ai fiori ed agli insetti visitatori dei fiori: F. Delpino.

DIARY

THURSDAY, December 22.

Royal, at 8.30.—Actinometrical Observations made at Dehra Doon and Mussoorie, in India: Lieut. Hennessey.—On the Constitution of the Solid Crust of the Earth: Archdeacon Pratt, F.R.S.—On the Extension of the Coalfields of England beneath the Newer Formations, and the Successive Physical Changes whereby they have been reduced to their present Dimensions: E. Hull, F.R.S.

FRIDAY, December 23.

Quekett Microscopical Society, at 8.

TUESDAY, December 27.

Royal Institution, at 3.—Burning and Unburning: Prof. Odling (juvenile lectures).

THURSDAY, December 29.

Royal Institution, at 3.—Burning and Unburning: Prof. Odling

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Erratum.—Page 94, first column, line 2 from bottom, for "*aquetocus*" read "*aquaticus*."

THURSDAY, DECEMBER 29, 1870

SCIENCE AT SCHOOL BOARDS

THE country may, we think, be congratulated on the election of School Boards in London and the provinces. Although from our point of view it may be deplored that so few men of Science, or persons having any pretension to understand what Science means, have been elected, it must be felt that the beginning of a great work has taken place in this country, the end of which no one can at present foretell. The nation, for the first time in its history, has taken the subject of education into its hands. The Education Act will be open to alteration and revision in the Houses of Parliament, and from step to step we may hope to see at last a department of Government representing the wishes of the people, dealing alike with the education given in our universities and our ragged schools. The great aim of the country must be to give to every child born in the kingdom the best education adapted to secure its happiness and usefulness in this world. There is no doubt that this will be the feeling that will prompt members of the various School Boards to carry out the powers which have been given under the Education Act.

We exceedingly regret that the various Boards have been elected rather upon a religious ground than upon the general principle of what is desirable to be taught in the schools. As we read the Act, there will be little opportunity left to the Board to increase or alter the conditions of "religious teaching" in any of the schools. It would have been better, perhaps, to have excluded all religious teaching from the primary schools, on the grounds, first that the feeling of respect and even awe which ought to attend the teaching of the Bible, is likely to be diminished by making it a common reading and task-book in schools; and secondly, that the clergy of the Established Church and of the various denominations, who are amply paid for their religious ministrations, ought especially to undertake religious teaching, and conduct it under circumstances that would render it most efficient and useful in the moral training of a child. There is also a third objection, and that is that the rate ought not to take the money of one set of people for the purpose of teaching the religion of another. There are certain moral obligations underlying all the higher religious creeds, to which no parent could possibly object, which ought to be taught and insisted on everywhere in universities as well as primary schools.

With regard to the other subjects to be taught in the schools, we would call attention to the danger there is that any extension of the means of education should lead to an imitation of the system now in existence. That system consists almost entirely of giving lessons out of books and teaching children words independent of the facts they represent. The present Chancellor of the Exchequer has well said, "I think it is more important for a man to know where his liver is seated and what its functions are, than to know it is called *jecur* in Latin and ἧπαρ in Greek." Of course there is no chance of Latin and Greek being introduced into our primary schools, although if they are such a precious means of developing the mind as they are assumed to be, there seems to be no reason why they should not. But the substitute for these branches of human acquirement is found in our lower schools in the shape of reading poetry, history, geography, and the like. If the sentiment of the Chancellor of the Exchequer exists in the new School Boards, the time seems to have come when some effort may be made to give up a certain amount of time in all schools to the teaching the facts of the external world. This is what is usually called scientific training, and has been almost universally regarded in our systems of education as something that may be dispensed with. But Science is after all but a systematic arrangement of observed facts by which the laborious investigations of the few may be made the possession of the many.

It may be urged in favour of this teaching that it educates (draws out) portions of the mind which cannot be cultivated by means of words and figures or moral lessons. A boy may be able to read all languages and master all problems in mathematics, and be a moral paragon, and yet commit some stupid blunder, from ignorance of some obvious chemical, physical, or vital law, that may cost him his life, or, what is more important still, may lead to the death of others. Our whole national history is full of terrible instances of punishment for breaking obvious and easily understood natural laws.

That Natural Science can be taught in schools there is no doubt. It has been introduced in a limited way into our great schools, as Harrow, Rugby, and Eton; and, so far as it has gone, it has not only not been attended with any diminution of acquirements of other branches of knowledge, but rather the contrary. In some of the national schools in Ireland, Science has been introduced, and we can bear testimony to the amount of useful information acquired by a class of boys in chemistry at the National School in Sligo.

The most difficult question for the School Boards to determine will be how to begin. In nine cases out of ten they have no men of Science to direct them. There is one comfort in London, that the Board will have a host in Professor Huxley, who, if they will listen to him, is undoubtedly capable of giving good advice. He will be ably backed by Miss Garrett, who, with her medical education, will be fully able to appreciate both the subjects and methods of any attempt to teach Science in our schools. Mr. Lucraft, the working man's candidate, has also advocated the teaching of Natural Science in schools. If the other candidates said anything on this subject, the reports of their speeches have not yet reached us. Still we may hope and we would especially recommend to the reading of all members of School Boards a "Report of a Committee appointed by the British Association on Scientific Education in Schools." It is a parliamentary paper, published in March 1868. We do not think this paper had the attention paid to it demanded by its intrinsic importance; and we are glad to recommend it, as especially adapted for the reading of members of School Boards and of all interested in education.

Without having any cut and dry system to offer to the public, we would advise that some attempt be made to teach some quantum of Natural Science somehow. The present masters will probably be utterly ignorant of any branch of Science, but there are plenty of students of Science who would undertake at first to instruct, perhaps in several schools. They should be instructed to teach children to observe facts, and lead them gradually from simple facts to the more obvious and easily understood laws of Science. Such classes are formed in Germany, in what are called *Real Schule*, and the system has been introduced into England under the name of Object lessons. Such teaching might be preparatory to taking up any one branch of Science, such as Chemistry, Experimental Physics, Botany, or the elements of Human Physiology.

We are glad to find that this subject has again been taken up by the British Association for the Advancement of Science. A few days ago a deputation of this Association waited on the Vice-President of the Council for the purpose of presenting a memorial on scientific teaching in elementary schools. Their reasons for urging this subject, they say, are three: "Firstly," the memorial says, "we conceive such teaching to be one of the best instruments of education in the sense of intellectual discipline, and in many respects better calculated to awaken intellectual activity than other studies; secondly, we think that a knowledge of the elements of Natural Science has a high value as information; and thirdly, we are of opinion that scientific training and teaching in the elementary schools will afford the best possible preparation for that technical education of the working classes, which has become indispensably necessary to the industrial progress of the country." The subjects they propose to be taught are elementary Physical Geography, elementary Physics and Chemistry, elementary Botany, and elementary Human Physiology. They think that by such an education the children of "the poor and necessitous" might be prepared to take advantage of the scholarships and exhibitions which are now only open to the children of the well-to-do classes of society.

E. LANKESTER

THE LEARNED SOCIETIES AND THE PRESENT CONDITION OF SCIENCE AND LEARNING

THE appointment of the Royal Commission on the present condition of Science will naturally turn the attention of many minds to the subject, and its discussion will certainly elicit many suggestions and schemes for the better culture of knowledge. The question is so large, so important, and so difficult, that the freest possible discussion will be necessary for its satisfactory solution.

At present we wish to direct attention to the question as to how we may obtain from the Learned Societies of the United Kingdom the greatest possible aid in the improvement of natural knowledge. The number of these societies is now large. Some of the provincial societies can claim an honourable place even when compared with the associations which are not confined to any one locality in their choice of members. Members of the Literary and Philosophical Society of Manchester, it should be remembered, were the first who were favoured with Dalton's Atomic Theory. Of what we may call the national societies, the number is increasing yearly, greatly to the detriment of real progress. Membership in these societies is coveted because it is supposed to indicate the possession of certain acquirements, it being thought, not unnaturally, that the members have won their spurs as investigators and interpreters of Science. Nor can we conceive of any better tests than those at present applied to candidates. Examinations are clearly impossible in this case, even if one were fully confident of the certainty of that method for detecting ability. It is evident that, on the whole, the regulations now enforced have been successful in their object, and that membership of a British Learned Society is generally not only a coveted distinction, but one deservedly prizeable.

Year by year these societies gather up the result of patient investigations, of long and careful research. Recording new facts, illustrating old truths, dissecting error, they pursue a course of steady consistent usefulness. Every one who has had to work up some special topic, must have a feeling of gratitude for the aid he has received from their publications. The societies are doing a good share of honest work, and doing it well. Their ranks include the most distinguished and the most ardent investigators in each branch of learning. Still we need not attempt to disguise the fact that they do not contribute so largely to the advancement of knowledge as it is desirable they should do. They have forgotten, or never known, that unity gives strength. They have neglected the great fact, daily becoming more apparent, of the unity of knowledge.

"The divisions which we establish between the Sciences are, though not arbitrary, essentially artificial. The subject of our researches is one: we divide it for our convenience, in order to deal the more easily with its difficulties. But it sometimes happens—and especially with the most important doctrines of each Science—that we need what we cannot obtain under the present isolation of the Sciences,—a combination of several special points of view; and for want of this, very important problems wait for their solution much longer than they otherwise need do. To go back into the past for an example: Descartes's grand conception with regard to analytical geometry is a discovery which has changed the whole aspect of mathematical science, and yielded the germ of all future progress; and it issued from the union of two Sciences which had always before been separately regarded and pursued." (Comte.)

Science suffers not only from the causes indicated in the preceding extract, but also from the dispersion of material in different receptacles, all of which are not accessible to the student. If the number of existing learned bodies be taken into consideration, and also their conflicting claims, it will be obvious that none except rich men can possess all the aid which they can give to the investigator. A paper upon the characteristics of one of our English dialects might appropriately be read before the Royal Society, the Society of Antiquaries, the Philological Society, the Archæological Institute, the Archæological Association, the Royal Society of Literature, the Ethnological Society, the Anthropological Society, and a score or more of the provincial societies. We find a valuable monograph on the Lancashire dialect in the Proceedings of the Philological Society, and another in the Transactions of the Literary and Philosophical of Liverpool, but for information on the eastern variety of that dialect, we must go to the Historic Society of Lancashire and Cheshire. The Cheshire glossary must be sought in the Archæologia, the Cumbrian in the Royal Society of Literature.

The same confusion may be predicated of almost every subject that can be taken up for inquiry. And, in spite of the multiplicity of societies, there is greatly needed throughout the length and breadth of the land a network of intelligent observers. We propose, as a remedy, that the present chaotic want of system be superseded by a National Institute for the Advancement of Knowledge. Such an institute might readily be obtained by the amalgamation of the present societies into one homogenous body. Whatever of interest and of value British savants might bring before it would be welcome and appropriate, and would be accessible to the student of the "knowledge which is one." In its organisation, the first labour would be the classification into sections. Whilst, on the one hand, there would not be three or four sections to perform the same work, on the other hand the entire domain of human knowledge could be fairly occupied, which is not the case at present, and the divisions marked with much greater accuracy than is now possible. The members residing in each district would form a local section, hold their meetings at regular intervals, and be a committee charged to watch over and promote the interests of Science and Learning in their particular neighbourhood.

In this brief and necessarily imperfect outline, much is omitted. Advantages not here indicated would result from the creation of a National Institute for the Advancement of Knowledge, but it is hoped that enough has been said to prove the desirability of such a foundation, having for object the attainment (in the words of Bacon) of "the knowledge of causes and secret motions of things; and the enlarging of the bounds of humane empire to the effecting of all things possible."

WILLIAM E. A. AXON

PROF. BALFOUR STEWART'S ELEMENTARY PHYSICS

Lessons on Elementary Physics. By Balfour Stewart, LL.D., F.R.S. (London: Macmillan and Co.)

THIS is a bold experiment, and decidedly deserves to be a successful one. Nearly all our elementary works, even on mere *departments* of Physics, are exremely bad, especially the so-called "original" ones; and those which have been translated from the French are little suited to the genius of this country—however excellent they may be in France—while they are usually spoiled by inaccurate translation, or by clumsy and injudicious addition of a mere cobbling or patching kind.

The reasons are not far to seek. It is very rarely that ve find in this country a genuine scientific man who can, like Faraday or Herschel, make himself easily intelligible ven on difficult subjects to an ordinary reader; still more rarely that we find such a man to have paid such pecial attention to the merest elements of his subjects as o thoroughly understand them himself, which ought to e regarded as an absolutely indispensable preliminary to is teaching them to others. Take for instance the quesion of the measurement of temperature in conjunction ith the second law of thermodynamics, that very second aw itself, or its connection with the equality of radiating and absorbing powers. Take even a simpler matter, the notion of a standard pound as a definite quantity of matter, not as something which shall be attracted by the earth with a certain force. Try all the elementary works in succession, and, if you are not driven mad by their inconsistencies and want of definiteness, endeavour to give in a clear, intelligible form the result of your studies on any such questions as those just mentioned. If you had no notion to begin with, you will have none, or worse than none, at the end; and, even if you began with thorough knowledge, you would probably end helplessly confused, doubting the simplest and most obvious truths. But this is the way we do things at home; and hard, indeed, must be our British heads, which, after they have managed our "*As in præsenti*," &c. &c., can plunge into this further chaos, and rise, as they often do, refreshed and invigorated by the struggle. A Frenchman, perhaps even a German, would perish in the attempt. But for them the path is made comparatively easy.

Nothing seems plainer than this—that he who has been ill-taught in the elements of his subject, however he may advance in knowledge (which is always a man's *own* work, whoever be his teacher), can hardly hope to understand these elements well enough to teach them to others. They have become to him a hateful thing, so he pushes on and avoids them as much as possible. Hence, that we may have really elementary works of a strictly scientific kind, we must have, not merely a genuine scientific man to write them, but one whose elementary instruction was good, or one whose strength has enabled him to get over its imperfections. These qualifications are certainly united in Dr. Stewart, for he had the late Principal Forbes for his teacher, and he is himself a man of quite exceptional powers, both in experiment and in reasoning.

It is scarcely possible to form a judgment as to the probable success of the present work. It is so utterly unlike anything to which we have been accustomed, that we can only say we never saw such a work, in English, at all events. Nothing so perfectly elementary, and yet throughout so intensely suggestive, have we ever met with. Even while reading the introductory chapters, we have several times laid down the book to follow trains of reasoning suggested by a single happy phrase that showed us something with which we had considered ourselves familiar, from a perfectly novel and interesting point of view. This, of course, will not strike the beginner, neither will it impede his progress; for it is not learned and abstruse disquisition or discussion, it is simply the clear vision of the writer.

Dr. Stewart does us much more than justice in the Preface, for he exaggerates the importance of a few suggestions of ours, made only with the view of keeping him to *his own plan*, which we consider to be an admirable one. The grand modern ideas of Potential and Kinetic Energy cannot be too soon presented to the student; he ought to be familiarised with them as soon as he commences the study of Physics. In fact, we believe that before many centuries have passed, perhaps before fifty years have elapsed, the word Force will have become as much a nuisance and an impediment to the beginner in Physics as the phrase Centrifugal Force is already.

However this may be, the work before us is an excellent one, and will certainly (*if there be teachers found sufficiently* *instructed to recognise its merits, and sufficiently humble and enterprising to stoop to learn anew, and by a better method, the elements of their science*), take its place at the head of elementary treatises on its subject. It is by no means faultless: no first edition on so new a plan could quite avoid confusion; there is excess of detail on many points, too little on others, and the language, though generally correct throughout, is sometimes almost mystical. This is not a reproach—quite the reverse—for it is mainly in these passages that we feel the strength of the author, and we are unfortunately not speaking from the beginner's point of view. He has evidently thought deeply, and the result is in all cases well worthy of careful study, especially for those who think themselves thoroughly masters, if but of the merest elements. No one can read the work without feeling that he has still something to learn, even in the most prosaic parts of the science. Dr. Stewart does not, as it were, follow the ordinary laws of war; he abjures pipe-clay and red tape, and he has a method of his own which we cannot but think is calculated to do a real service to the beginner. Even methods in mathematics cannot be stereotyped; Euclid is about to be laid on the shelf; and it is not at all unlikely that in a few years the so-called Cartesian x, y, z, will disappear, to make way for Hamilton and his vectors. Thus it is, and shall be, with the so-called *statical* proofs of the Parallelogram of Forces, we shall get back to Newton's methods as nearly as modern nomenclature will permit; and so likewise in other parts of physics. The reign of *inartificiality* and *simplicity* must soon be inaugurated, and this work will greatly tend to hasten its advent.

It would be improper to finish without finding some additional fault, especially after all we have said in praise of the work, and even Dr. Stewart's recent accident (from the effects of which we are delighted to hear he is steadily recovering) must not influence us.

The printing is excellent; but some of the woodcuts (the balance, p. 59, and the strained beam, p. 71, for instance) are not merely execrable, but, what is far worse, misleading. No mention is made of the Peltier effect at a thermoelectric junction, nor is Sir W. Thomson's so-called "specific heat of electricity" alluded to, though both might easily have been introduced without increasing by more than a page or so the bulk of the volume. These are matters of such fundamental importance, and are capable of such easy description, that they certainly ought to have been given. There are other points of a similar kind, but it is not necessary to mention them.

Dr. Stewart very fully treats of the grand question of the equality of Radiation and Absorption, the question which first brought him prominently before the scientific world; but he has done it with such an excess of modesty that his own genuine claims might be endangered, were there not happily other works in which his services to this important branch of science are fully recognised.

It is peculiarly sad that Prof. Stewart should have been temporarily disabled just when he was getting into working order his Physical Laboratory in Manchester: no one is better fitted for such work than he is; let us hope that he may soon be in a position to resume the direction of it, and to teach beginners by means of his excellent Manual.

P. G. TAIT

OUR BOOK SHELF

The Academy. Vol. I. (London: Williams and Norgate. 1870.)

WE congratulate our twin brother (or sister?) the *Academy*, on the appearance of its first volume. The journal had at its starting a clear *raison d'être*, to respond "to a widely felt and constantly expressed dissatisfaction with the existing organs of literary and scientific criticism." The wide field embraced in the programme has rendered the editor's task anything but an easy one. Of the literary department it does not come within our province to speak; the scientific portion, we can fairly say, has been honestly and ably executed. This department consists of two sections—original reviews, and scientific notes. The former, in accordance with the practice of the rest of the paper, are all signed. The desirability of signed articles is one that has been much debated. Whatever may be its relative advantages or disadvantages in literature or politics, we are convinced that in science the former greatly outweigh the latter. In reading a criticism on a scientific work, it is before all things necessary that we should know that the critic has a right, from his own knowledge of the subject, to speak with authority. The signatures to the scientific articles which will be found in this volume are themselves sufficient guarantee that the subject is discussed from a standpoint from which something is to be gained by the reader. The scientific notes consist of paragraphs under the various heads of chemistry, physics, geology, zoology, botany, physiology, &c., epitomising the most important discoveries or researches of the month. Though the subjects are rather unequally treated, the notes have evidently been drawn up with great care by competent men, and the whole gives a very fair *résumé* of the more important advances in each department of science. If we might mention one section that appears to us to have been particularly well done, it is that of physiology. A list of the new books of the month, English and foreign, is also given, and the titles of the more important scientific magazine articles, with occasional abstracts of them. We notice with pleasure the conscientious manner in which the editor invariably acknowledges the source of his information, a practice we could wish to see more generally carried out by his brothers of the craft. Other literary journals have been content hitherto to supply their readers with their modicum of science either second-hand and very much out of date, or with a disregard to accuracy which has rendered it perfectly valueless. The *Academy* is doing good service in bringing scientific subjects before educated readers who have no special scientific bias, in a style that is likely to interes them in it, and in a manner that may be relied on as soun and accurate, and calculated to increase the knowledge i which they are, as a rule, so lamentably deficient.

Die Praxis der Naturgeschichte. Zweiter Theil: Dermoplastik und Museologie, oder das Modelliren der Thier und das Aufstellen und Erhalten von Naturalien sammlungen. Unter Mitwirkung von Praparato Bauer, Prof. Dr. G. Jager, Stadtdirektions Arzt D Steudel, und der Thier- und Landschafts-Maler, Pau Meyerheim und Friedrich Specht; von Philipp Leopol Martin. 8vo, pp. 240, six plates. (Weimar: B. I Voigt. London: Williams and Norgate 1870.)

FEW tasks are more distressing to a right-minde naturalist than the inspection of the ordinary mounte specimens of animals in most museums in this countr and elsewhere. More hideous spectacles than usuall meet one's eyes when visiting these establishments is impossible for man to form, or mind to imagine. Som little advance, it is true, has been made of late year upon what was formerly the prevailing type of a "stuffe beast." But no real reform can take place until th curators of museums have come to recognise the gre truth, that, unless such objects are properly mounted, it is worse than useless to exhibit them to the public at all. They should be taken down and stowed away in drawers, or preserved in any other way that may be convenient for scientific study. Left in their glass cases, they are much more likely to repel than to attract the ordinary observer, for whose benefit the exhibition is intended.

Under such circumstances we cannot receive otherwise than with pleasure a treatise prepared with the view of teaching the true principles of the art of taxidermy and their proper application. The Royal Cabinet of Natural History at Stuttgart is well known to those who have visited it as one of the few institutions of this kind where real care and skill are exhibited in the mounting of the specimens, and no one can be more fitted than its energetic "*preparateur*" to give instructions upon a subject of which he has shown such perfect knowledge. Herr Martin has, moreover, obtained the assistance of several individuals who are fully qualified to assist him in his task, which appears to have somewhat of a comprehensive scope. In a former part of the present work, Herr Martin has treated of the various methods of collecting animals of all sorts in the field, and of preserving them for scientific purposes. The fact of a third edition of this former part having been already called for shows that the work has been appreciated by those or whose instruction it is designed. In the present section of his volume, Mr. Martin and his fellow-workers treat more especially of the processes to be performed in the museum itself, such as the modelling in plaster of beasts large and small, the formation of preparations of the internal organs, the making of skeletons, and the mounting of microscopical objects. Full instructions are likewise given upon every point connected with the practical working of a public museum, not only as regard the objects themselves, and the best mode of exhibiting them, but also in relation to the wants and requirements of the visitors that resort to such institutions.

P. L. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Mimicry versus Hybridity

BEFORE attempting to combat the old theory under which Mr. Murray has taken refuge, in opposition to the theory propounded by Mr. Bates, I must first make a few remarks upon the different forms of mimicry to which the Lepidoptera are subject.

Mimicry may be divided into two heads, viz.:—the mimicry of one lepidopterous insect by another, and the mimicry of the vegetable kingdom, and of backgrounds generally, by Lepidoptera. As Mr. Murray doubtless refers to only the first of these heads when he speaks of hybridisation, I need not trouble the reader with any remarks respecting the second. Mimicry, then, between butterflies and moths, may again be divided into three sections: that which modifies both sexes, that which chiefly modifies the females, and, lastly, that which chiefly modifies the males;* these variations of modification are all easily explained by the theory of protective assimilation variously adapted to the economy of the different modified species; but it can in no way be explained by the theory of modification by hybridity. Mr. Murray speaks of hybridisation as if it were a thing recognised by lepidopterists, and of no uncommon occurrence, whereas it has, so far as I know, only occurred in the Heterocerous Lepidoptera, and only between species of the same genus; there is, indeed, a case on record of a skipper butterfly and a burnet moth being taken *in coitu*, but no reasonable being could expect that any issue would result from such an union; again, I maintain that if it were even possible for hybridity to occur between different sub-orders, families, or even genera of Lepidoptera (which, by the way, is as likely as hybridity between a vulture and a dove or a horse and a rabbit), the offspring would inevitably be modified in structure just as much as hybrids between distinct races or species of vertebrates are; they would moreover, if fertile, certainly revert to one or other of the parent stocks, which, however we do not find to be the case; if the fertilisation of flowers and butterflies were the same, hybrids might be as common in the one case as the other, and the results attained might be more nearly alike; but as butterflies are not fertilised through the transmission of pollen by external agencies, and as they seem to have a decided preference for mates belonging to their own peculiar species, hybridisation must needs be a thing almost unknown amongst them. Lastly, I need scarcely say that the fact of birds hunting by sight and not by smell only does not in any way destroy the argument respecting the favoured and non-favoured species of Lepidoptera; the same thing may be said of lizards, frogs, dragon-flies, and spiders, which all of them persecute the order, and which all avoid not those insects only which have a peculiar odour, but those which, like the *Danainæ*, *Heliconinæ*, *Acærinæ*, and others, have an acrid taste resulting from an offensive liquid which they exude from the body. I have been more fortunate than Mr. Scudder, inasmuch as I have frequently seen birds catch and devour the unprotected species upon the wing, whilst I have received abundant evidence both from scientific and non-scientific collectors respecting the perfect immunity which the *Danainæ*, &c., enjoy from all kinds of persecution, whilst their less fortunate brethren come to an untimely end.*

British Museum ARTHUR G. BUTLER

I AM rather surprised that Mr. Andrew Murray should have advanced his theory of mimicry being due to hybridisation, without adducing one solitary fact to prove that hybridisation between distinct *families* of insects ever occurs, or that, if it do occur, the offspring are fertile *inter se*. Mimicry is most frequent between very distinct *families* or higher groups, and often between different *orders* of insects. We may fairly consider that the "natural orders" of plants, as being the next well-marked groups above genera, are about equivalent to the *families* of insects, so that the analogy furnished by hybridisation among plants, on which alone Mr. Murray's theory is founded, wholly breaks down, unless he can show (which he has not done) that such hybridisation occurs between species of different "natural orders," or of well-marked groups higher than genera. It would be mere waste of time to discuss the details of a theory whose fundamental assumption is not only quite unsupported by fact, but is diametrically opposed to the almost, if not quite, universal fact that hybrids do not occur between species of different families or higher groups.

Mr. Scudder's letter contains some interesting and suggestive facts, and opens up a new field of investigation as to the immunity of certain species, in their egg or larva state, from the attacks of hymenopterous and dipterous parasites. It is, I believe, now stated for the first time, that the peculiar secretions which render the Danaidæ distasteful to birds not only extend to their larva and egg state, but act as a safeguard from the attacks of parasites. The objection that it would have been more advantageous for the larva than for the imago of the *Limenitis misippus* to mimic the *Danais archippus*, appears to me to have no weight. We do not know, for instance, if such mimicry would be any defence against parasites who may be guided by smell rather than sight; and from the frequent limitation of certain odours and secretions to whole genera or families, the variations necessary to produce them may be of rare occurrence.

The fact that *Limenitis misippus* and *L. ursula* are about equally plentiful is not at all remarkable, since there are species of all degrees of rarity in every extensive group; but in this case it happens that both insects are mimickers, *Limenitis ursula* resembling the common N. American *Papilio philenor*, especially on the under side, which is exposed when the insects are at rest. This case of mimicry is not so perfect or so striking as the other, but that it is one is pretty certain, and there are several other instances in various parts of the world in which *Papilios* of certain groups are the objects of mimicry. Although Mr. Scudder has never seen a bird capture a butterfly, others have been more fortunate, and that they are thus captured very largely in the tropics is certain. It is not improbable, from the rarity of mimicry in the temperate zone, that the few cases which exist may have been produced under the more favourable climatal and organic conditions of the semi-tropical epochs anterior to the glacial period.

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* An interesting illustration of this type of mimicry exists between the genera *Belenois* and *Mylothris*, the males of the African group of *Mylothris* being identical in colour with males of the genus *Belenois* (sub-family Peirinæ).

* The Hon. Mr Justice Newton, who assiduously collected and took notes upon the Lepidoptera of Bombay, informed me that the *Charaxes Psaphon* of Westwood was continually persecuted by the bulbul, so that he rarely captured a specimen of this species which had not a piece snipped out of the hind wings; he offered one to a bulbul which he had in a cage, and it was greedily devoured, whilst it was only by repeated persecution that he succeeded in inducing the bird to touch a *Danais*, which he offered to it

ALFRED R. WALLACE

The Difficulties of Natural Selection

THE papers read by me before the Entomological Society "On the Relation between the Colour and the Edibility of Lepidoptera and their Larvæ" having been noticed and commented upon by Mr. A. W. Bennett and others in NATURE, I have deemed it desirable to offer a few remarks on the subject.

The object I had in making the experiments was to ascertain whether there could be proved to exist any relation between the colours of larvæ and their edibility.

The disciples of Mr. Darwin argued that the brilliant colours of so many male birds arose from sexual selection, and that the equally striking colours of flowers were but guides to insects, to enable them to distinguish, at some distance, the flowers from the leaves, and thus insure fertilisation by the interchange of pollen. Such reasons, however, were quite valueless to account for the bright colours of the asexual larvæ of many Lepidoptera, several species of which are banded and striped with blue, yellow, and red; colours which instead of concealing them by harmonising with the leaves on which they feed, are often in complete contrast with them.

Now Mr. Wallace had a theory that these gaily coloured larvæ were uneatable by birds, and that their gay colours were protective, because if they were indistinguishable from eatable species, they would be seized by birds, and though rejected afterwards, would be so much injured that the probability of their becoming imagines would be very remote, even if they were not at once killed.

This I found to be the case; in my experiments extending over many years, and most carefully made with several species of birds, I have not met with one instance in which a strikingly-coloured larva was eaten. In most cases they were not even regarded when thrown into the aviary, although I had several birds always on the watch for the eatable species, with which I constantly fed them; while these latter were seized immediately they were seen.

The larva of the *Cucullia verbasci* is conspicuously coloured blue and yellow, and feeds without any attempt at concealment on several species of Verbascum. I placed the plants in the aviary, and fed the Cuculliæ upon them until every leaf was devoured, and the caterpillars gnawed holes in the stem; but not one was in the slightest degree injured, yet at the same time other larvæ were greedily eaten.

On the other hand, I found that all larvæ were eagerly eaten which have soft smooth bodies and dull colours, while the hairy larvæ are rejected entirely.

These eatable species are protected in various ways; some are nocturnal in their habits, descending to the ground during the day; some feeding on the under sides of the leaves; others arrange their bodies in a line with the shoots of the plants and look like a streak of the bark; some are of precisely the colour of the leaves, or even of the corolla of the plant on which they feed; others roll themselves up in leaves, the larvæ of the *Geometridæ* are often exactly like twigs, with the terminal and side buds imitated.

This latter resemblance is so complete that, after being thirty years an entomologist, I was deceived myself, and took out my pruning scissors to cut from a plum-tree a spur which I thought I had overlooked. This turned out to be a larva of a Geometer two inches long. I showed it to several members of my family, and defined a space of four inches in which it was to be seen; but none of them could see that it was a caterpillar. Surely this was a case of protective mimicry.

All the eatable larvæ agree in not moving when there appears the least danger, and very rarely moving at all during the day.

Even if there were no cases of protective mimicry in the larval states of Lepidoptera, I do not think that would be any argument against the existence of such in the perfect state. It appears to me rather that as so few specimens become imagines in proportion to the eggs produced, the more need is there that these few should survive.

I cannot, therefore, agree with Dr. Scudder in thinking that mimicry has been supposed to exist where it is least wanted, viz., in the perfect state of Lepidoptera. Nor can I coincide with Mr. Bennett that it is a matter of indifference to the supporters of the theory of Natural Selection whether twig-like caterpillars are eaten by birds or not. My point is that they are often so like twigs that they are passed over as such by insectivorous birds, and that the closer the resemblance the better their chance of escape.

I believe myself that Mr. Darwin's theory will survive, and even be benefited by, the criticisms of its opponents; but what I do dread is the injury it may receive from the false arguments of some of its illogical supporters.

Lest I may unwittingly place myself in the latter category, I will bring my remarks to a close.

J. JENNER WEIR

6, Haddo Villas, Blackheath, S.E.

Butterflies and Birds

A CORRESPONDENT in NATURE, Dec. 22, states that after fifteen years' experience in butterfly hunting, he has never seen one in a bird's bill. I was not aware the circumstance was unusual, for I have frequently seen the common sparrow chase and capture such butterflies as *V. urticæ* and *P. rapæ*. It is quite a hare and greyhound affair, the butterfly often eluding for some time the swift pounces of its pursuer, so that the hunt is a long one.

T. G. B.

St. John's College, Cambridge

Ceratodus Forsteri

SIR PHILIP GREY EGERTON presents his compliments to the Editor, and would esteem it a favour if he would insert the following paragraphs, from two letters recently received from Professor Agassiz, in an early number of NATURE. It will be gratifying to all men of science to know that the distinguished Professor has so far recovered from his late severe illness as to be able again to interest himself in scientific pursuits.

Oulton Park, Tarporley

"Cambridge, November 9

"I am slowly recovering, and find myself gradually returning to the ways of active life. As I wake anew to feel an interest in scientific pursuits, there is nothing for which I have a greater longing than the fossil fishes. If I could leave my house I would fly to you to resume the examination of your and Lord Enniskillen's collections. The recent discovery of Krefft has added fuel to the fire, and I feel the most intense desire to revise the facts bearing upon the relations of the Ganoids and Selachians in general, and more particularly those of the Cœlocanthi, to which, from the examination of the skeleton sent me by Krefft, I find his *Ceratodus Forsteri* belongs. It will no doubt turn out that the Dipterini are close relations. In this connection I am reminded of what you once wrote to me of the teeth of Ctenodus. Will you now have the kindness to give me all the particulars? I am having sections of the teeth of *Ceratodus Forsteri* and some of the fossil species made for comparison. I have little doubt already that this genus will turn out to be one of the most curious *synthetic* types (I call them) in the animal kingdom, exhibiting characters of Placoids (Selachians) in the teeth, Ganoids in the scales, their embryonic characters in the preservation of a dorsal chord, instead of distinct bony vertebræ, and finally hollow bones as in birds."

"Cambridge, Dec. 8

"I take it some of your naturalists will crow over what they will be pleased to call my stupendous mistake in referring the teeth of Ceratodus to the Selachians, when the fish proves to have large imbricated scales; and yet I never was more pleased than when I learned the fact, for it settles beyond dispute the existence in nature of types, to which I have long ago called attention, under the name of *synthetic types* (see my Essay on Classification), but of which naturalists have thus far taken little or no notice. When I described the teeth of Ceratodus as those of a distinct genus among the Cestracionts, I was led to do so by appearances which secured for this association the assent of all naturalists. As long as the fossil teeth only were known, nobody questioned the relationship. Owen himself, in his 'Odontography,' mentions the teeth of Ceratodus and their structure, and has not a shadow of a doubt that I am right in placing that genus near Cestracion; and now comes the discovery that Ctenodus, a genus also referred to the Cestracionts, is based upon the dental plate of a bony fish, closely allied to the one recently discovered by Krefft, and referred by him to the genus Ceratodus. Is not all this the most palpable evidence that there exist in nature types which combine structural features that are entirely separate in other types? and it is to such types I have applied the name of synthetic types."

Lumiere Cendree

It may perhaps be of some interest to you to know that the phenomena of "Lumière cendrée" was distinctly seen in Surrey on the evening of the 25th inst., between 4 and 5 P.M. With the aid of an opera-glass, I saw clearly the whole of the dark portion of the moon's disc; and some friends who were with me at the time were able to see it with the naked eye.

H. G. S. Smith

Trinity College, Cambridge, Dec. 27

Measurement of Mass

With reference to the very favourable notice in your last number of my edition of Deschanel's "Traité de Physique," will you allow me to remark that my reason for rewriting the section on *mass* (§ 42) was that Deschanel, in accordance with what has been till recent years an almost universal custom, employs a variable unit of force, and, as depending upon this, a variable unit of mass, so that the number denoting the mass of one and the same body is diminished as the body is carried from the equator to the poles, and would increase up to infinity if the body fell to the centre of the earth.

The reviewer says, "the conception of *mass* is always a difficult one for a beginner." This is doubtless true when the conception is hampered with the inconsistencies arising from this vicious system of measurement; but I do not think the conception of a *pound* or *gramme* of matter presents much difficulty, and these are the units in which, according to the best modern usage, I have indicated that mass is to be expressed.

As regards the coefficient of absorption of ammonia, the reviewer is right. A mistake was committed in extracting the number from a table, of which, if I may judge by his initials, the reviewer is the author. In future, I would entreat him to make his tables more easy of reference.

J. D. Everett

Belfast, December 26

Hailstones

In Nature of the 15th there is an account of hailstones of a form deviating considerably from the spherical. Hailstones are frozen raindrops, and a rain-drop falling through a vacuum would of necessity be spherical, but in falling through the air it must tend to assume the form of least resistance, whatever that may be. I was told many years ago of hailstones which had been picked up and found to be of the form of Minié bullets. I do not vouch for the truth of this, but I think it likely; the Minié bullet was, I believe, the nearest approach to the form of least resistance that the inventor was able to arrive at.

Joseph John Murphy

Old Forge, Dunmurry, Co. Antrim, Dec. 20

Darlingtonia Californica

Mr. Robinson's suggestion, reported at page 159, as to the cultivation of this plant in England, has been anticipated by Messrs. Veitch and Sons, who have grown the plant for a considerable time in their houses at Chelsea.

In London, as in California, this curious plant possesses the same irresistible attraction to insects, and as I have repeatedly examined living plants at Chelsea, perhaps the following notes taken in connection with those printed in your last number may have some interest.

This so-called "pitcher plant," when fully grown, resembles in shape the upraised head and body of an excited cobra, with hood expanded and prepared for a spring; the head is at right angles with the hollow vertical body, and apparently presents no opening by which an insect could enter; under the place where the lower jaw would be, hang two large reddish appendages like the "wattles" of a fowl. At Chelsea this plant possesses such an extraordinary attraction for flies (principally blue-bottles), that the hollow "pitchers" are generally full of their dead bodies; what this attraction is I am unable to say, as the plant is scentless.

Last year I had a Darlingtonia before me for some three or four hours, whilst sketching it, and I then observed that the blow-flies made straight for it immediately they entered the room. Insects alight on the red "wattles" and then fly upwards into the (previously unseen) red-lipped entrance to the tube; owing to the sudden twist in the neck of the pitcher, they are at once compelled to descend the hollow body, and, as far as I have observed, they never return alive. They keep up a buzzing noise for half an hour or so, and then apparently die.

The old "pitchers" are generally full of dead flies, &c., and the lowermost insects, in rotting, cause the "pitchers" to decay and split, the flies within being then displayed. These dead flies often drop out through the fissures and become grouped round the bottom of the plant.

Worthington G. Smith

Aurora Arcs in the East

I am inclined to agree with Dr. Burder as to the invisibility of Auroras by daylight, yet I can confidently assure him that I have many times seen the arch "almost due east," that is when the extremities point N.N.W. and S.S.E. When such a phenomenon occurred in Newfoundland, some of the old weather-wise settlers would tell me to expect falling weather (snow or rain) on the following day, as the Northern Lights were in the south. But I am sorry to say that I did not note how often the Aurora appeared as above, but I *did* note that snow fell on *seventy-eight* consecutive days in the autumn of 1867 and commencement of the winter of 1868.

Henry Reeks

The Milky Way

In the number of Nature for November 17, Mr. John Jeremiah states that "Heol y Gwynt" is the only proper Welsh name for the Milky Way. Such is far from being the case. I am acquainted with no less than *nine* other names, equally proper for that luminous appearance, such as *y llwybr llaethog*, *y ffordd laeth*, *llwybr y gwynt*, *galaeth*, *eirianrod*, *crygeidwen*, *caer Gwydion*, *llwybr Olwen*, and *llwybr y mab afradlawn*. Of these names, *y llwybr llaethog* and *y ffordd laeth* answer precisely to Milky Way; *llwybr y gwynt* (common enough in Carmarthenshire) is synonymous with *heol y gwynt*; *galaeth* (from *llaeth*, milk) corresponds with galaxy; *eirianrod* signifies a bright circle; and *crygeidwen* a white cluster. To *caer Gwydion* (the mural enclosure of Gwydion) belongs a tale, which may be compared with the stories of classical antiquity on the same subject. Gwydion is a noted character in early Welsh romance, in which he figures as an astronomer and an enchanter. He was the son of Don, king of Llychlyn or Scandinavia, and is said to have lived in the fourth century. According to the Welsh poets, he travelled through the heavens in search of a lady, who had eloped with Gronwy Befr, and left a track behind him, which has ever since been called *Caer Gwydion*. When he found the faithless lady he changed her into an owl. His scientific acquirements are often mentioned in Welsh mythology, and frequent allusions are made to him as an enchanter by the early bards. He is said to have been instructed in magical arts by Math ab Mathonwy, and in the Mabinogi, or tale which bears the name of the latter, his achievements are detailed at length. According to some of the Welsh records he was buried at Morfa Dinlle, on the seashore near Carnarvon.

Gwydion is not the only one of the family of Don whose name is associated with astronomy. Don himself gave his name to the constellation of Cassiopeia, which is called in Welsh *Llys Don*, the Court of Don; and *Caer Arianrod*, the Corona Borealis or Northern Crown, is so called after his daughter Arianrod.

Llwybr Olwen (the path or course of Olwen) refers to another distinguished character in Welsh mythology. Olwen was the daughter of Ysbyddaden Bencawr, a prince of the Northern Britons, who lived in the sixth century. Her extreme beauty was proverbial, and her charms are frequently alluded to by the ancient bards. It is stated of her that four white trefoils sprang up wherever she trod, and from this circumstance she was called *Olwen*, or white track. She was sought in marriage by Cilhwch, prince of Clyddon, and his adventures in order to obtain her form the subject of the Mabinogi of Cilhwch and Olwen, which will be found printed, with an English translation, in Lady Charlotte Guest's "Mabinogion," ii. 197,249

What connection the other name, *llwybr y mab afradlawn* (the path or course of the prodigal son), may have with the

Milky Way, is not obvious, as I am not aware of the existence of any fable bearing on the point.

D. Silvan Evans

In reply to Mr. Reeks, I was quite correct in stating that the wind would blow in the course of the Milky Way; and to be sure of it, I have communicated with my friend at Llangadock, who has repeated what I have previously stated. He also tells me that on Sunday night (the 11th inst.) he looked out and found the "wind blowing from the east, and the Milky Way was to be seen coming from the north-east." He still thinks it possible to predict the weather by this kind of observation. However, apart from this, the Welsh word is sufficient to prove the correctness of my former letter, which means the "Road of the Wind."

Dec. 13 John Jeremiah

Meteoric Shower

I observed a most beautiful star-shower on the night of the 5th inst., at about a quarter to nine o'clock. It crossed the "tail" of Ursa Major in a direction almost easterly, and slanting towards the earth at about an angle of 30°. At first the phenomenon resembled the flight of a flock of wild geese, but after a little the nearer stars inclined towards the earth more than those farthest away, so that in all I could see about thirty stars. I write to you as the period of recorded star-showers mentioned in Prof. Ansted's Physical Geography, is from Dec. 6th to 13th, and I observed this star-shower on the 5th December.

The Commons, Killybegs, Dec. 14 John C. Ward

Hereditary Deformities

The articles upon this subject in Nature, Sept. 8, Oct. 20, and Nov. 3, remind me of what I learned fifteen years ago while visiting tribes of Sioux Indians, assembled to the number of 5,000, near the mouth of the Yellow Medicine River, in Minnesota. The Indians were collected at this point for the purpose of receiving their annuities from the U.S. Government, and were accompanied by their families. It is customary for the squaws of their tribes to have tattooed upon the prominences of their cheek-bones small discs, of from one-eighth to one-fourth of an inch in diameter. I was informed by a physician, who has passed much of his time with these tribes, that sometimes a child was born with these marks. This was confirmed by the U S. Government Indian Agent. I had no means of verifying these statements; they were believed by my informants, who were gentlemen of veracity.

Chas. M. Wetherill

Lehigh University, Nov. 19

Right-handedness

Canon Kingsley is a close observer of nature, and if his generalisation be correct in the following instance, it would seem that the tendency to develop the right arm to the comparative neglect of the left is not confined to man. In describing the call-crabs of Montserrat, he says that one of the claw-arms, generally the left, is dwindled to a mere nothing, and is not seen, while the other is disproportionately large. I am well aware that the claws of lobsters are seldom equal in size, but have had no opportunity of ascertaining whether it is the right or the left claw which is superior, nor whether there is any rule in the matter.

C. J. R.

Sun Stroke

In the *Revue des deux Mondes* for the 15th August (page 854), there is a remark which, though somewhat exaggerated, is of very great value and practical significance. The writer says, "The phenomenon known as 'Sunstroke' is due to the action of light, and not, as is generally believed, to the elevation of temperature." An exception has to be made in cases where the sun playing, especially on the back of the head and neck, produces unmistakeable sunstroke. Every surgeon practising in the East also meets instances of "solar apoplexy," which present themselves as often as not during the night, but only in the excessively hot weather. However, I know from personal experience that it is quite possible to lay oneself up completely with intense headache, constant nausea, cold extremities, &c., by exposing the eyes only to the glare of the sun, the head and neck being completely sheltered by a helmet and *puggree*, and the body being at rest in a carriage. Further, I have found it possible, when accidentally obliged to expose myself, to avoid all inconvenience by merely wearing deeply-smoked glasses, my head being guarded only by an ordinary felt hat. But this is an experiment not to be tried rashly. The conclusion obviously is that whenever there is an intense glare, whether attended by intense heat or not, the first condition to fulfil is to shelter the eyes. As the retina is in truth an expansion of the brain, the brain is more accessible to external influences through the eyes than through any other avenue.

R. A. Jamieson

Shanghai, Oct. 24

GLYCERINE EXTRACTS OF PEPSIN AND OTHER FERMENTS

A short time ago Von Wittich published in *Pflüger's Archiv* some interesting results of an attempt to isolate, by means of concentrated glycerine, pepsin and other so-called ferments found in animal and vegetable bodies.

The mucous membrane of a pig's stomach, washed and freed as much as possible from water, was finely minced and bruised, and then covered with pure glycerine. After standing twenty-four hours, a few drops of the glycerine, diluted with acidulated water, digested fibrin with remarkable rapidity. After pouring off the whole of the glycerine, a second, third, and even fourth glycerine extract could be made, all manifesting strong peptic powers. On treating, after filtration, these glycerine extracts with a large excess of alcohol, a slight precipitate was obtained, which, separated by filtration and re-dissolved in acidulated water, though giving only the faintest proteid reaction, was strongly peptic.

In a similar manner salivary gland and pancreas gave up to glycerine an amylolytic or starch-converting ferment, almost entirely free from proteids, and a "laden" pancreas also gave up a ferment capable of digesting fibrin in an alkaline medium. Barley (*not germinated*) gave up to glycerine a non-proteid diastase; and almonds a ferment capable of acting on amygdalin.

I have repeated many of Von Wittich's experiments with almost entirely similar results. We certainly have in glycerine a new means of working out the intricate problems of these so-called ferments. The glycerine extracts, for the most part at least, seem to remain unchanged for a very long period, so that a stock of ferment can always be kept in store. On the other hand, tissues may, by repeated extraction with glycerine, be exhausted of their ferment, and yet little, if any, otherwise changed, so that they can be examined under conditions hitherto impossible.

Not the least value of the new method lies on the practical side. The means hitherto adopted of preparing the so-called pepsin for medical purposes are confessedly clumsy and inefficient. By glycerine we can now extract, without any trouble whatever, a pure palateable peptic liquid, one which apparently will last any length of time. It is, moreover, to be depended on for its peptic powers; any one who has fairly tested by actual experiment the various "pepsines" of commerce, will understand the value of this remark.

M. Foster

NITRO-GLYCERINE AND GUN-COTTON

It may be of some interest at the present moment to give a brief summary of certain comparative experiments undertaken with nitro-glycerine and gun-cotton, with a view to ascertain their respective destructive nature and safety of employment as industrial or warlike agents. As it is occasionally inconvenient to employ a material of this kind in the form of a liquid, a modification of nitro-glycerine, known as dynamite, and which is simply powdered glass or sand saturated with the explosive, was applied in the experiments; the force of the dynamite very nearly equals that of nitro-glycerine, and is of course much more readily handled than the liquid explosive itself. Nitro-glycerine or its compounds are the only agents of this nature that can compete in any way with gun-cotton, either as regards its igniting force or cost of production; and for this reason the experiments with these two materials have been watched with particular interest by military men, and have indeed formed the subject of a special report recently submitted to Government by the Committee on Explosives.

The explosive force of detonated dynamite and the Abel gun-cotton, as the compressed or pulped form of this material is termed, was considered to be about equal, and on this account the investigation was more particularly confined to the methods of ignition of the two substances. Professor Abel had already shown, in his communication to the Royal Society, that gun-cotton detonated only under certain conditions and but by the instrumentality of particular agents. And here it should be borne in mind that there exists a very great difference between the detonation and inflammation of gun-cotton. A block of the compressed material, for instance, may be set fire to in an ordinary room without the semblance of danger, the cotton burning vigorously and rapidly, it is true, but without any approach to explosion; ignited, however, by means of a small quantity of fulminate of mercury or fulminate of silver, the explosion is of the most violent description. The fulminates above-named are the only ones found to bring about the ignition of gun-cotton in this truly terrible manner; iodide of nitrogen fails to have any effect thereon, and chloride of nitrogen is occasionally successful in doing so, provided it is employed as a primer in sufficient quantity. While, however, gun-cotton is thus very difficult of detonation, except by the use of special means, nitro-glycerine, or dynamite, readily detonates under ordinary circumstances. That is to say, not only do the fulminates above-mentioned secure its ignition, but percussion-cap and other compositions, as also a sharp concussion, inevitably bring about its combustion.

An interesting experiment will indeed show at once the marked difference between the two explosives. Two wooden boxes were filled with compressed gun-cotton and dynamite respectively, and placed in a suitable position at a rifle range, where they could be hit with certainty by small arms. A bullet was fired at each box, and the results were very conclusive; the dynamite detonated in a terrible manner at the shock, while the gun-cotton was merely inflamed, and burned in a rapid but steady manner.

It was further found that in order to secure certain and perfect detonation, it was always necessary to employ a much larger and more powerful detonating fuze (one containing a large amount of fulminate) for the explosion of gun-cotton than was required for nitro-glycerine, proving beyond doubt, therefore, that the latter is much more readily ignited than the fibrous material. This is of course a great safeguard, and added to the fact that under many circumstances of accidental ignition gun-cotton inflames harmlessly and does not detonate, speaks much in favour of pyroxiline. Indeed the use of nitro-glycerine can, according to our best authorities, be applied only within very narrow limits, as, for instance, for blasting and mining purposes, and its employment even in this sphere necessitates very careful supervision.

From this it will be at once seen that the recent prognostications of several of our war correspondents that the Prussians intend to employ dynamite shells in the bombardment of Paris must be entirely without foundation, for, according to the results obtained by the Explosive Committee in this country, the discharge of a nitro-glycerine shell from a gun would be of itself sufficient to bring about the immediate bursting of the arm itself.

ASSOCIATION FOR THE REFORM OF GEOMETRICAL TEACHING

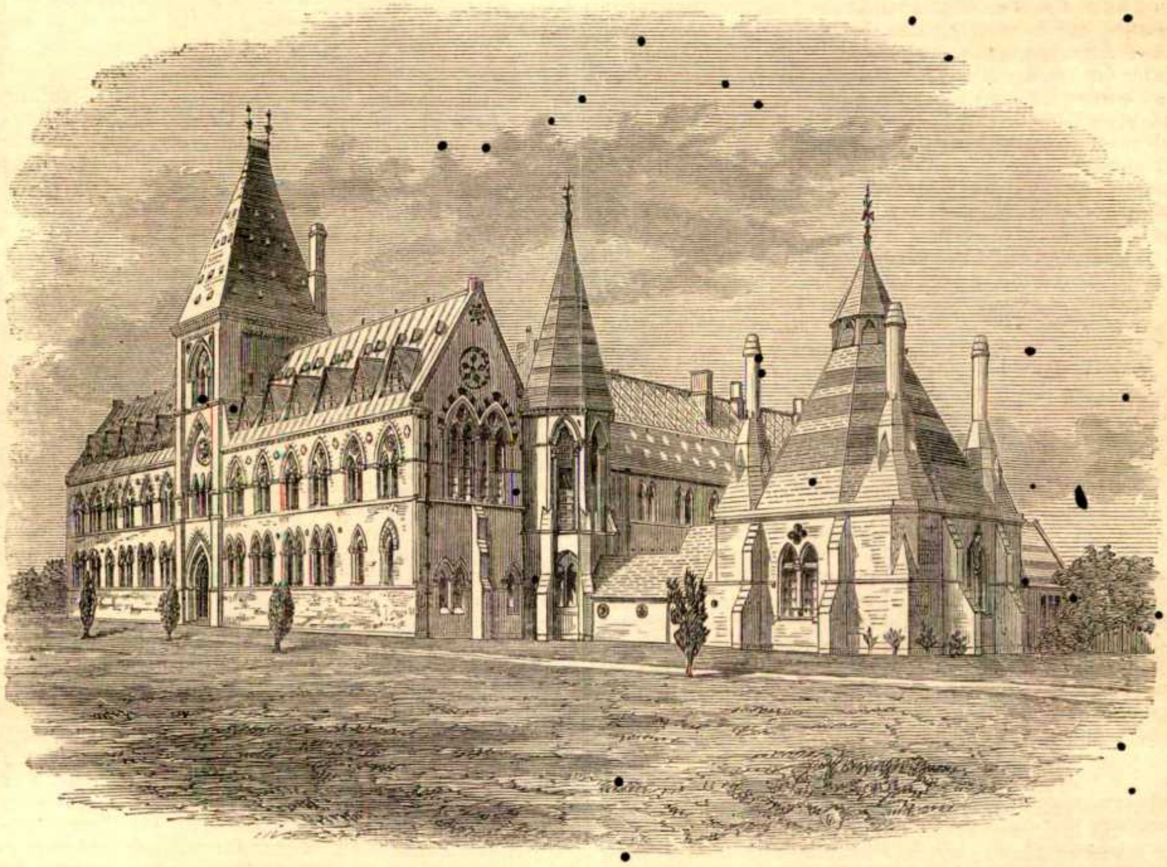
THE following circular has just been issued:—"For some time past an effort has been made to improve the teaching of Geometry in English schools. The undersigned—all mathematical teachers—are of opinion that good would result from the formation of an Association for the Reform of Geometrical Teaching, and are desirous to elicit the opinion of others who may be interested in the movement. The objects of such an Association would be—1. To collect and distribute information as to the prevailing methods of instruction in geometry practised in this and other countries, and to ascertain whether the desire for change is general. 2. To use its influence to induce examining bodies to frame their questions in geometry without reference to any particular text-book. 3. To stamp with its approval some text-book already published, or to bring out a new one under its own auspices. Should you be willing to become a member of such an Association, you are requested to send your name and address, with a small subscription to meet the necessary expenses of printing advertising, &c., to Mr. Levett, King Edward's School, Birmingham. (Signed) Rawdon Levett, M.A., Senior Mathematical Master, King Edward's School, Birmingham; E. F. M. MacCarthy, M.A., Second Master, presiding over the Modern Department, King Edward's School, Birmingham; J. M. Wilson, M.A., late Fellow of St. John's, Cambridge, Mathematical Master of Rugby School; Robert Tucker, M.A., late Scholar of St. John's College, Cambridge, Hon. Sec. London Mathematical Society, and Mathematical Master, University College School."

A Conference is intended to be held on the 17th of January, 1871, at 2.30 P.M., in the Mathematical Theatre, University College, London, Dr. Hirst in the chair, for the following purposes:—The Association will first be organised. The following resolutions will then be proposed: 1. "That the main object of this Association is to induce all conductors of examinations, at which pupils who have been trained under different systems present themselves, to frame their questions independently of any particular text-book; and that with a view to this object, the members present at this meeting do pledge themselves to use every effort to increase the numbers and extend the influence of the Association." 2. "That with a further view of extending the influence of the Association, local secretaries be appointed for different parts of the kingdom, whose office it shall be to collect information, to make the objects of the Association more generally known in their immediate neighbourhood, and to communicate on all matters of interest with the Central Committee." 3. "That the local secretaries, *ipso facto*, be members of the committee of management." 4. "That all members of the Association shall collect information with regard to text-books and methods of teaching geometry in England and other countries, and that such information shall be forwarded to any secretary or local secretary of the association." 5. "That the committee of management shall, from time to time, print and circulate among others such information as they may consider valuable." 6. "That this meeting is of opinion that in any new text-book—(*a*) the following principles, only partially or not at all recognised by Euclid, should be adopted:—(i) hypothetical constructions, (ii) the arithmetical definition of proportion, (iii) superposition, (iv) the conception of a moving point, and of a revolving line; (*b*) the following limitations should be removed:—(i) The restriction of the number of axioms to those only which admit of no proof, (ii) The restriction which excludes all angles not less than two right angles; (*c*) modern terms, such as locus, projection, &c., should be introduced. These points will be voted upon in detail.

NATURAL SCIENCE AT OXFORD

THE progress which Natural Science has made at Oxford within the last few years has far exceeded the anticipations of even the most sanguine of its promoters. It is but ten years ago that the New Museum was opened, and not much longer since the School of Natural Science was founded. Since then, year by year, the interest shown in these studies has steadily augmented, the number of undergraduates attending the University College Science Lectures has augmented in proportion as the number of these lectures has increased, and the School of Natural Science has become recognised as on a par with the other three great schools of Philosophy, Mathematics, and Law and Modern History. This has been chiefly brought about by the high standard of excellence required by the examiners in this school. When the position taken by Natural Science at a university which has commonly been condemned for neglecting this very subject, is fully recognised outside its own walls, there can be no doubt but that a far greater number than at present will come up to Oxford to pursue their science studies there. Hence it may not be here out of place to give as briefly as possible a short *résumé* of the opportunities held out to Natural Science students at Oxford, in the way of university and college lectures and the various scientific museums and libraries, as well as to notice the numerous rewards and honours which are open to all such students. To do this completely would far exceed the limits of this article, so that what follows must only be taken as a sort of index, as accurate as possible, to a subject, the details of which can be obtained by writing to the tutors of the various colleges mentioned.

Taking the rewards and honours first, we would notice that the following colleges award scholarships and exhibitions for Natural Science, after an examination combining both book-work and practical work in any one or any two of the three great branches of Natural Science, Chemistry, Physics, and Physiology.



THE MUSEUM AT OXFORD

Balliol—one of 75*l.* every alternate year; one to be given in 1871.

Merton—one of 80*l.* and one or two exhibitions every year; no limit to age.

Christ Church—one of 100*l.* every year; age not to be above twenty.

Magdalen—one of 75*l.* and one or two exhibitions every year; age not above twenty.

Jesus—one of 80*l.*, generally every year; no limit as to age.

New College—one of 100*l.* occasionally.

Queen's—one of 75*l.* occasionally.

Lincoln—one of 60*l.*; a closed scholarship generally given to Owens College students.

There is but little doubt that many of the other colleges will give similar scholarships as time goes on.

But far greater rewards than these are the various fellowships, of from 150*l.* to 300*l.* per annum, which are open generally one or more every year, either for Natural Science alone or for Natural Science combined with Mathematics. These fellowships are awarded after a competitive examination, and are open to those who have taken their B.A. degree; and, unlike the system in vogue at Cambridge, they are open to all members of the University, and are not confined to the members of the particular college which offers the fellowship. Fellowships have been given for Natural Science at the following colleges: Merton, Pembroke, Wadham, and Oriel, and, we believe, at Queen's and Magdalen, and one is to be given next February at Brasenose for Mathematics or Natural Science. It may not be superfluous to add that those who do not succeed in obtaining such a high reward as a fellowship, if they have taken a first class in the Natural Science School at Oxford, rarely fail to obtain valuable appointments after taking their degree, as Natural Science masters or lecturers at various colleges and public schools, whilst some are induced to stay up at Oxford as demonstrators and assistants to the professors, or else as college lecturers or private tutors.

In addition to the purely college rewards just mentioned, the University offers the following valuable emoluments, the first two only open to those who have taken their B.A. degree. The Radcliffe travelling fellowship, of the value of 200*l.* a year, and tenable for three years, of which eighteen months *must* be spent abroad, and the holder must be studying medicine and ultimately take his medical degree at Oxford. The Burdett Coutts Geological Scholarship of about 60*l.* for two years. A gold medal for the Johnson Memorial Prize Essay on some Natural Science or mathematical subject, awarded every four years. Various special prizes for essays, &c., given to the University by various benefactors.*

In addition to these, all the various open University Scholarships and prizes, so numerous at Oxford, are, of course, open to Natural Science students as to all others.

Every term, speaking generally, courses of lectures are given on the following subjects:—

Chemistry, by Prof. Sir B. C. Brodie, Bart., F.R.S.; Physiology and Zoology, by Prof. Rolleston, F.R.S.; Geology, by Prof. Phillips, F.R.S.; Physics (Heat, Light, and Electricity), by Prof. Clifton, F.R.S.; Botany, by Prof. Lawson, M.A.; Zoology (Invertebrata), by Prof. Westwood, F.R.S.; Mineralogy (occasionally), by Prof. Maskelyne, F.R.S. These lectures are open free to all undergraduates.

Lectures are also given at various Colleges, as at Christ Church, on Chemistry (advanced), by Mr. Vernon Harourt, F.R.S.; on Physics and Mechanics, by Mr. Reinold; and on Physiology, by Mr. Thompson; at Merton on Chemistry (theoretical); at Magdalen on Physiology and Chemistry; at Wadham on Physics and Mechanics, which are open free to the members of the respective Colleges; and on payment of a small fee to others.

In addition to these lectures, a large amount of practical work is made an absolute necessity for a degree in the Natural Science School. Every opportunity for this practical work is given at the Museum, where, under ne roof, all the various splendid collections of comparative anatomy, geology, mineralogy, and instruments or experimental, physical, and chemical science are collected together, and are made available for instruction. It is here also that all the University lectures are given, with the exception of those on Botany, which are given in the Botanical Museum in the Botanic Gardens. The lustration on the opposite page is from a photograph of the Museum taken before the building of the New Physical Laboratory. It would occupy far too much space in the resent article to describe the contents of this Museum, adequately. Suffice it to say that on the left-hand side are the rooms occupied by Professor Rolleston, for practical work at physiology and comparative anatomy and osteology, fitted up with every convenience, and freely communicating with the general collection of specimens on these subjects placed in the Central Court. On the oppote side are the rooms and collections of Professor hillips in geology and mineralogy, and above these in the gallery the magnificent collection of insects and invertebrata under the superintendence of Prof. Westwood. The building on the right-hand side, built apart from the Museum, but connected with it by a narrow passage, is the chemical laboratory. On the opposite side of the central building, not shown in the illustration, is the fine building lately erected as a physical laboratory for Professor Clifton. This building, which is the most perfect physical laboratory in the world, was only opened this term. The collection of physical science apparatus is very valuable, most of it having come from the last Paris Exhibition.

There is every facility given at the Astronomical Observatory for a practical acquaintance with astronomical instruments and methods of observation. There is a good chemical laboratory at Christ Church, as well as one at Magdalen, where, also, the valuable collection of fossils and minerals of the late Dr. Daubeny is open to all working at these subjects. Magdalen, also, has a very good astronomical telescope, and various modern meteorological instruments. A collection of minerals and geological specimens is also in process of formation at Merton. The Botanic Gardens contain every requisite for the thorough study of botany, and in the Museum in connection with it, is a very large and valuable herbarium containing collections of plants for every quarter of the globe.

Lastly, we have to mention what will ultimately tend as much, if not more, than anything else, to make Oxford the great home of Natural Science in the future. We allude to the splendid Radcliffe Scientific Library in the Museum. This Library occupies one side of the building, and consists of two great rooms each 80 feet long, 24 feet wide, and 20 feet in height. It contains the finest collection of scientific books almost in the world, certainly the most accessible. The importance and value of this fine Library cannot be over-rated. Connected with the Bodleian and the Radcliffe Libraries, it contains, as far as possible, complete sets of all the Transactions and Publications of every recognised Scientific Society in the world, and all the new scientific works are added as soon as published. Admirably arranged, admirably managed, freed from all narrow restrictions as to admittance, and open daily from ten to four, and twice a week in the evening from seven to nine, every possible facility is given to those who are working at Natural Science. We do not hesitate to say that until such libraries are founded in other places, Oxford cannot help becoming the great centure of scientific culture in England.

J. P. EARWAKER

NOTES

UNFORTUNATELY the weather does not appear to have been very favourable for the observation of the Eclipse in Sicily. The following telegram has been received from Mr. Lockyer, dated Catania, December 22, 9.40 P.M.:—"Observations of eclipse greatly interfered with by unfavourable weather, but substantial results have been secured. A definite [contact?] of the corona was noticed at a height of about one-third of radius as presented for corona. The sphero-spectroscopic method for first contact was successfully employed. The American observations of last year upon the corona are confirmed." The Astronomer Royal has received the following telegram, which was despatched by Lord Lindsay immediately after the Eclipse. Lord Lindsay's place of observation was La Maria Louisa, which appears to be near Puerto, the mainland station opposite Cadiz:—"Photographs successful. Two good pictures of corona. Polariscope doubtful. Sketching good. Corona [gives] continuous spectrum, no lines. Broken sky." From telegrams received from other members of the expedition, it is hoped that the weather may have been more favourable at the more westerly stations. At Oran, however, we hear that dense clouds covered the sky for twenty minutes before the period of totality, and till after it was over. The previous day there had been a gale of wind.

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* Thus a prize of 50*l.* was awarded last year for an essay "On Longevity" by an anonymous donor through the University; and a prize of 100*l.* is now offered for an essay to refute the materialism of the present age.

DR. RUSSELL, Lecturer on Chemistry at St. Mary's Hospital, has been appointed to the Chair of Chemistry at St. Bartholomew's Hospital, lately occupied by Dr. Matthiessen; who also, up to the time of his appointment at St. Bartholomew's, filled the Chair at St. Mary's.

DR. HENRY E. ARMSTRONG has been appointed Professor of Chemistry at the London Institution, an office once held by Mr. W. R. Grove, Q.C., F.R.S., and subsequently by Mr. J. Alfred Wanklyn. Dr. Armstrong studied chemistry under Professors Hofmann, Frankland, and Kolbe, and has been associated with Dr. Frankland and the late Dr. Matthiessen in original researches. The *Pharmaceutical Journal* states that this appointment is connected with a project for establishing practical chemistry classes in the laboratory of the London Institution.

THE American scientific journals record the death, in his twenty-ninth year, of Edward W. Root, Professor of Chemistry in Hamilton College, a pupil of Schneider, Rose, and Bunsen; From his grasp of mind, power of organisation, and clearness of enunciation of scientific truths, he was one of the most promising of the rising generation of chemists in America.

THE days named for the reception of the different classes of objects at the Annual International Exhibition for 1871 are as follows:—Machinery, February 1, 2, 3, 4; scientific inventions, Feb. 6 and 7; educational works and appliances, Feb. 8 and 9; pottery and raw materials, Feb. 10 and 11; woollen and worsted fabrics and raw materials, Feb. 13 and 14; sculpture not applied to works of utility, Feb. 15 and 16; paintings applied to works of utility, Feb. 17; sculpture applied to works of utility, Feb. 18 and 20; engraving, lithography, photography, &c., Feb. 21; architectural designs, drawings, and models, Feb. 22; tapestries, carpets, embroideries, &c., Feb. 23; designs for all kinds of decorative manufactures, Feb. 24; copies of pictures, mosaics, enamels, &c., Feb. 25; paintings not applied to works of utility, Feb. 27 and 28.

THE Royal Horticultural Society's meetings and shows at South Kensington for 1871 are fixed to take place on the following days, namely, on Wednesdays, January 18, February 15, March 1 and 15, April 5 and 19, May 3 and 17, June 7 and 21, July 5 and 19, August 2 and 16, September 6 and 20, October 4, November 1, and December 6. The date of the great provincial show at Nottingham, originally fixed for June 13—17, has been altered, and it is now arranged that it shall commence on June 27, and continue till July 1. The principal meetings at South Kensington will be those on March 15, April 19, May 17, June 7, and July 5, the latter being the National Rose Show

THE days fixed for the Exhibitions of the Manchester Botanical and Horticultural Society, in the gardens at Old Trafford, next season, are, we understand, as follows:—The National Show, now become one of the most important events of the year, will commence on May 26, and continue till June 2; this will include the Royal National Tulip Society's Show on May 26, 27. In addition to this, a great Exhibition of American plants will be made by Mr. Anthony Waterer, of Knap Hill, the period extending from May 26 to June 17, and an Exhibition of Roses and Fruits will take place on July 7, 8.

THE Royal Botanic Society has just issued schedules for its two spring shows in 1871, which are each to be of two days' duration, and are fixed to take place on March 22, 23, and April 12, 13. As usual, new plants and plants of economic interest are admissible, though not scheduled. The Society's summer shows are announced for May 25, 26 (Thursday and Friday), June 14, 15, and July 12, 13 (Wednesday and Thursday).

A DISCUSSION having been carried on in the French Institute on "Winds," M. de Fonvielle sent to the Secretaries a quotation from the works of the celebrated philosopher, M. Mariotte, the author of the "Law for the Expansion of Gases," explaining the fall of pressure noticed during the south-western winds by the direction of the aerial stream which blows from the lower parts of the atmosphere to the higher. He says, moreover, that winds coming from northern quarters descend instead of ascend, and he accounts in this manner for the augmentation of pressure. This note was published in the *Comptes Rendus* at the time.

M. GAUTHIER-VILLARS, the scientific publisher, has completed the publication of Father Secchi's work on the Sun. It comprises more than four hundred octavo pages, and will certainly be largely circulated when Paris is open. Father Secchi has written it in French, having secured the assistance of some learned Jesuits. It is not, however, merely a translation of his former Italian work on the same subject.

A CORRESPONDENT of the *Gardener's Chronicle* has forwarded to that paper the following account of the condition of some of the nurseries near Paris, dated "Châtenay, Dec. 4, 1870.—I am sorry I have but sad news about the establishments; they are all deserted, and the magnificent collections are perishing. In detail I can only report of the establishments of MM. Croux and Durand Fils; the others near Bagneux, Châtillon, and Bourg-la-Reine we only passed several times at night, when marching to the batteries in course of erection, for the staying there during the daytime is not very agreeable on account of the shells from Vanvres, Montrouge, and Bicêtre. M. Croux's principal establishment at Châtenay is the quarters of the Staff of the Bavarian Artillery; the large Palm-house, sixty to eighty feet long, is occupied by the horses, the flower-tubs being made use of for cribs; the magnificent Conifers (Wellingtonias, Pinus Pinsapo, &c.), of which we found numbers of fine specimens, have all been cut down to form a fence along the road to Fontenay-aux-Roses, to prevent the French from having a look into our batteries from their forts. But the most sad sight is offered by the Jardin pour les études pomologiques, belonging to M. Croux, and situated near Aulnay. The beautifully trained fruit trees, after having been much broken by the pulling out o the wires, which were used for making gabions, are now com pletely eaten down by the 2,000 sheep and 80 to 100 cows shu up in the garden. Nor have the nurseries in the open field been spared; the stems of the young trees had to serve as stakes for gabions, while the branches were used for fagots. A similar sad sight is afforded at the branch establishment of M. Durand Fils, near Clamart; the greenhouses being, to a great extent, demolished by shot coming down here as thick as hail, and the plants they contain are dried up or frozen, for we had -6° R. = 18° F the day before yesterday, and yesterday morning a considerable fall of snow. It will be about the same with the other establishments not visited by me, and it may be taken for granted tha the losses of these people are beyond replacement, and wil bring many of them to the grave."

SILLIMAN'S *American Journal of Science and Arts* will, afte the close of the present year, become a monthly journal. It wa founded by Prof. Silliman in 1818, and now numbers 10 volumes. From its commencement it has been the leading vehicle for the original papers of American scientists.

BRITISH botanists will be glad to learn from Messrs. Longman's lists that Mr. Watson's "Cybele Britannica" may now b obtained from them at the reduced price of 5*s.* per volume. The three parts of the invaluable "Compendium" of the same work ar also issued by them in one volume at 10*s.* The author considers that this "must largely supersede the usefulness of the origina work," of which he still invites any possessor "to apply to him for a free copy of the Compendium as a necessary supplemen thereto." He has already sent a copy to all the possessors o the four volumes with whose addresses he is acquainted.

DR. BUCHANAN, the Professor of Physiology in the University of Glasgow, has just published the third part of his "Essays on the Forces that carry on the Circulation of the Blood." The present part is engaged with the consideration of the Pneumatic Forces. He is of opinion that the ordinary acts of respiration powerfully influence the current of blood in its passage through the whole vascular system. In proof of this he adduces the collapse of the large veins of the neck observed during inspiration, and the fulness during expiration, the former of which phenomena he attributes to the tendency to a vacuum existing in the chest, and the pressure of the external air which empties the great veins of their blood and forces it into the chest; whilst the latter he considers to be due to the fact that, after the termination of an inspiration, no more blood, or very little, enters the chest till the beginning of the next inspiration, and the consequence is that the blood propelled onward by the force of the heart accumulates in the veins near the chest to an extent proportionate to the length of the interval. Again, in order to show that the influence of the respiration extends to the arterial system, he refers finally to the united testimony of all physiologists, who are agreed that the pulse is less voluminous and feebler during inspiration, whilst it recovers its volume and strength during expiration and the period of repose; and, secondly, to the movements of the brain when exposed by removal of the part of the skull. Dr. Buchanan draws attention also to the oscillations of the hæmastatic column observed by Hales, which are clearly associated with the respiratory acts; to the phenomena of asphyxia, and those of the fœtal circulation; the difficulty respecting the latter he ingeniously turns in his favour, by maintaining that, inasmuch as no movements of respiration take place here, the absence of this help to the circulation is supplied by the free communication existing between the right and left sides of the heart, whereby both ventricles are able to exert their influence in maintaining the system in circulation.

THE valuable museum at Brighton is being utilised by the delivery of conversational lectures in the geological, entomological, antiquarian, economical, and sanitary departments. The object is to explain the specimens, and we learn from the *Brighton Examiner* that the plan has been well carried out and has proved very attractive.

GUSTAV ROSE, one of the veterans of German geology, celebrated the 50th anniversary of his doctorate on the 9th inst., when the learned societies of Berlin sent delegates to offer him their good wishes and congratulations.

THE *British Medical Journal* prints the following as an appropriate pendant to the condemnation of tobacco by Kerckringius, cited in our issue for December 8, by John Allen, M.D., F.R.S., on the Evils of Alcohol (*Synopsis Universæ Medicinæ Practicæ*, Amstelodami, MDCCXXX, cap. xvi.) "There remains another sort of poisons, such as vinous spirits and intoxicating distilled liquors. The frequent and excessive tippling of these, as is the practice of each returning day, hath destroyed myriads of mortals, nay, hundreds of thousands more than all the poisons put together; whence I am wont to style this most pernicious evil emphatically THE HARM. It proves not only the parent of very many, and those the worst of diseases, but to numbers suddenly fatal; upon which accounts, if it deserve not the appellation of poison, I must confess I know not what does. Spirit of wine, taken inwardly, is death to almost all creatures; to vegetables of all denominations without exception, when applied by way of pabulum, even to the parent vine, whence itself is derived. The generous physician hath an unpleasant task upon his hands. Men addicted to these spirituous liquors abominably sacrifice day, night, and themselves, to continually sipping, as it were, a liquid fire. When all digestion is lost, the solids unbraced, the juices corrupted; when the human fabric, which hath been long tottering, is just falling to the ground—then are we called in to its support. What must we do? Even as town-scavengers (*scabini*); and ten to one but, after all the abandoned sot returns at once, like a sow that is washed, to wallowing in the mire. Thus he irrevocably prostitutes his health to the last, being prodigal of that life of which he ought to be most tender; and his early end is the consequence of intemperance. What advantageth then the doctor, and what the divine? Fruitless would be the endeavours even of a Luke himself in both his capacities, either as physician or as evangelist. Deaf as a rock to all counsel or persuasion, he runs into the very arms of death, and courts destruction. To this he is prompted by an eternal thirst, which he greedily indulges; and the greater the indulgence, the greater the thirst—the thirst of those pernicious distilled liquors, with which the tragic scene is expeditiously closed; and the dismal catastrophe, in the last moments, is the finishing both his bottle and himself."

A CORRESPONDENT in Honolulu, after making a botanical tour in the Kaala range, says, "Botanising on this island is not without considerable danger. Only imagine descending a steep decline of 70°, which had to be done chiefly by swinging from the roots of one tree to the branches of the next one below, and that at a height of 2,000 feet above the deep gorge beneath our feet." Nature, however, seems in all cases to provide a reward for her admirers, who voluntarily expose themselves to such dangers for the purpose of bringing to the eye of science her numerous hidden beauties, for the writer continues to say, he was not a little surprised by the discovery of a violet with splendid snow-white waxy flowers, some of which were almost half an inch in diameter and exquisitely perfumed. He considers it probably a variety of *Viola chamissoniana*, which he found in its ordinary state lower down in the forest; but the pure white flowers stretching out their long peduncles above the surrounding low undergrowth, and luxuriating in the full sunshine of an azure blue sky, far exceed in beauty those of *V. chamissoniana*, which are of the ordinary violet colour.

FROM the notes of a short tour through the eastern parts of the provinces of Echigo, Iwashiro, and Uzen, made in June and July of the present year by one of H.M. Consuls in Japan, we extract the following:—"In passing through Yazawa and some other villages, we found hemp, said to be of good quality, grown in frequent localities on the way, and vegetable wax trees in abundance. I was informed at Tsugawa that the extraction of lacquer from the same tree is prohibited there, the tree being reserved for the production of wax. As the lacquer is obtained by making incisions in the bark of the tree while young, the result of which is the death of the tree before coming to full maturity, both products can hardly be obtained from the same tree. This appears to be the reason for the prohibition. At Yonezawa, on the other hand, the extraction of lacquer from the tree is permitted, the result of which is, that little vegetable wax is produced there. I observed that many of the trees in the neighbourhood of Tsugawa had been injured, apparently, by the severity of last winter." The trees here alluded to are those belonging to the genus *Rhus*. The most important wax-producing species in Japan being *R. succedanea* L., the bulk of the varnish being yielded by *R. vernicifera* Dec. The wax is obtained from the small fruits, while the varnish is procured by tapping the trees. The species met with at Tsugawa must have been *R. succedanea*, as this species yields both wax and varnish. Several other species also yield varnish more or less in China and Japan. Little is known about the preparations of this varnish as used in the ancient lacquer work of the Japanese; and it is said that the modern workers in this article in Japan have themselves lost the secret of its preparation.

EARLY MENTION OF THE AURORA BOREALIS

THE explanation given by Mr. G. Henry Kinahan* of the superstition prevalent in Ireland regarding "showers of blood," is extremely interesting, and to a great extent the true one; but any student of Irish history must feel how difficult it is to apply it to the interpretation of not a few prodigies recorded in the earliest chronicles. The word "blood" is frequently met with in accounts of wars, in such a manner as to make it quite impossible to construe it into an allusion to the Aurora; nor is it in many places capable of a dual meaning, for instance, the "Chronicum Scotorum"† under A.D. 531, says, in relating the drowning and burning of Muircertach Mac Erca,—

The king Mac Erca, returns
To the side of the Ui Néill;
Blood reaches girdles in the plain;
Territories increase afar.

Mac Erca was killed by a fairy woman named Sin. The words "Blood reaches girdles" are not very clear, but the Aurora cannot be meant. Under an earlier date it is related (A.D. 497)—

The battle of Seghais—
A certain woman caused it;
Red blood was brought over lances
By Duisech, daughter of Duach

The further we go back, the obscurer it becomes, and the greater the difficulty in attaching a consistent meaning, especially such an one as a record of auroral appearances. In Dr. Lynch's "Cambrensis Eversus," vol. i., it says:—

"A.C. 673.—Fionachta succeeded his father in the throne. During his reign an enormous quantity of wine fell like fleeces of snow from the sky."

"A.C. 561.—Elim Ollfinachta . . . . succeeded . . . . He was called Ollfinacha, because snow, which fell during his reign, tasted like wine."

The first of these quotations might be explained by saying that an Aurora was visible during the falling of snow, which appeared red from reflection, but this will not do for the second. Under A.D. 604 (Chron. Scot.) occurs—

Great was the *red sorrow*
Over the chieftains of Erinn all—
Aedh Slaine, with multitudes,
Aedh Roin, Aedh Buidhe, were slain.

Here *red* sorrow means a "bloody sorrow," but I cannot see in this any reference to an Aurora.

For a more direct and satisfactory record of an auroral appearance, the following are given in Chron. Scot.:—

"A.D. 659; A.D. 660—Darkness on the Kalends of May, at the ninth hour; and in the same summer the sky was seen to burn."

"A.D. 670.—. . . . A thin and tremulous cloud, in the form of a rainbow, appeared at the fourth watch of the night of the fifth day before Easter Sunday, stretching from east to west in a clear sky. The moon was turned into blood."

"A.D. 680.—. . . . Loch nEchach was turned into blood." Perhaps this was caused by reflecting the colour of an Aurora.

The "Anglo-Saxon Chronicle" contains the next later observation:—

"A.D. 685.—This year it rained blood in Britain, and milk and butter were turned into blood.‡ The Chron. Scot. follows with:

"A.D. 688.—The moon was turned into the colour of blood on the festival of Saint Martin (11th November)." This is singularly corroborated by the "Brut y Tywysogion" (The Chronicles of the Princes.)§

"A.D. 688.— . . . . it rained blood in the island of Britain, and in Ireland."

"A.D. 689.— . . . a battle against the son of Penda. Bloody rain fell in Lagenia." (Chron. Scot.)

"A.D. 690.— . . . the milk and butter turned to blood." (Brut y Tywy.)

"A.D. 692.— . . . the moon turned of a bloody colour. (Brut y Tywy.)

We now come to a most perplexing record of phenomena, which cannot, I am afraid, be explained; they occurred in

"A.D. 714.— . . . it rained a shower of honey upon Othan Bec; a shower of silver upon Othan Mór; and a shower o blood on the Foss of Laighen." (Chron. Scot.)

If the shower of blood means an Aurora, what do the other showers mean? What is a shower of honey?

The auroral hypothesis will not satisfactorily apply to the following:—

"A.D. 734.— . . . This year the moon was as if it had been sprinkled with blood." (Anglo-Sax. Chron.)

But it may to the following:—

"A.D. 744.—This year a *red* crucifix appeared in the heavens after sunset." (Flor. Wor. and in Anglo-Sax. Chron., under A.D. 743.)

An Aurora is undoubtedly meant in the next record:—

"A.D. 793.— . . . This year dire forewarnings came over the land of the North-humbrians, and miserably terrified the people; these were excessive whirlwinds and lightnings, and *fiery dragons* were seen flying in the air."* (Anglo-Sax. Chron.

This may be taken as the *earliest direct mention* of an Aurora Borealis in England. "Fiery dragons" are not more inapplicable to the phenomenon than the term "merry dancers," til very lately used in the Orkneys.

The next mention of "blood," whether as a celestial phenomenon or not, I shall leave for others to say is:—

"A.D. 811.—This year was a year of prodigies. . . . . I was in it also, cakes were converted into blood, and blood used to flow from them when being cut. . . ." (Chron. Scot.)

Digressing for a moment, I am here reminded of what an Irish woman told me. She said that in Ireland a man, for masticating the sacramental wafer, had a flow of blood from his mouth unti he was well-nigh drowned. This blood-tradition seems not to have entirely lost its hold on the Irish peasantry, although appearing in so many garbs.

A *column of light* is recorded to have appeared, but very few will accept it as an Aurora Borealis:—

"A.D. 819—. . . . heaven afterwards revealed the deed by means of a column of light." (Florence of Worcester.)

"A.D. 850.—. . . . a column of light shot up to heaven, and remained visible to the inhabitants of that place [Repton] for thirty days." (Flor. Wor.)

The next in chronological order is:—

"A.D. 866.—Loch Lebhinn was changed into blood, so that i became clots of gore, like the lights of animals, all round its edge." (Chron. Scot.) I may here remark how difficult it is to say what this record really means, especially when it states tha the Loch became "clots of gore."

"A.D. 878.—It rained a shower of blood, which was found in lumps of gore, and blood on the plains of Ciannachta. . . ." (Chron. Scot.)

Perhaps the following may refer to an auroral appearance:—

"A.D. 890.—The heavens appeared to be on fire at night on the Kalends of January." (Chron. Scot.) But, not so the next:—

"A.D. 898.—Aideidh . . . [was slain] in treachery. . . . . a shower of blood was shed in Ard-Ciannachta." (Chron. Scot.) This looks very much like a repetition of A.D. 878

The next observation does not occur till

"A.D. 938.—The sun was of the colour of blood, from the beginning of one day to the middle of the day following." (Chron. Scot.) I shall not attempt to say what this was caused by, as I cannot conceive the *sun* being visible during the *night*. The Chronicles I have had access to do not mention the word "blood," nor any miracle connected with natural phenomena since this date, that would afford the least ground for surmising that an Aurora Borealis was meant, till

A.D. 944.—When it records:—"Two fiery columns were seen a week before Allhallowtide, which illuminated the whole world." (Chron. Scot.) Knowing under what various forms Auroras appear, it may not be at all extravagant to suppose this "fiery column" to have been such a phenomenon; however, this is not so convincing as the next on record:—

"A.D. 979.—That same year was seen a *bloody* cloud, oftentimes in the likeness of fire; and it was mostly apparent at midnight, and so in various *beams was coloured*. When it began to dawn, then it glided away." (Ang.-Sax. Chron.)

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* See NATURE, December 8, 1870.

† Published under the direction of the Master of the Rolls. Translated by W. M. Hennessy, M.R.I.A., 1866.

‡ Showers of Blood are mentioned as having taken place in Tit. Liv. Book 42, Sect 20. It says: "There was a report of it, having rained blood for three days at a town in Italy." And in Pliny, Book 2, Chap. 56, "It rained blood when M. Acilius and C. Persius were Consuls."

§ Published under the direction of the Master of the Rolls. Translated by the Rev. John Williams Ab Ithel, M.A., 1860.

* In "The Philosophical Grammar" by Benj. Martin, 1738, there is given a list of fiery meteors, and among them are the various forms of the Aurora Borealis:—"*Ignis Pyramidalis*, the pyramidal fire, when it resembles a pillar of fire standing upright, *Draco Volans*, a flying dragon, when the middle parts be thicker and broader than the ends, *Capra Saltans*, a skipping goat, when it appears to have a skipping motion, to be sometimes kindled and sometimes not." (Pages 204, 205)

Florence of Worcester mentions it as having been seen in

"A.D. 978.—At midnight [the 18th of the Kalends of May (14th April)], there was seen throughout all England a cloud, which was sometimes of a blood-colour, and sometimes fiery; it afterwards broke out into rays of different colours, and disappeared about daybreak." (Chron. Flor. Worc.)

Gaimar, in his "History of the English," repeats this in the following manner:—

"A.D. 978.—At night, as he [the murdered King Edward] lay in the moat, a heavenly light spread itself there; the light was bright (no wonder!) it very much resembled the sun. This ray came over the holy body—the top of it was in heaven."

Putting down 978 as the correct date, and which is confirmed by William of Malmesbury (so far as the death of King Edward is concerned), this year may be accepted as the one in which an Aurora was seen.

From the last date till the year 1052, I cannot find any mention of "blood"-phenomena, or direct references to auroral appearance, and again in this year the evidence is very vague; it says:—

A.D. 1052.—A tower of fire was seen at Ross-Deala, on the festival of St. George, during the space of five hours, blackbirds innumerable going into and out of it, and one large bird in the middle of it." (Chron. Scot.)

The next, still more puzzling, will be my last:—

"A.D. 1103.—In the province called Berkshire, in a place called Heamstede, *blood* was seen by many to flow out of the ground." (Chron. Flor. Worc.)

I think that it will now appear from this account that any attempt to lay down *one* meaning for the whole of the numerous mentions of blood-appearances must fail, although it is, in a few instances, very clear that an Aurora is meant; but it seems as if a distinctive interpretation must be applied to the entries above given. On the whole, it is quite certain that this phenomenon has been seen at very remote dates by the inhabitants of Britain and Ireland; also that the enigmatical "blood"-miracles were not confined solely to Ireland, but seem to have been revealed alike to the *Keltic* inhabitants of England and Wales. I say *Keltic*, because I find the majority of records among the Chronicles to relate more especially to the *Ancient Britons* and *Irish*, and in many cases I believe (as one may judge from the above chronology) the style of poetical descriptions and form of mythical allusions are Keltic. The well-known Druidical Hymns, which appear in old Irish literature, are fair specimens of what I mean. One in particular concludes a mythical story with, "and the third (brother), guided by the lightning from his brother's fingers, shoots an arrow at the swimming hag, who immediately disappears in a *pool of blood*."*

There are two vague poetical descriptions which I imagine to have been suggested by the Northern Lights, in Hesiod's "Theogony," where he describes the war between the Gods and the Giants in the West. (Elton's Translation.)

He says:—

> The gods from Saturn sprung, and those whom Jove
> From subterraneous gloom releas'd to light,
> Terrible, strong, of force enormous, burst
> A hundred arms from all their shoulders huge.
> (Lines 884—887)
>
> . . . . . Through the void
> Of Erebus, the preternatural glare
> Spread, mingling fire with darkness.
> (Lines 924—926)

From astronomical calculations this war is stated to have taken place at the autumnal equinox in the year 756 B.C., and to have terminated at the era of Nabonassar; so that such an appearance *may possibly* have assisted Hesiod in composing this poem. The Hindu astronomers also seem to have heard of, or seen a heavenly phenomenon, which I imagine to have been something like an Aurora, if the hypothesis be true, that with the ancients natural phenomena were invariably made the themes of their verse, and were shrouded in allegorical descriptions. The most remarkable passage I have met with occurs in the Mahābhārata, Book i. chap. 15 (Wilkins's Translation):—

"They now pull forth the serpent's head repeatedly, and as often let it go; whilst there issued from his mouth, thus violently drawing to and fro by the Suras and Asuras, *a continual stream of fire*, and smoke, and wind, which, ascending in thick clouds replete with lightning, *it began to rain down upon the heavenly bands*, who were already fatigued with their labour."

The date of this supposed war is placed at 945 B.C.

If the foregoing passages be compared with what the "Prose Edda"* says, the hypothesis will not appear unreasonable. In the chapter on "The Twilight of the Gods, and the Conflagration of the Universe," it says:—

> Midgard's protecting ward
> Bravely fights and slays
> The serpent monster.
> Then shall all mankind
> The earth abandon.
>
> Dimm'd 's now the sun,
> In ocean earth sinks;
> From the skies are cast
> The sparkling stars;
> The fire-reek rageth
> Around Time's nurse,
> And *flickering flames*
> With heaven itself playeth.

The idea of "flickering flames" is original, or, at least, not borrowed from the Eastern poets; and, in my judgment, could only apply to the Aurora, it may be an extraordinary appearance of it; and as the Aurora, which has been seen in England this year, was also visible in India, I think it not at all unlikely that "a continual stream of fire," which "began to rain down," is a record of a similar extensive phenomenon.

Dec. 20 JOHN JEREMIAH

BALLOON ASCENTS FOR MILITARY PURPOSES

III.

THE laws of the motion of a balloon, dependent on the change of level, appear to have been hitherto very little discussed from a scientific point of view. It is, however, a motion which can be procured very easily by throwing out a small quantity of sand, or of gas, if the balloon is properly constructed, and which is of great importance for any expedition in time of war, more perhaps than even the attempt at guiding its direction. The number of minutes required for descending from a great altitude as well as for ascending to a certain level, being the most important consideration for the aeronaut endangered by the vicinity of some foreign force, this was analytically examined by M. Dupuy de Lome. It is the first instance that I know of such a disquisition since Euler worked his equations relating to the elevation of an aerostatic sphere supposed to be inextensible, and to be carried away in the atmosphere with a certain amount of motive power due to the small specific gravity of the included gas. That beautiful analytical disquisition is the last ever written by the old philosopher, who was totally blind at the time. It was found written by him on the *tableau noir* where he was making his calculations on the very day before he died. He had received the intelligence of the great experiment tried by Mongolfier, and his excited brain had produced during the night that masterly piece of mathematical skill which was unhappily his last! This contribution to scientific ballooning is to be found in the "Mémoires de l'Académie des Sciences de Paris" for 1781, a date anterior to the experiment of Mongolfier, which is accounted for by the issue of the volumes being always later than the date inscribed on them. Another singularity is that Euler speaks of gas for filling the balloon, while Mongolfier's was merely heated air. We must not, however, give Euler the merit of having been the real inventor of *Charlières* or gas balloon, as Mongolfier believed that he prepared gas by burning damp corn-straw! Before returning to the questions considered by Dupuy de Lome, we may be allowed to mention that the use of gas enclosed in a gasholder of any description, was suggested by Blake in his lectures at Edinburgh, and by an Italian philosopher transacting business in London. Carvalho tried to give to the idea of Blake the shape of an experiment, but uselessly, for the want of a proper varnish, which was invented by Charles a few months only after Mongolfier's great experiment.

The principal difficulty for strangers to the scientific working of an aerostat, appears to be to draw a broad line between the *motive power* or *force ascensionnelle*, and the *space* offered for free dilatation without any gas being lost in the air, which space I will call *the dilatation chamber*, although generally there is no special balloon to allow for this, but the lower part of the balloon is left unfilled.

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* See Thierry, "Hist de Gaull.," and Pritchard's "Eastern Origin of Celtic Nations."

* Mallett's "Northern Antiquities" (Bohn's Edition, 1847, p. 455).

The *Géant* had a special balloon for this purpose attached to the bottom of the large one by a kind of short connecting tube of a very large diameter, and this balloon was called the *compensator*. The arrangement can be seen by the engravings showing the *Géant* in *aerial travels.*

If we suppose that we have at our disposal a balloon of india-rubber susceptible of any distension, we are in a position to assimilate the motion of our aërostat to the elevation of an *Atwood's Machine* moving upwards with a certain moving weight, without any other friction than that of the air. All the calculations and formulæ worked for that philosophical instrument can be used. a = the weight of the air replaced; b = the weight of the balloon including gas and everything; $\frac{a-b}{a}$ will be the motive power; the motion will take place according to the rule for accelerating powers in a medium where the pressure is diminishing, as is the case with the atmosphere when the balloon is ascending. If we pay no attention to the friction on the air, which is very small indeed when motion is slow, we have an acceleration of motion varying as t, and a height obtained varying as t^2, as is well known. If g' is the new motive power and g the motive power of the ordinary specific gravity, then $g' = g \times \frac{a-b}{a}$; $\frac{a-b}{a}$ remaining constant under the assumption we have given.

The best way to realise this assumption is to suppose that the balloon is partially empty when leaving the surface of the earth, and the assumption holds good as long as the balloon is not filled up by dilatation. The time taken by the dilatation to fill up the space allowed to it, regulates the level where the balloon can ascend without losing any of its motive power. This friction must be considerable in cases where the balloon is to be sent to a great distance from the earth. The chamber of dilatation must be chosen in proportion to that distance. At all events it must be calculated thus: R being the radius of the sphere $\frac{4}{3}\pi R^3$ is the maximum volume which the gas is able to take without escaping into the atmosphere, C being the weight of gas and Δ its gravity for each cubic metre $\left(\frac{4}{\pi}R^3 - \frac{C}{\Delta}\right)$ is the number of cubic metres which can be afforded for dilatation, S_0 H_0 being the actual pressure on the earth. The pressure can be diminished in the proportion $\dfrac{\frac{C}{\Delta}}{\frac{4}{3}\pi R^3}$ without the gas beginning to escape. If we call H_1 the altitude where this escape is to begin, we can write the equation $H_1 = H_0 \dfrac{\frac{C}{\Delta}}{\frac{4}{3}\pi R^3}$ To ascertain the actual value of the corresponding altitude, it is necessary to look at the empirical tables inserted in the *Annuaire du Bureau des Longitudes* of each year, and calculated for reckoning the altitude from actual barometric pressure. These tables were calculated by M. Mathieu on the assumption of the truthfulness of Laplace's equations given in his *Mécanique céleste;* but I suspect these assumptions are not sound, and may possibly mislead French aeronauts, while Prussians are watching them below ready to shell their balloon if it comes within range. But having instituted no direct measures for ascertaining the value of this law, aeronauts are obliged to make use of it. The calculations of M. Dupuy de Lome for carrying his intended balloon out of range suppose the truthfulness of the numbers of the *Bureau des Longitudes.* If in doing so aeronauts are not sure of escaping hostile bullets, they can at all events get rid of every analytical obstruction, which is certainly something.

When the escape begins, the motive power is not destroyed at once. It certainly diminishes at a very quick rate if the *appendix* is wide enough to give free issue to the gas, and the vertical motion is also rapid. If not there is some danger of explosion, as can easily be imagined. Generally aëronauts are very anxious to get rid of this danger, which can be done very easily by opening the escape valve. But this operation involves the aëronaut in a "sea of troubles," as the valve for discharging the gas is rendered gas-tight only by the application of a proper plaster. The only way of dealing rationally with that excess of gas is to have a proper vertical motion when the discharge begins. That condition is very easy to obtain if you start with a very small ascensional power indeed.

In the fraction $\frac{a-b}{a}$ is very small, and moreover if the fraction $\dfrac{\frac{C}{\Delta}}{\frac{4}{3}\pi R^3}$ is large enough, you can conduct your balloon very safely at any distance if you do not meet with dark clouds, or burning sun. You can get rid very easily of these conditions by disposing cleverly of your ballast, as will be very easily understood by everybody after some explanation. There is however a circumstance which is very annoying in our military ascents, and on which it may not be useless to say a few words: the necessity of going at a certain level before reaching the meridian of the enemy's lines. The aëronaut is therefore obliged to know that distance and the quickness of the motion of the wind, so that the fraction $\frac{L}{V}$ gives the number of minutes at his disposal to reach the level required above that dangerous meridian. V = medium velocity of the wind, as obtained by balloons sent free into the air, S = elevation which the enemy's bullets cannot reach, I suppose it to be 6000 yards. Having no proper experiments at my disposal, I beg leave to make that gratuitous assumption. M. Dupuy de Lome, in his contribution, speaks only of 3000 metres, but Krupp's cannon was not in operation when he was writing. L = distance of enemy's lines from the workshop. It is of great importance to increase that distance as far as possible, and I advised the Government to take two starting stations, one from the northern bank to be used when the wind was blowing southerly, and one from the southern when the wind was blowing northerly, so that in every case aeronauts might cross the whole of our city. But the suggestion was disregarded. The solution adopted was to start in the night time! It is a singular mode, very unscientific, to solve an analytical problem by the sending of the aëronauts either to the great ocean to be drowned, or to Norway to be frozen. The subject is far from being exhausted as to the rebounding of the balloon according to the law of oscillatory motions. But fearing to extend my remarks to a length beyond the patience of my readers, I beg leave to end my contribution at this point, thanking the editor of NATURE for the hospitality exhibited towards a French aeronaut, and hoping to be more fully acquainted with the English scientific public on some future occasion.

W. DE FONVIELLE

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 22.—"On the Constitution of the Solid Crust of the Earth." By the Ven. Archdeacon Pratt, M.A., F.R.S. In this paper the author applies the data furnished by the pendulum-observations recently made in India, to test the truth of the following hypothesis regarding the Constitution of the Earth's Crust, which he propounded in 1864, viz. that the variety we see in the elevation and depression of the earth's surface in mountains and plains and ocean-beds has arisen from the mass having contracted unequally in becoming solid from a fluid state; and that below the sea-level, under mountains and plains, there is a deficiency of matter, approximately equal in amount to the mass above the sea-level; and that below ocean-beds there is an excess of matter approximately equal to the deficiency in the ocean when compared with rock; so that the amount of matter in any vertical column drawn from the surface to a level surface below the crust is now, and ever has been, approximately the same in every part of the earth. In order to make this hypothesis the subject of calculation, the author takes the case of the attenuation of matter in the crust below mountains and plains, and the excess of matter below ocean-beds, to be *uniform*, to a depth m times the height above the sea-level or the depth of the ocean, as the case may be. The results are shown in the following Table, in which the numbers are the last figures in the ratio of the differences of gravity to gravity itself, carried to seven places of decimals. The decimal point and ciphers are omitted for convenience.

Stations.	Differences of Gravity.			
	Relative effects of local attraction deduced from pendulum observations	Residual errors after correction by the method of		
		Dr. Young.	This Hypothesis.	
			$m = 50$.	$m = 109$.
Indian arc stations.				
Punnæ	—	—	—	—
Bangalore . . .	+384	−562	− 78	−557
Damargiæa. . .	−323	−926	−455	−584
Kalianpur . . .	+341	−208	+338	+315
Kaliana	−707	−957	+ 69	+320
Coast stations.				
Punnæ	—	—	—	—
Alleppy	+302	+314	+331	+360
Mangalore . . .	−166	−154	−122	− 79
Madras.	−197	−192	−138	− 78
Cocanada . . .	+142	+153	+216	+291
Ocean station.				
Minicoy Island.	+894	+906	+ 31	+102

The author points out from this table that Dr. Young's, or the usual method of correction for local attraction, so far from improving matters, introduces very large residual errors of the arc and ocean stations; and, at places on the arc of meridian, all lying on the same side with reference to Punnæ. He observes that neither the usual method nor his own much affects the coast-stations, and attributes this to the want of more complete knowledge of the contour of the surface, both above and below the sea-level, in these parts. But his own method, in the case $m = 50$, remarkably reduces the effects of local attraction at stations on the arc of meridian and out at sea (in Minicoy, an island 250 miles west of Cape Comorin or Punnæ); for the sensible negative quantity at Damargiæa and positive quantity at Kalianpur indicate a deficiency of matter below the first and an excess below the second—which exactly tally with the results independently brought out by relative deflections of the plumb-line as obtained by the survey; and the two large and most important effects, negative at Kaliana and positive at Minicoy, may be said to be almost annihilated by this method of correction. This last case of an *excess* of gravity out at sea (where the surrounding ocean has a deficiency of matter) being explained by his method, he regards as a very strong argument in its favour. And he finishes by saying that if his method is thus far successful in the particular supposition of the distribution below, whether in excess or defect, being *uniform*, which is most likely not strictly the case, there is every reason for concluding that pendulum-observations give support to the hypothesis regarding the constitution of the Earth's Crust, when viewed on a large scale, admitting of local peculiarities, like the deficiency of matter near Damargiæa and the excess near Kalianpur, and the similar deficiency near Bangalore.

"On the Extension of the Coal-fields beneath the Newer Formations of England; and the Succession of Physical Changes whereby the Coal-measures have been reduced to their present Dimensions." By Edward Hull, M.A., F.R.S., F.G.S., Director of the Geological Survey of Ireland. In this paper the author, embodying with his own the observations of previous writers on the physical geology of Great Britain, especially those of Murchison, Godwin-Austen, Ramsay, Phillips, and the late Professor Jukes, showed that the Coal-measures were originally distributed over large tracts of England, to the north and to the south of a central ridge or barrier of Old Silurian and Cambrian rocks, which stretched across the country from North Wales and Shropshire into the Eastern Counties, skirting the southern margin of the South Staffordshire Coal-field. This barrier, or ridge, was a land-surface till the close of the Carboniferous period. To the north of the central barrier, the highlands of Wales, the mountains of the Lake district, and probably small tracts of the southern uplands of Scotland, formed land-surfaces skirting portions of the Carboniferous area, while the Carboniferous tract to the south of the central barrier was probably bounded by a land-surface trending along the southern coast of England. The distribution of the Coal-measures at the close of the Carboniferous period was illustrated by a map. It was then shown that the whole Carboniferous area was subjected to disturbances through the agency of lateral forces, whereby the strata were thrown into folds along axes ranging (approximately) in east and west directions; and as denudation accompanied and followed these disturbances, and acted chiefly over the arches (or anticlinals), large tracts were divested of Upper Carboniferous strata, and thus the first phase in the marking out of the limits of our present coal-fields was brought about. The effects of these movements and denudations were illustrated by another map. The disturbances which ensued after the deposition of the Permian strata, and which produced the discordances of stratification between the newer Palæozoic and Mesozoic formations, were shown to have acted along lines ranging approximately north and south, parallel to the axis of the Pennine Chain, and consequently in a direction transverse to those of the previous period. These disturbances were also accompanied by the denudation of strata from off the anticlinal arches, and the consequent disseverance of the coal-measure tracts over certain definite areas. The results of these movements (the second phase in defining the bounds of the coal-fields) were illustrated by a third map. From a consideration of the foregoing observations, the author came to the conclusion that the tendency of the British coal-fields to arrange themselves into the form of "basins" (sometimes partially concealed by newer strata), a tendency strongly insisted on by Professor Ramsay, F.R.S., was due to the intersection of the two systems of flexures above described, one anterior to the Permian period, the other anterior to the Triassic period, and that the actual disseverance of the coal-fields into basins was due to denudation acting with greatest effect along the anticlinal arches of these flexures. The inference that the Yorkshire and Durham coal-fields are really basins rising to the eastward under the Mesozoic strata was drawn—an inference supported by the easterly rise of the coal-measures along the sea-coast from the Coquet to the Tyne. Guided by these principles, the author maintained that we were now in a position to determine with great accuracy the actual limits of the coal measures under the Mesozoic formations over the area to the north of the central barrier ridge, and that to the south of the ridge the application of the same principles would assist towards the solution of the question, though in a less degree, owing to the fewer opportunities for observation of the Palæozoic formations. The author, however, concurred in the views advanced by Sir R. I. Murchison,* that in consequence of the great amount of denudation which the carboniferous rocks had undergone over the area o the south of England previous to the deposition of the Mesozoic formations, little coal was to be expected to remain under the Cretaceous rocks.

Anthropological Society, December 20.—Dr. Charnock, V.P., in the chair. The following were elected Fellows, Mr. H. W. Bellew, Peshawur, India; Mr. F. Tagart, F.R.C.S.; and Mr. C. Cornish Brown. The Rev. W. W. La Barte, M.A., Local Secretary for Brighton. Mr. A. L. Lewis read a paper, "On some Archaic Structures, principally Megalithic, in Cornwall and Devon, with remarks on their probable Uses." The author drew a comparison with similar monuments in other places, and illustrated the paper by the exhibition of models, from his own sketches and measurements, and by photographs. The structures described were, a circle called Dance Maen, two circles both called "Nine Maidens," three circles together called the "Hurlers," all in Cornwall; and one on Dartmoor, Devon, all which the author believed to have been used for sacrifice, and compared with some so used in India; "Chun Quoit," a sepulchral dolmen; "Lanyon Quoit;" "the Spinster," Devon; "the Trevethas Stone," Cornwall, used for sacrifice; the "Men-an-Tol," Cornwall, a monument of perhaps a phallic character; and the ancient towns called Chun Castle, Cornwall, and Grimspound, Devon.—Dr. Henry Muirhead contributed some remarks on the Difficulties of the Theory of Natural Selection. The opinions of Mr. A. R. Wallace on the subject were criticised; and the term "survival of the fortunate" suggested for that of "survival of the fittest," adopted by Mr. Wallace from Mr. Herbert Spencer. The subject of individual variation was discussed at some length, and the laws of divergence commented on. The author expressed his dissent from Mr. Wallace's interpretation of Darwin's theory.

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* In his address at the meeting of the British Association at Nottingham, 1866. On the other hand, the views of Mr R. Godwin-Austen, F.R.S., which tend rather in an opposite direction, should be well weighed by all who are interested in this question.—*Quart. Journ. Geol. Soc.* vol.

Mathematical Society, Dec. 8.—Mr. W. Spottiswoode, F.R.S., president, in the chair. Mr. J. Hamblin Smith, M.A., of Caius College, Cambridge, was elected a member.—Prof. H. J. S. Smith made a communication on the subject of Elliptic Integrals.—Prof. Cayley read a note on his former paper, "On the Theory of the Rational Transformation between two Planes, and on Special Systems of Points," followed by an account of an addendum to his recent memoir on "Quartic Surfaces." In this latter communication he stated the following theorem :—Take any seven points ; an eighth point at pleasure on the dianodal surface of the seven points ; a ninth point at pleasure on the dianodal curve of the eight points. In the system of nine points so determined take any one as vertex, and joining it with the remaining eight, construct the ninth line of the "ennead": we have thus nine lines passing through the nine points respectively. These nine lines meet in a point which is the "enneadic centre" of the nine points; and further, the ten points form a completely symmetrical system, so that each one of them is the enneadic centre of the remaining nine. [The name "ennead" is given to any nine points *in plano*, which are the intersections of two cubic curves, or to any nine lines through a point which are the intersections of two cubic curves ; the ten points in space are such that taking any one whatsoever as vertex, and joining it with the remaining points, the nine lines form an ennead.] The author stated the following system of correspondence as a subject for investigation—viz., given any eight points in space ; then to every point in space corresponds a line through this point, viz., the ninth line of the ennead obtained by joining the point with the eight given points respectively ; and to each line in space a point or points on the line, viz., the point or points for each of which the line is the ninth line of the ennead obtained by joining the point with the eight given points respectively.—Dr. Hirst entered at some length into an explanation of the methods employed in his paper "on the Polar Correlation of two planes, and its connection with their Quadric correspondence." Profs. Cayley, Smith, Mr. Cotterill, and the author took part in a discussion on the paper.—Prof. Henrici exhibited a large model of Dr. Sylvester's amphigenous surface, which has for its equation

$$JK^4 + 8LK^3 - 2J^2LK^2 - 72JL^2K - 432L^3 + J^3L^2 = 0$$

where $$K = \frac{D - J^2}{128}$$

The equation of the surface is obtained by substituting $x = 1024\,L$, $y = \frac{2}{5}\,D$, $z = 6\,J$ and taking x, y, z as rectangular cordinates. The unit was taken $= \frac{1}{5}$ of an inch. The sections parallel to the axes of cordinates are unicursal curves. Thus the cordinates x, y, may be expressed and terms of z and a parameter ϕ :—

$$x = \frac{1}{64}\,\frac{1}{\phi^3(\phi + 1)}\,z^3$$

$$y = \frac{1}{64}\left(\frac{\phi + 2}{\phi}\right)^2 \frac{\phi - 3}{\phi + 1}\,z^2$$

The surface is of the ninth order, and has two cusp lines. The one is a common parabola in the plane $L = 0$, and has the equation $K = 0$ or $D = J^2$. It is of the second species, that is to say, any plane section of the surface possesses a cusp of the second species where it cuts it. The second is a common cusp line. It is a curve of double curvature of the third order, and has the equations

$$27\,L = -J^3\,;\quad 3\,D = -125\,J^2$$

thus the projections on the three axes are a common parabola, a semi-cubic and a cubic parabola respectively. Both cusp lines touch one another at the origin, where the axis of J is a common tangent ; thus the origin is a triple point, as appears also from the equation. The surface divides the whole space into two congruent parts. If we turn the surface through an angle of 180° about the axis of D, which is altogether on the surface, one half will take the original position of the other. The plane $D = 0$ touches the surface along a curve $2048\,L = J^3$, and divides each half of the space, separated by the surface, into three distinct parts. It is this property, which connects the surface in so remarkable a manner with the theory of binary quintics, and by aid of which Dr. Sylvester has shown (Phil. Trans. Part iii. 1864) how to decide whether the roots of an equation of the fifth degree are real or imaginary.

BERLIN

German Chemical Society, November 14.—Alex. Müller reported on the determination of very minute quantities of sulphuric acid in water. His method requires but small quantities of water, and consists in adding to it a weighed quantity of chloride of barium and an equivalent proportion of chromic acid. For every equivalent of sulphuric acid present in the water, one equivalent of chromic acid remains free, and can be determined by colorimetric comparison. He was thus enabled to determine one milligramme of sulphuric acid in fifty grammes of water.—Messrs. Emmerling and Engler have prepared phenyl-methyl-aceton : $C_6\,H_5\text{-}CO\text{-}CH_3$ by distilling together benzoate and acetate of lime. Nitric acid converts this body into two isomeric nitro-compounds of the formula $C_6\,H_4\,(NO_2)\text{-}CO\text{-}CH_3$, one of which is crystalline and the other a liquid. These compounds have acquired an unusual interest in the hands of Emmerling and Engler, serving as they have done for the first artificial formation of indigo blue. Indigo blue having the composition $C_8\,H_5\,NO$, the nitro-compound mentioned $C_8\,H_7\,NO_3$ has to lose $H_2\,O$ and O to yield $C_8\,H_5.NO$. This it does under the influence of soda and powdered zinc. The transformation is somewhat analogous to the reaction lately described by Baeyer, by which nitrocinnamic acid, $C_9\,H_7\,NO_4$, submitted to the action of powdered zinc, loses CO_2 and O, and forms indol, $C_8\,H_7\,N$. It appears that two molecules of nitro-phenyl-methyl-aceton enter into the reaction, and that the formula of indigo blue ought to be doubled, thus :

$$\begin{array}{ll}C_6\,H_4.\,CO.\,CH\,[H_2] = [H_2]\,HC.\,CO.\,H_4\,C_6 \\ N[O_2] \qquad\qquad\qquad\qquad\qquad\qquad [O_2]\,N\end{array}$$

The elements put in brackets are those eliminated in the reaction, by which apparently the nitro-groups are converted into azo-groups. Nascent hydrogen transforms the azo-groups into the hydrazo-groups, that is to say, the artificial indigo-blue into indigo-white. The latter reaction has been employed to identify the artificial with the natural product.—L. Henry indicated a practical way of forming iodate of potassium, by treating the corresponding chlorate with protochloride of iodine : $K\,Cl\,O_3 + I\,Cl$ gives $K\,I\,O_3 + Cl_2$.—Dittmar and Kékulé have prepared a glycolic acid of the aromatic series. Cymol from camphor was converted into toluylic acid, then into bromotoluylic acid, and by the action of baryta water into oxytoluylic acid $C_6\,H_4 \begin{array}{l}CH_2\,OH \\ CO\;\,OH\end{array}$ —E. Erlenmeyer has studied the action of cyanamide on the hydrochlorides of compound ammonias, particularly of methylamine. He has thus produced methyl-guanidine, hitherto called methyluramine, and obtained by oxydising kreatine. It appears that crystallographic differences exist between the platinum salts of the artificial and of the natural compound. The same chemist has found that ordinary butylic alcohol yields isobutylic and acetic, but not propionic acid. He likewise communicated researches on the differences of the various valerianic acids.—A. Lieben communicated his views on the formation of chloral-alcoholate, thinking that trichlorinated acetal precedes the formation of the above compound.—A. Bunge reported on the electrolysis of some sulphur compounds.—C. Liebermann has investigated the substance described some years ago by Roussin under the incorrect name of artificial alizarine. It is formed by gradually adding nitro-naphthaline and zinc to sulphuric acid previously heated to 200°. Brown crystals are thus separated, giving colours with alkalies of a different hue from those produced by alizarine, and showing the composition $C_{10}H_6O_4$. It appears to be binoxynaphthochinone, and to stand in the same relation to naphthaline in which alizarine stands to anthracene. He calls the substance naphthazarine ; the colours it gives are of no practical interest.—C. Vogel reported on the practical production of oxygen and of hydrogen by the New York Oxygen Gas Company. This company prepared in the month of August 20,000 cubic feet of oxygen a day at the price of five cents a cubic foot. The gas is pressed into copper reservoirs under a pressure of ten atmospheres, and largely used for laboratory and medical purposes, but chiefly for hydro-oxygen lamps in bridge-building under the surface of rivers. In lecture rooms also this kind of illumination is largely used to procure enlarged views of small photographs or drawings made on gelatine. The process for making oxygen is that first used by Jessie du Mothay. Iron torts are filled with 900 lbs. of a mixture of peroxide of manganese and caustic potash. A current of air passed through the torts heated to 450°, transforms their contents into manganate. current of steam is then passed through which sets the oxygen ee and leaves a residue of sesquioxide of manganese and caustic otash. These processes may be repeated innumerable times. ydrogen, together with carbonic acid, is produced on a smaller cale by heating to redness retorts filled with anthracite and ydrate of lime, for fifteen minutes. Afterwards steam is passed rough to reproduce hydrate of lime. The same mixture can us be used for three weeks. The hydrogen is sold at two cents cubic foot in copper reservoirs holding sixty gallons similar to ose described above, which are sold by the company at about ghteen shillings a piece. The sale amounted in August only to ,000 cubic feet a day, as coal gas is generally used in preference hydrogen.

November 28.—C. Rammnelsburg reported on the composition f tantalites and niobides. — A. Muller described a furnace for rganic analysis.—Fr. Mohr in a lengthy paper tried to refute e laws of Avogadro and all modern theories in general. The me communicated his views on the connection of the conducting ower for heat and the molecular movements of gases.—J. Thomson s determined the heat of combustion of hydrogen equal to 340·00. his number stands between those found by Andrews (338·00) and Farne and Silbermann (344.00). He has also determined the antity of water yielded by the combination of two litres of ydrogen, and found a number closely coinciding with that given y Stas. The same chemist described an apparatus for showing e combustion of oxygen in an atmosphere of hydrogen and a ode of exhibiting the alternate reduction and combustion of opper.—C. Bender has analysed some combinations of the ydrate and the chloride of magnesium.

VIENNA

I. R. Geological Institute, November 22.—Fr. von Hauer elivered the anniversary address, with a report on the progress ade by the Geological Survey during the past year.—Baron oust. Beust gave a description of the layers of zinc and lead es on the Schneeberg, in Tyrol. Although known for a long me, their real value has never been appreciated. The parts of e bed at present opened contain, after a moderate deduction, es to the value of more than eighty million florins (eight illion sterling). All measures are being taken for an extensive vestigation.—M. F. von Hochstetter exhibited miniature volnoes formed by sulphur in a soda manufactory. Sulphur elted in water under a steam-pressure of two to three atmopheres, combines with a certain quantity of water. Larger asses of this melted sulphur (one to two quintals) were poured conveniently deep wooden vessels. In consequence of the frigeration, a crust was soon formed on the surface. In this ust a hole was kept open, through which, as the congelation of e sulphur proceeded, periodical eruptions of melted sulphur, gether with exhalations and explosions of steam, were observed. fter the lapse of an hour and a half, a miniature volcanic cone as formed, with all the characters of a volcano formed by sucessive lava streams. Observations which can be made during is experiment are sufficient to explain and to confirm many cts observed in real volcanoes. If the artificial eruption is terrupted by a second hole made in the crust, the cone becomes ollow, and if this hollow cone is crushed and the eruption again aused by closing the second hole, a model is formed of a ounger volcanic cone which is surrounded by an outer barrier, ike Vesuvius or the Peak of Teneriffe. If the process is conucted to the end without interruption, the result is a massive one with a closed crater, which resembles perfectly the homogeneous dome-volcanoes, as Seebach calls them. These domes, r massive cones, must therefore be considered as the inner nassive nuclei of perfectly extinct volcanoes, the lava, ashes, and tuffs of which have been removed by denudation.

BENGAL

Asiatic Society, September 7.—The Hon. J. B. Phear, President, in the chair. "The Vástu Yága and its bearings upon Tree and Serpent-worship in India," by Babu Pratápachandra Ghosha, B.A. The Vástu Yága and various other forms of Serpent and Tree-worship are traceable as much to a feeling of ear as to other causes. It is evidently a sacrifice, invented by the ancient Aryan conquerors with a view to propitiate the aborigines or primeval owners of the land. Vástu is the principal god, and though the aborigines themselves are not worshipped by name, the Nága is no doubt the ostensible object of worship. The several gods, properly *pitris* (ancestors, predecessors, former owners) that occupy the several *mandalas*, are also the names of Nágas. The Vástu is the God Earth, quite distinct from Dhará (Terra) and in the prayer he is represented as the supporter of the world. The Vástu Yága, therefore, appears to be a memorial of the foundation of the new Aryan home and of the Nágas, a powerful race of aborigines. In the ceremony for dedicating a tank, a stick is planted on its banks. This stick is the *Nága-yashti*, or the Nága-pole. The application of the term Nága to the reptile class is without doubt of comparatively recent date, and since that time may be noted the double meaning of the word applied to the Nága aborigines as well as to the Nága serpents. Ananta is worshipped not as a snake, but as a form of Vishnu. It literally means eternity. The *Anantacha-turdas'i*, *Nágapanchami*, and such other minor vratas, though connected with the Nágas, have nothing to do with the actual reptile. The aborigines of India bore a peculiar relationship to the first Aryan settlers. Many of the aborigines were held in high estimation, and in a legend the goddess Sarasvati is described as imparting the art of music to two of the Nágas (*Kamvala* and *As'vatara*), and the name of Karkotaka, another Nága, is enjoined to be uttered every morning, There are again several fruits, trees, and things which are named after the Nágas, and these are all derived from the N E. frontiers of India. From the above, it would appear that the Nágas as a race of powerful aborigines were respected for their prowess and also hated for their barbarous habits. The eminent among them were soon identified with some Hindu gods, and ultimately the Nágas, as a race, became a class of gods. Serpent-worship, in the true sense of a creature-worship, was never prevalent in India, and though, under peculiar circumstances, this worship may be seen at the present day among the several hill tribes, still such a practice does not obtain among the Aryans. The serpent, as an emblem of eternity, is respected, but it is the worship of Vishnu and not of the reptile. Serpents have crept into our mythological legends, but in whatever form they appear, they are put down as enemies of Vishnu. Ráhu is darkness, and its stellar form is a snake. Sun = Hari = Vishnu, the destroyer of Ráhu, the first destroys as darkness, the second as snake, and the third as death. Figures of Nágas occur in sculptured stones, but only for ornamentation. Several trees are described in later Puránas as forms of Vishnu and other gods, but they are cherished with a degree of care because of their extreme usefulness in the tropical country. For instance, *Tulsi* as an aromatic herb, the *Durva* as a fodder on which the cattle live, the religious fig-tree as offering cool shelter, the cocoa-nut as a refreshing fruit. Some trees again are noted as obnoxious when planted near dwelling houses, because in a Hindu hygienic point of view, they are considered injurious to health. The papaya plant is one of those that no Hindu would like to have near his house.

PHILADELPHIA

Academy of Natural Sciences, Sept. 6.—Mr. Vaux, Vice-president, in the chair.

Sept. 27.—The President, Dr. Ruschenberger, in the chair.—"On the Flowers of *Aralia spinosa* L. and *Hedera Helix*, L." by Thomas Meehan. The study of *Aralia spinosa* L. affords some interesting facts which do not seem to have attracted the attention of other observers. In Dr. Gray's indispensable "Manual of Botany," it is said to be "more or less polygamous." I have had many specimens under my daily observation this season, from the earliest opening till the last blossom appeared, and find that it is much more nearly monœcious than the above quotation would imply. There are three different sets of flowers corresponding to the thrice compounded branchlets of the large panicle. When the flower scape elongates, it seems suddenly arrested at a given point, and a very strong umbel of *female* flowers appears at the apex. A great number of secondary branches appear along this main one; and they also suddenly terminate each with an umbel of female flowers. From these secondary branches a third series appears, and these flowers are well filled with anthers that are abundantly polleniferous. The female organs of these flowers of the third class are, however, defective, as only a few bear capsules, and in these, a large portion of the seeds have no ovules. The polygamous character is confined to this third series of flower, the first two having purely pistillate blossoms. In these there do not seem to be the rudiments of stamens. The most remarkable part of this process of development is, that the whole of this first series of female flowers should open so long before the male ones come, that they fall unfertilised. The greater part of the second series also fall, and the crop of seeds is mainly made up of a few of the last opening ones of the series, and the comparatively few hermaphrodite ones which are found in those of the third class. It is a matter for curious speculation what special benefit it can be to the plant to spend so much force on the production of female flowers too early to mature, and then producing such an immense mass of pollen to go utterly to waste. Examining the flowers of the allied European evergreen ivy, *Hedera Helix* L., I find similar laws of distribution of the sexes as in *Aralia spinosa*, with the addition of a somewhat different structure in the male from the female flowers. In Europe the plant is described as often having a single umbel as a flower spike. It is quite likely in these cases that the flowers are hermaphrodite. In all the cases I have met with here, the inflorescence is a compound of several umbels—a terminal one female, and the lateral ones male, as in *Aralia*. But there are rudiments of stamens in the flower, and in occasional instances I find a filament developed; but never, so far, with any polleniferous anthers. The flowers of the central female umbel have rather longer and stronger pedicels than the lateral male ones. The calyx is united with the ovarium for one-half its length, and the latter much developed in the unopened flower. In the male the segments of the calyx are two-thirds free, and the petals are much longer than in the female flowers. As in *Aralia spinosa*, the male flowers do not open until some time after the female ones; and not before some of the latter, impatient of delay, have fallen unfertilised. I have so often and in so many varied ways demonstrated to the Academy that in plants the male element is a later and inferior creation, that it seems almost supererogatory to point out that these plants illustrate the same principle.

October 4.—Prof. Leidy made the following communications in palæontology. He directed attention to a collection of fossils from Sweet Water River, Wyoming Territory, recently received as part of the results of the geological exploration of Prof. Hayden. The most numerous and characteristic remains are those of a species of *Merycochœrus*, about two-thirds the size of *M. proprius*, from the head-waters of the Niolorara river. The species was named *M. rusticus*. Other remains found in association with the former are referable to a species of *Hipparion*, to *Canis vafer* and *Merycodus necatus*. Two additional fossils, from a tertiary deposit near Fort Bridger, are referable to a small species of *Lophiodon*, which was named *L. modestus*, and a small suilline pachyderm, which was named *Hyopsodus paulus*.

October 18.—Prof. Leidy directed attention to a collection of fossils received from the Smithsonian Institution, from Rev. Thomas Condon, of Dalles City, Oregon. The specimens were obtained from a tertiary deposit in the valley of Bridge Creek, a tributary of John Day's River, Oregon. The greater number and more striking remains belong to a species of *Oreodon* larger than any previously discovered. The skull is about fourteen inches in length, and is intermediate in character to that of *O. major* and *Merycochœrus proprius*. The species was named *Oreodon superbus*. Among other remains of the collection are those of *Oreodon Culbertsoni*, *Agriochœrus antiquus*, *Leptomeryx Evansi*, and *Anchitherium Bairdi*. A fragment of an upper jaw with two true molars probably belongs to *Lophiodon occidentalis*. Other fossils indicate ten species of *Rhinoceros*, probably *R. occidentalis* and *R. hesperius*. Others probably indicate *Elotherium superbum* and *E. ingens*. A small fragment of an upper jaw with a molar tooth apparently indicates a larger species than *Anchitherium Bairdi*, and was referred to a species with the name of *A. Condoni*.

October 25.—Prof. Leidy stated that he had recently received several boxes of fossils collected during Prof. Hayden's expedition in Wyoming Territory. Among the mammalian remains are those of a pachyderm about the size of an ox, and related to the *Chalicotherium* and *Titanotherium*. These were referred to a species with the name of *Palæosyops paludosus*. A fragment of a lower jaw, with true molars like those of the peccary, but with pointed lobes, was referred to a species with the name of *Microsus cuspidatus*. The animal was about the size of a rabbit. The remains of a lower jaw of an ursine animal, about the size of a raccoon, was referred to a species with the name of *Notharctus tenebrosus*.

November 1.—Prof. Leidy exhibited the tooth of a mosasauroid reptile from the miocene tertiary deposit of Gay Head, Martha's Vineyard. From the peculiar minutely-lettered appearance of the enamel, the tooth was referred to a species with t name of *Graphiodon vinearius*. He also referred to a now e tinct species of crocodile, indicated by portions of a skull collect in Prof. Hayden's expedition, from a tertiary deposit of Big San River, Wyoming. The skull, when perfect, measured eighte inches long. It has nearly the form of that of *Crocodilus v garis*. The upper jaw is deeply indented back of the fou tooth, and a pair of deep pits occupy the front of the palate. T species was named *Crocodilus Elliotti*.

BOOKS RECEIVED

ENGLISH.—The Truth of the Bible: B W. Savile (Longmans).—Physi Geography: Mary Somerville, new edition (Murray)—Voyage round World, 2 vols: Marquis de Beauvoir (Murray).—Natural History of Azores: F. C. Godman (Van Voorst).—New Zealand and the South- Islands: Capt. Meade (Murray)—Body and Mind: H Maudsley, M. (Macmillan)—A Manual of Zoology: H. A. Nicholson, M D. (Blackwoo

FOREIGN.—(Through Williams and Norgate)—Plantarum novarum Fas H. Van Heurck.—Synonymia botanica, 1te Halfte: Dr L. Pfeiffer—Le buch der Chemie, 1te Lieferung: Dr. G. F von Gorup-Besanez—Die Be hungen zwischen dem Atomgewichte und der Natur der chemischen Eleme Dr. H. Baumhauer.

DIARY

THURSDAY, DECEMBER 29.

ROYAL INSTITUTION, at 3.—Burning and Unburning: Prof. Odling (juve lectures).

SATURDAY, DECEMBER 31.

ROYAL INSTITUTION, at 3.—Burning and Unburning: Prof. Odling.

MONDAY, JANUARY 2, 1871.

ENTOMOLOGICAL SOCIETY, at 7.

TUESDAY, JANUARY 3.

ROYAL INSTITUTION, at 3—Burning and Unburning: Prof. Odling.
ZOOLOGICAL SOCIETY, at 9—Notes on the breeding-places of *Steator caripensis*: Hon. A. Gordon—Descriptions of thirty-four new species Shells from Australia: Mr. George French Angas.
ANTHROPOLOGICAL SOCIETY, at 8.—The Manx of the Isle of Man: Richard King—The Anthropology of Lancashire; Dr Beddoe, P A.S L.—On Forms of Ancient Interment in Antrim: Dr. Sinclair Hold

THURSDAY, JANUARY 5.

ROYAL INSTITUTION, at 3.—Burning and Unburning: Prof. Odling.

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THURSDAY, JANUARY 5, 1871

PROGRESS OF SCIENCE IN 1870

THE year which has just come to a close has neither been characterised by many new and striking scientific discoveries, nor have any novel applications of Science to ordinary industry and manufacture attracted special attention. The work done has been more a strengthening of that of past years, and a confirming or a disproving of theories and experiments, than the inventing of new ones. In one branch of Science only has any great advance been made, and that, as we shall presently show, we believe to have taken place in Geology. But this advance is one somewhat overlooked at present; but still of so important a character that, when once fully recognised in all its bearings, it may tend to disprove much of the geological teaching of the present day.

Taking the various Sciences as much as possible separately, we will begin with ASTRONOMY. Here attention has been chiefly directed, as has been the case for so many years past, to the Sun. Since it is now generally understood that when once the nature of this vast self-luminous body is accurately made out, much light will be thrown on many now perplexing and strange phenomena, the Eclipse of the 22nd of December last was anxiously watched for, and all possible observations were taken here by those who were unable to take part in the Government Expedition to Spain and Sicily. It is to be hoped that the labours of this Expedition, in spite of accident both on land and sea, and the unsatisfactory state of the weather at the time of observation, will yet yield results of great importance. At any rate we may fairly congratulate ourselves that at last we have a Government which has shown itself in other instances besides this special one, not unmindful of the claims of Science and of the value of accurate scientific investigation.

Mr. Lockyer and Mr. Huggins have continued their spectroscopic observations of the Sun, and Prof. Zollner has published a very valuable paper on the solar prominences, theorising very boldly as to the temperature and pressure at the Sun's surface;* while in America Prof. Young has worked with good results at the same subject. Before leaving this branch of our subject, we would mention that Mr. Proctor has published some novel views as to the constitution of the stellar systems, which, under the somewhat fanciful titles of "star-drift" and "star-mist" must be familiar to most of our readers.

Whilst the vast domain of ORGANIC CHEMISTRY has been still further widened by the innumerable workers who plunge into this branch of the subject and neglect the many untrodden paths in Inorganic Chemistry, nevertheless no special or important discoveries are to be chronicled, unless we may mention the beautiful process by which Indigo has been synthetically constructed by M.M. Emmerling and Engler, following closely on the artificial manufacture of Alizarine by M.M. Liebermann and Graebe.

MOLECULAR PHYSICS has occupied a large share of attention, and the discussion before the Chemical Society on the existence, or non-existence, of Atoms and Molecules, has only too clearly shown how doctors differ amongst themselves, and that the very foundations of a Science, considered so essential by some, are utterly repudiated by others. A very remarkable paper on the Size of Atoms, originally published in these columns (NATURE, vol i. p. 551) by Sir William Thomson, in which he gives four distinct trains of reasoning by which he arrives at a proof of their absolute magnitude, has attracted much attention, and has been translated and copied into most of the continental and American scientific journals. Dr. Thomas Andrews has also pursued his remarkable investigations on the Continuity of the liquid and gaseous states of matter. The death of Prof. Wm. Allen Miller, F.R.S., and Dr. Matthiessen, F.R.S., have left sad voids in the ranks of our English experimental chemists.

In BIOLOGY, the investigations of Prof. Tyndall, "On Atmospheric Germs, and the Germ Theory of Disease,"* have contributed to a clearer knowledge of the nature of some of the most virulent of our infectious diseases, and have caused those diseases to be studied in a much more scientific manner than before.

The theory of Spontaneous Generation, which has been very prominently before the scientific world for the last ten years, has, during the past year, been very strongly attacked on the one hand by Prof. Huxley, and defended on the other by Dr. Bastian and Dr. Child. In his Inaugural Address to the British Association meeting at Liverpool, Prof. Huxley gave a long review of all the researches on the subject, from the time of Spallanzani and Needham to the present day, and declared his belief, after carefully weighing the evidence on both sides, that all life has its origin in some pre-existing life, and that Spontaneous Generation, or, as he termed it, Abiogenesis, is not now proved to take place. The investigations of Dr. Bastian, originally intended to have been read before the Royal Society, were published instead in these columns, in a series of three long articles (NATURE, vol. ii. pp. 170, 193, 219), in which he gave the reasons for his belief that Spontaneous Generation certainly does occur. Feeling himself attacked and his experiments somewhat underrated by Prof. Huxley in his Address, he criticised it at considerable length, and detailed the results of some new experiments (NATURE, vol. ii. pp. 410, 431, and 492) which confirmed his previous deductions.

The Darwinian theory of Natural Selection has been attacked by Mr. A. W. Bennett and Mr. Murray,† and defended by Mr. A. R. Wallace and others; Mr. Wallace having also vindicated his claims to priority in this question, since he published many of the now-recognised theories and speculations on the subject of Natural Selection, at a time when he was resident in the East Indies, and entirely unacquainted with what Mr. Darwin had written on the same subject.

As respects GEOLOGY, during the past year the Government has continued its grants of money for the purpose of Deep Sea Dredgings, and at present the report of the most recent Expedition is anxiously looked forward to. The results of the Expedition in the autumn of 1869, as given to the public by Dr. Carpenter, Prof. Wyville Thomson, and Mr. Gwyn Jeffreys during the past year, have been of the greatest possible interest and importance. They found that on the same level, at the bottom of the deep sea, two different deposits are in process of formation side by side, each characterised by a distinct Fauna, and yet apparently produced under perfectly similar conditions of land and sea, area, depth of water, &c. On investigating this curious result, however, it was found that the temperature of the water circulating over these two areas is very different, and that this mere difference of temperature is capable of entirely changing the character of the fauna of the simultaneously formed deposits. Thus an entirely new element is brought into geological speculations, since it is shown that at one and the same time strata may be accumulated containing widely different organic remains. In addition to this, they have shown that the calcareous deposit known to us as chalk is now being deposited all over the bed of the Atlantic Ocean, and there are many weighty reasons for believing that this deposit has gone on steadily ever since the time during which we imagined the cretaceous rocks of the world to have begun and ended. Many organisms formerly supposed entirely extinct have been re-discovered in these deep-sea dredgings; and, in short, much has been done to show that our past geological reasoning requires thorough and careful revision. Prof. Gumbel's discovery of the existence of Bathybius and similar organisms at all depths, and stretching over an indefinite period of geological time, is of the greatest importance in relation to this subject. Prof. Agassiz, on the other side of the Atlantic, has published reports of the deep-sea dredging off the Florida Coast, and has stated that the results of his researches, and those of others, both English and Scandinavian, have convinced him that there is life all over the sea bottom, and that where evidence of marine life cannot be found, we are justified in calling in the agency of the sea to explain certain obscure facts. These conclusions cannot be without their important bearing on many commonly received geological theories.[*]

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* Translation in full, NATURE, vol. ii. p 522-526

* See NATURE, vol. i. pp 327, 351, 499, &c.

† NATURE, Vol iii. pp. 30, 49, 65, and 154.

In BOTANY many very careful series of observations have been made in the physiological department. Among the most important we may mention those of Prillieux and Duchartre in France, confirmed by Dr. M'Nab in this country, that, contrary to the previously accepted hypothesis, plants do not absorb any appreciable amount of aqueous vapour through their leaves; and those previously announced by M. Dehérain, that the evaporation of water from the leaves of plants is due to sunlight rather than to heat, and proceeds independently of the degree of saturation of the atmosphere. Much attention has also been paid in Germany, Italy, and England, to the fertile field of the phenomena of fertilisation, opened out by Mr. Darwin's observations.

In METEOROLOGY there is no great advance to chronicle. It still remains a Science without a head, a chaotic mass of facts with no definite order or arrangement; for though many are working at this subject, and some valuable papers on the Origin of Winds and Storms have been published, still no definite progress can be ascertained.

The splendid appearances of the Aurora Borealis, visible all over the British Isles in September and October, have directed public attention to those unmistakeably magnetic phenomena, and to the connection which exists between their appearance, great magnetical perturbations, and large solar spots. They have been examined very frequently during the past year by means of the spectroscope, and there is distinct evidence of lines in the green and red portion of the spectrum, the latter presumably due to hydrogen. We would direct attention to our desire to publish a complete tabular list of the more remarkable meteorological phenomena of the past year, so as to be serviceable to observers in all parts of the world. To render this as perfect as possible, we would invite the kind co-operation of all those interested in the subject who can forward us any data.

We cannot conclude without noticing how much Science has lost during the latter half of the year just ended by the fearful struggle that has taken place between France and Germany, where each nation has brought into requisition all the resources of Science only to inflict as much injury as possible on the other. For nearly six months we have witnessed the sad sight of workshops shut up, laboratories closed, universities and public schools wanting both professors and students, and the friendly emulation of similar tastes and pursuits turned to the fierce rivalry of the sword. Science will have to deplore the untimely loss of many of her most attached workers, and their country will have lost those who would in happier times have done her as much honour at home as they have shown bravery in the field. Whilst the French Academy, shut up in besieged Paris, has brought the art of ballooning to its present state of perfection, so that now it is used as a means of communication with the outside world, the result of the subtle strategy of the Germans, and the scientific education they so generally possess, has been to give them advantages which have, to the present time, baffled their adversaries.

J. P. E.

THE INTELLIGENCE AND PERFECTIBILITY OF ANIMALS

The Intelligence and Perfectibility of Animals from a Philosophic Point of View. With a few Letters on Man. By Charles Georges Leroy, partly under the pseudonym of "The Naturalist of Nuremberg." (London: Chapman and Hall, 1870.)

THESE Essays, written nearly a century ago, seem to have been intended chiefly as an answer to the doctrines of those French philosophers who maintained that animals were merely animated machines, or, as it was expressed by Buffon, that "the animal is a purely material being, which neither thinks nor reflects, but which nevertheless acts," and that "the determining principle of the animal's actions proceeds from a purely mechanical influence, absolutely dependent upon its organisation." Our author, on the contrary, maintains that the mental faculties of animals are strictly comparable with those of man; that they remember, combine, and reflect; that they are capable of self-improvement; and even that they possess a true language fully adapted to their needs. To support his views he gives what we may term a generalised life history of several animals, such as the wolf, fox, stag, fallow-deer, and roebuck, which his position of Ranger of Versailles and Marly gave him ample opportunities of studying. The chief fault of these interesting sketches is, that they detail hardly any of the special observation on which the generalised statements are founded. We are, therefore, unable to tell how much is fact and how much inference; and, what is probably the result of careful life-long observation fails to produce that effect of reality which a more direct narrative style would have given to it. In a few cases, however, he gives us actual observations; as when he proves that animals can count, by stating the fact that in order to destroy crows, which were destructive to game, a hut was made at the foot of a tree where there was a nest, in order to shoot the old birds when they returned to their young. It was found, however, that after the first time the man was always watched into the hut, and the crows would not return till he had left it or till night. To deceive them two men went to the door of the watch-house, one entering and the other passing on, but the crows would not come. The next day three went and two passed on, but still with no effect; and it was not till five or six went and all but one passed on, that they were deceived, being unable to count so many.

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[*] During the past year all the most important papers on Deep-Sea Dredging have appeared in these columns, and we would refer our readers to Vol. i. pp. 135, 166, 267, 611, 657; Vol. ii. pp. 257, 513 &c.

M. Leroy appears to reject altogether what is commonly termed Instinct, maintaining that the word should be applied only to those acts which are the direct consequences of organisation, such as the grazing of the stag, or the flesh-eating of the fox; but not to the expedients to which those animals resort in the gratification of their natural wants, which are due to sensation, observation, memory, and experience. To the objection that many animals perform complex operations perfectly well without experience, and always in the same manner, he replies that in many cases the fact is not so. He maintains, for instance, that there is a distinctly perceptible inferiority in the nests made by young birds, thus anticipating the observation of the American Wilson; and further remarks that the best constructed nests are formed by birds whose young remain a long time in them, and thus have more opportunity of seeing how they are made. He says that the nests of young birds are ill-made and badly situated; and that the defects of these first constructions are remedied in time, when their builders have been instructed by their sense of the inconveniences they have endured. He maintains that nests of the same species of bird differ as much as human dwellings, and that of a hundred swallows' nests no two are exactly alike; and he imputes to want of long-continued observation our failure to discover improvement in them; a want which curiously enough, has been remedied by M. Pouchet, who has found a decided improvement in the nests of swallows at Rouen during his own lifetime. Our author has also some excellent remarks on hereditary habit, as strikingly shown in the case of many of our sporting dogs, and which, he believes, in wild animals is often mistaken for instinct; and he concludes that "It is possible that the actions which we see performed by some animals, independently of the teachings of experience, are the fruit of a knowledge of very ancient date, and that in former times a thousand trials, attended with more or less success, have finally led to the attainment of the degree of perfection which we see manifested in some of their works at the present day."

The migrations of birds, also, he maintains are the result of no blind instinct, but of instruction handed down from generation to generation. He says, "Let us take the swallows as an example which every one can observe. In the first place, their departure is always preceded by assemblages, the frequency and duration of which can leave no doubt that their object is to effect all the necessary preparations for a voyage undertaken by creatures who have the faculty of sensibility, and of understanding one another, and who are united for a common purpose. The incessant and varied twittering which reigns in these assemblies, clearly indicates communications and orders, indispensable for the numerous offspring of the year. They must stand in need of preliminary instruction, constantly repeated, to prepare them for the great event. Frequent trials of flight are no less indispensable, and are often followed by a repetition of previous lessons, which makes our roofs and chimneys ring again. Assemblies of men who should speak a foreign language could not give more evident signs of a similar project. But there is a more convincing proof than this analogy that these migrations are not the result of a blind and mechanical inclination. When, at the time fixed upon for the flight, which cannot, owing to weather, be retarded without compromising the welfare of the whole species, some, and even a large number of indviduals, are too young to follow the rest, they are left behind and remain in the country. But it is in vain that they reach maturity; the supposed *attraction towards a certain region* does not affect them, or too slightly to enable them to gratify it. They perish, the victims of their ignorance, and of the tardy birth which made them unable to follow their parents."

The letters on Man, which are curiously mixed up with those on animals, are neither so interesting nor so well reasoned. Their object is mainly to deduce the complex phenomena of human existence from the two principles of "the love of ease" and "*ennui*," which being antagonistic, lead men to all kinds of expedients to secure the one or escape from the other. These, with sympathy, which he considers the pre-eminently human emotion, are made to explain most of the facts of man's mental nature. The work is written throughout in a pleasing and simple style, and exhibits to us a loving student of nature who observed and thought for himself, and who, in many of his conceptions, was far in advance of the great philosophers of the last century, among whom he lived.

ALFRED R. WALLACE

OUR BOOK SHELF

Use and Limit of the Imagination in Science. By Prof. Tyndall. (London: Longmans and Co.)

THIS is a second edition of Dr. Tyndall's Discourse at the meeting of the British Association at Liverpool. To it is now appended his Address as president of Section A of the British Association at the Norwich meeting. This was analysed by Dr. Clark Maxwell at a later meeting (see NATURE, Liverpool Meeting, Address to Section A). There is also added a short Essay from the *Saturday Review*, with the quaint title of "Earlier Thoughts," suggesting the irrepressible "Country Parson," A.K.H.B. Another curious addition is a selection of favourable, unfavourable, and often ridiculous critiques of various parts of his discourse; which reveal the existence of a strange state of things in the arcana of editorial dens.

One or two trenchant notes are appended to the Discourse. That regarding "microscopists" and spontaneous germ-formation is especially deserved and well laid on This race of pseudo-experimenters, who do not know the simplest necessities of accurate experiment, but who by mere assertion endeavour to bear down genuine scientific men, is really the class which does mischief alike to science and to the cause of religion. Dr. Tyndall is certainly not fairly censured by the so-called "religious" press, but does it ever deal either praise or censure fairly? His remarks on the Materialists at the end of the Address are quite conclusive as to the absurdity and injustice of calling him by that name. There is a verse in Scripture that would suit the case exactly, but we leave the problem as a puzzle to the Editor of the *Record*.

The Student's Manual of Comparative Anatomy and Guide to Dissection. By G Herbert Morrell, M.A., B.C.L. (Oxford: Shrimpton.)

WE have received the only part as yet published of this treatise, viz, that relating to the Birds. Mr. Morrell gives with great care, in a tabular form when possible, a condensation of all the information to be obtained in such works as Huxley's "Lectures on the Skull," and "Classification of Birds," Wagner's, Siebold's and other manuals, and the "Cyclopædia of Anatomy and Physiology," supplementing this by extracts from his own notes of dissections in the anatomical laboratories of the Oxford University Museum, and of Professor Rolleston's lectures given there. It is proposed to issue an atlas of woodcuts borrowed from various works to illustrate the letterpress. The book will be found very convenient by students at Oxford and elsewhere, who are carefully studying the comparative anatomy of the Vertebrata. We must decidedly object, however, to the omission of one group of organs *entirely*—the reproductive. It is a concession to a strange prejudice, and really renders a good work incomplete.

E. R. L.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Eclipse Expedition

I WRITE you a brief account of the doings of the English Eclipse Expedition up to the present time, thinking it may be interesting to your readers.

After leaving London we proceeded direct to Naples, staying on the road only a few hours at Cologne, half a day at Verona, and a day at Rome. Everywhere on the journey the most marked kindness was shown to the Expedition as the representatives of English Science. From Ostend to Verona we never changed carriages, and there only on account of a slight breakage in the carriage itself. Never once were we asked for passports, never once were our instrument cases overhauled, or anything beyond mere personal baggage, at the custom houses At Naples we stayed a day, which was occupied in taking the lids off all the cases, repairing one or two slight accidents which had befallen the instruments, and making plans for our distribution in Sicily.

At four o'clock in the afternoon of Wednesday, the 13th, we were all on board the *Psyche*, a despatch boat which had been lent by the Government to convey ourselves and instruments to Sicily. A delicious sunny afternoon, a sea as smooth as molten glass, a ship's company receiving us with the utmost kindness and hospitality, how could the evening not pass as merry as a marriage bell? I cannot retail to you all the jokes which passed, the lively chats and quiet strolls by moonlight, the polariscopes and spectroscopes pointed to the sea and sky, ere long destined to address their momentous questions to the Sun himself, now having their merits and demerits freely discussed by the *savants*; but you can imagine it all At last we turned in to enjoy that repose which sea air always induces in landsmen.

At about six we were all called and told we were approaching the Straits of Messina, the very Scylla and Charybdis of our classical lore. In moonlight, soft, cool, and delicious, we beheld those rocks of Scylla and steered through Charybdis, though we felt it not, and so onwards in the rising sun through the Straits. After breakfast we held our (as we then thought) final council only about an hour before we were to separate for our various Sicilian stations. Scarcely had the council broken up, when the sad event occurred which was destined to alter all our plans. We were steaming along about ten miles north of Catania, the gigantic cone of Etna, capped with snow, appearing to rise from the cliffs of lava under which we were passing, when the ship struck on a treacherous sunken rock, shown in none of the charts. I cannot pass over this sad catastrophe without referring to the noble behaviour of our gallant commander, Lieut. Fellowes, to whose coolness and energy we owe the safe disposal of ourselves, instruments, and baggage at Catania, without any serious loss. Mr Lockyer, at the request of the captain, immediately telegraphed to Malta, whence the *Royal Oak* came to the aid of the *Psyche*. More lately the admiral of the Mediterranean fleet (Sir Hastings Yelverton) has arrived in the *Lord Warden*, and anchored beside her, and I am glad to be able to say that if the present calm weather continues there is every prospect that the ship may be saved

For this expedition it was found necessary to make Catania the head quarters instead of Syracuse, as had been originally intended, as there would inevitably have been too great delay in removing the heavier instruments to Syracuse to carry out the original plan. Since our arrival, we have received all possible help from our own and the Italian Governments. From the latter we obtained the use of the garden of the Benedictine monastery, where we are working, as well as guards at some of our stations. Both here and elsewhere the American and Italian observers join heartily with us, and to their co-operation we owe much.

The arrangement of the English parties are now as follows. At Catania we are strong in the spectroscope. Mr. Lockyer and Mrs Lockyer are working with a large reflecting mirror fitted with a single prism, but with special adaptation for giving plentiful illumination, as well as for placing the hydrogen spectrum side by side with the spectrum obtained from the corona Mr. Seabrokes has a large refractor fitted with a spectroscope of six prisms for examining the chromosphere. Mr Pedler works with a small direct-vision instrument. Prof. Thorpe has mounted his apparatus on the top of a portico in the garden, and has been engaged the last few days in making out daily curves of the chemical intensity. To Mr. Vignolles, sen, and Mr. Vignolles, jun, and myself are committed the time and general observations In addition to the above, we hope to have the assistance of several of the officers from ships in the neighbourhood, who will make sketches of the appearances, and help the observers in various ways.

Our next detachment, under the charge of Prof Roscoe, has left us, intending to find a stage of observation as high as possible on Etna. It is confidently expected that, by leaving some seven or eight thousand feet of the densest atmosphere behind, delicate but important observations may be made which would be impossible at a lower level. This party is piloted by Prof. Sylvestre, of the University of Catania, who, in common with all the authorities here, has shown the utmost courtesy toward the Expedition. The spectroscope observations will be conducted by Prof. Roscoe, with the assistance of Mr. Bowen; the polariscope is under the charge of Mr. Harris; photographic arrangements under Dr. Vogel; while Mr. Darwin will sketch the appearances presented.

At Agosta we are strongest in the polariscope, this instrument requiring the longest possible duration of totality. The party is under the charge of Prof. Adams (of King's College, London), who, in the polariscope, is assisted by Messrs Ranyard and Clifford. The only spectrocopist stationed here is Mr. Burton, and the sketcher, Mr. Brett. This party have been living under canvas for the last few days, and, I need hardly add, received the utmost kindness from a military detachment stationed there with them.

At Syracuse we are only represented by one photographer, Mr. Brothers, assisted by Mr. Fryer and by Mr. Griffith, who will take observations with his polarimeter.

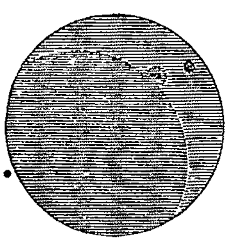
The authorities at Malta sent us here two sappers, thanks to whose exertions our observations have been erected with great rapidity, enabling us to station our instruments and make preliminary trials in the very positions they will occupy during the Eclipse. Since our arrival these preliminaries have kept us all extremely busy, and the ultimate success of our operations must in a great measure be attributed to the unremitting energy of Mr. Lockyer and Prof. Roscoe

Up till to-day the weather has been superb, day after day just like the warm days we often have in England towards the end of June. The thermometer in the shade has reached from 75° to 80° F., while the barometer has been steady, but with just sufficient tendency downward, to fill with gloomy apprehension the less sanguine of our party. During this afternoon things do not look well, heavy clouds have been sailing over head, and have quite shrouded the upper five thousand feet of Etna; but we have yet hope, and all we can do is to wait patiently for about twenty hours, hoping then to get at least a bright gleam for the space of a minute and a half. If the sky so far favour us doubtless to-morrow will be an epoch in the history of astronomy.

L. CUMMING

Catania, Sicily, Dec. 21, 1870

The Eclipse



A REMARKABLE phase in the moon's passage across the sun was the perfect apparent contact of the limb of the moon with a sun-spot, of which the annexed figure is a fair representation. The noticeable thing was that the body of the moon itself and the sun spots were of so precisely the same tint that no trace of a division was perceptible, one appearing to be merged in the other as long as the contact lasted.

Exeter, December 22, 1870 W. F.

Eozoön Canadense

THE letter of Mr. T. Mellard Reade on the subject of *Eozoon Canadense*, contained in your number for December 22, exhibits so complete a misapprehension of the state of our knowledge of that fossil, that I feel it necessary to break the silence which I have for some time imposed upon myself as regards this subject, in order that your readers may not be misled by the positiveness of his assertions.

1. Mr. Reade speaks of Eozoön, with the exception of the Tudor specimen, as having been obtained *only from metamorphosed rocks*. In reply to this, I have to state that the Eozoonal structure is *most* characteristically displayed in those portions of the Serpentine Limestone of the Laurentian formation which have undergone the *least* metamorphic change. In fact, the Calcareous lamellæ of the best specimens of Eozoön in my possession show less departure from the shelly texture with which I have become familiarised by the special study of the microscopic appearances of Shell, &c., for more than thirty years, than do the great majority of undoubted shells, corals, &c., contained in the least altered rocks of any geological period.

2. Mr. Reade assumes that the presence of the Serpentine lamellæ, which alternate with the Calcareous lamellæ, is itself an indication of metamorphic action. This position can only be sustained by those who are ignorant of the processes which can be shown to be at present going on upon the sea-bottom, and of which we have evidence in various geological periods; whereby the sarcodic substance of animals of various types of organisation, but especially of *Foraminifera*, undergoes replacement by siliceous compounds precipitated from sea-water during its decomposition. It was long since shown by Prof. Ehrenberg, that green sands of various ages, from the Silurian to the Cretaceous, are essentially formed of the *internal casts* of Foraminifera. The late Prof. Bailey (U.S.) first proved that the production of such internal casts is taking place at the present time. I have long had in my possession a set of beautiful internal casts of this kind, procured from the late Mr. J. Beete Jukes's dredgings on the coast of Australia. And quite recently I have obtained from Captain Spratt's dredgings in the Ægean a most remarkable series of such casts, which includes representations in green and ochreous Silicates, not merely of the sarcode-bodies of *Foraminifera*, but also of the sarcodic network that occupies the interspaces of the calcareous reticulation which I demonstrated twenty-three years ago (Brit. Assoc. Report for 1847) to be the basis of the skeleton throughout the class of *Echinodermata*. And Dr. Duncan has shown that a like process is taking place at the present time in the case of *Corals*; their animal substance being replaced by Silicates, whilst their Calcareous skeleton remains unchanged. No *mechanical* agency can account for this replacement. It is not effected by the percolation of Silicates in solution, under the "*hydrothermal*" action which Mr. Reade (following the lead of Messrs. King and Rowney) invokes as having been concerned in the production of the Canadian Eozoon. And I am justified by the opinion of several of our ablest Chemists and Mineralogists in the assertion that no agency save a progressive *chemical substitution* can account for the production of these wonderful models; the Silicates being precipitated from sea-water by the decomposition of the sarcodic substance which they replace and represent. Whether or not this doctrine be accepted, it may be confidently affirmed that whatever be the agency concerned in *their* production, the filling-up of the cavities of the Calcareous skeleton of Eozoon may be fairly accounted for in the same manner.

3. The most characteristic features of the best-preserved specimens of the Canadian Eozoon can thus be completely paralleled by those of analogous formations at present going on. Let us suppose that the North Atlantic sea-bed, instead of being covered by minute individualised *Globigerinæ*, were occupied by a shell-producing Rhizopod having the indefinite extension of *Bathybius*, and that its sarcodic substance came to be replaced (as in the instances just cited) by Silicates precipitated from sea-water; such a composite formation, elevated so as to become a terrestrial rock, *without any metamorphism whatever*, would be the precise parallel of the *Eozoon Canadense*. And just as, at the present time, the replacing minerals are not always the same, though always compounds of Silica, so the substituted material in Eozoön often consists of other minerals than Serpentine, always, however, being Silicates. In fact it was the *uniformity* of Morphological character, with *variety* of Mineral composition, that first led Sir William Logan—a geologist second to none in experience and judgment—to the suspicion of its organic origin.

4. Mr. Reade represents me as having made "the important admission" that "the several features in the structure of Eozoön (chamber-casts, canal-system, and proper walls) could be separately paralleled elsewhere," meaning, I presume, in undoubtedly Mineral structures. I have nowhere, that I can recollect, made any such admission: on the contrary, I have repeatedly argued that whilst the *combination* of structural characters in Eozoon affords the most unmistakeable evidence to those whose previously acquired knowledge enables them to appreciate their value, there are individual features which are inconsistent with any conceivable hypothesis of its purely Mineral character. Of these I may here state two, which I have always found to be most convincing to such as are familiar with the microscopic appearances of Minerals: *First*, the fact that the "canal-system" which traverses the Calcareous lamellæ passes *across* their cleavage-planes, instead of *between* them; and that this canal-system has precisely the same distribution, whatever may be the mineral which occupies its tubes, whilst its finest ramifications are frequently filled with calcite, as in the least-altered fossil Foraminifera. The idea that such arborescent expansions can have been produced by any kind of *infiltration* of one mineral into another, is thus, in the judgment of some of the most eminent Mineralogists of the day, *altogether untenable*; whilst the *precise parallelism* pointed out by Dr. Dawson, between the canal-system of Eozoon and that which I had shown to exist in the recent *Calcarina*, is no less satisfactory to Naturalists conversant with Foraminiferal structure. *Second*, the fact that the "Nummuline layer" or "proper wall" of the chambers consists of a Calcareous lamella traversed by Siliceous aciculi, which sometimes lie straight and parallel, are sometimes curved, and sometimes penicillate; the precise equivalent to this being shown in the chamber-walls of recent Foraminifera, when the pseudopodia which occupied their tubuli during life have been replaced by Silicates. I assert again, on the authority of Mineralogists of the highest eminence, that such an arrangement cannot be shown in any undoubted mineral, and that it cannot be attributed to any physical agency. To liken this "Nummuline layer" to Chrysotile or any similar modification of Serpentine, shows a misapprehension of its essentially composite structure.

5. I cannot admit that the question of the Organic nature of the Canadian Eozoon (if question it be) is in the least degree affected by the occurrence of Metamorphic rocks presenting more or less morphological resemblance to it, in combination with undoubtedly Mineral characters. We should never think of deci-

ding the nature of Chalk, or of any more ancient Foraminiferal limestone, by the condition of its altered forms; the evidence of their Organic origin being supplied by the microscopic examination of specimens exhibiting the least evidence of change, and this evidence being not in any degree invalidated by the most complete mineralisation of particular examples. A large number of specimens of Ophicalcite have been submitted to me from various sources, as to some of which I have been able to say pretty confidently that they were originally Eozoic, but have been altered by subsequent metamorphism; whilst others do not present any feature whatever which would lead me to assign to them an Eozoic origin. To assume that these last (of which the Strath rock may be an example) are to be placed in the same category with the Canadian Eozoön, and thence to affirm that because they are purely Mineral productions, *it* cannot be Organic, involves a *petitio principii* by which it would be perfectly easy to prove the same thing of Chalk.—Let me illustrate my position by a parallel case. I have lately demonstrated* the existence of a Foraminiferal structure *departing much more widely than Eozoon does from any previously known type*, in a class of globular bodies from one to two and a half inches in diameter, occurring in the Upper Greensand; these having been previously regarded by experienced Geologists as mere Mineral concretions. Now, it so happens that the Magnesian Limestone of the North of England contains large masses of spherical concretionary-bodies, bearing a strong *general* resemblance to *Parkeria* in internal structure as well as in outward form, but hitherto regarded, I believe universally, as Inorganic; and a reasoner like Mr. Reade would argue in this way:—"Because impartial geologists have pronounced the Permian concretions undoubtedly inorganic, the Greensand spheres are so likewise; and Dr. Carpenter's *Parkeria* becomes extinct as a fossil." But it likewise happens that the structural features which are most peculiar in *Parkeria* present themselves also in a remarkable living Foraminifer recently obtained from great depths in the *Porcupine* dredgings; so that the truly Foraminiferal nature of *Parkeria* cannot be a matter of the slightest doubt. And the only question now is, whether a careful microscopic examination of the minute structure of the Permian concretions may not afford, through its likeness to that of *Parkeria*, more or less definite indications of *their* Organic origin, obscured by subsequent metamorphism. The application is obvious.

6. I am equally unable to admit that if a rock presenting all the characteristic features of the Canadian Eozoon were to be found shading off into one containing characteristic Liassic fossils, this would afford the least tittle of evidence against the Organic character of the former. As the *Lingula* of the ancient Siluria has, in the judgment of our most eminent Brachiopodist, come down unchanged to the present time, and as even the same *varietal* modifications of Foraminiferal types were existing in the Triassic period as now in the Mediterranean, I see no reason why we should limit Eozoon to the Laurentian epoch. When this subject was last discussed at the Geological Society, I ventured to say that it would not in the least surprise me to find Eozoön, or something very like it, now existing on the deep-sea-bottom; and the notion was not treated by any of the eminent Geologists then present as having any *a priori* improbability. Since that time, the Coccoliths first discovered by Prof. Huxley, and the Coccospheres first observed by Dr. Wallich, in the Globigerina-mud, and afterwards recognised by Mr. Sorby in Chalk, have been detected by Prof. Gümbel in Silurian rocks; so that it is clear that the Biological condition of the deep sea has changed much less in vast periods of Geological time, than has that of shallower waters; whilst the probability has now almost reached a certainty that Rhizopodic life has been at least as largely concerned in the production of Calcareous deposits in earlier Geological periods, as we know it to have been in the later.

7. Though Mr. Reade "feels assured that whenever impartial Geologists take the question up, the fossil itself will become extinct," his assurance is not borne out by the judgment of the large number of impartial Zoolgists, Continental as well as British, who have satisfied themselves, by a careful examination of my series of microscopic preparations, of the Organic nature of the Canadian Eozoon, and have authorised me to express their entire accordance in my interpretation of its phenomena. An eminent Professor in one of our own Universities used this emphatic expression—"The matter seems to me not to admit of *hesitation*, much less of *doubt*." My last Continental visitor, Prof. Carus, who is well known to possess a comprehensive and practical knowledge alike of Zoology, Palæontology, and Mineralogy—assured me that having come without any prepossession on the subject, he left me with a full conviction of the justice of my views. The respect paid by such Naturalists as Professors Milne-Edwards, Carus, Lovén, Van Beneden, and Escher von der Linth,—typical representatives of the Science of France, Germany, Sweden, Belgium, and Switzerland,—to my own judgment in a matter as to which *they* regard the special studies of a third of a century as giving me some claim to authority, may console me for the contemptuous repudiation of microscopic evidence in which Mr. T Mellard Reade has thought it becoming to indulge. I am far from expecting, however, that anyone should pin his faith upon my own *ipse dixit*, supported though it be by the entire concurrence of my three fellow labourers in Foraminiferal investigation, Messrs. Parker, Rupert Jones, and H. B. Brady. And if it be thought that the decision of any tribunal of really "impartial geologists" is likely to carry more weight with the scientific public than that of the authorities I have cited, I am perfectly willing to go into the question with them; provided, however, that such tribunal consists of, or at any rate includes, men who are sufficiently conversant with the Microscopic appearances of undoubtedly Organic structures, to be able to recognise such appearances when they see them. One of the strongest opponents of the Organic origin of Eozoon designated as "an agatized mineral" a section of a *recent* Nummuline shell; that exhibited a minute tubulation corresponding with that of the nummuline layer of Eozoon, which he had just before characterised in the same manner. Another attributed the production of a perfectly mineralised internal cast of *Polystomella* in green silicate, from Capt. Spratt's Ægean dredgings, to the working-in of mud. And a third has abstained from even looking at my specimens, though I have repeatedly expressed my willingness to give him an opportunity of examining them. Such are *not* the judges before whom I would consent to plead the cause of *Eozoon*.

WILLIAM B. CARPENTER

Mimicry *versus* Hybridisation

ALLOW me space for a word or two in reply to Mr. Wallace and Mr. Butler's observations on my papers on Mimicry and Hybridisation.

There is only one point in my argument to which they have taken exception, and although, of course, I am not therefore entitled to assume that their silence on other points means assent, I may at least infer that in their view the point objected to is most open to assault, and that if it were established, the reader may regard the rest with increased confidence.

The objection is that the instances of hybridisation in plants which I have cited as parallel to the cases of mimicry between the Danaids and Nymphalids were merely cases of hybridisation between species of the same genus or allied genera, whereas these butterflies are more distantly related. The question, as thus put by these gentlemen, resolves itself into a question of comparative degrees of affinity, and Mr. Wallace, with his usual skill, tries to throw the onus of proof from his shoulders to mine. But with all submission we shall keep it where it naturally lies. He puts it that my argument rests on the assumption that hybridisation can take place between different orders or families, and quite logically (supposing me to have done so) objects to my making any such assumption in regard to insects, seeing that nothing of the kind has ever been observed in other animals or in plants. But I rest my argument on no such assumption. I ask no other measure for insects than is given to plants. It is Mr. Wallace who makes the assumption that the amount of difference between Lepidoptera has a different value from that attached to it in any other organic beings. It is he who claims for differences which in any other creatures would be regarded as no more than specific the importance of generic or ordinal. But however this may suit the artificial classification of the systematist, we cannot allow it when we come to deal with the actual workings of nature.

I am not surprised that either Mr. Wallace or Mr. Butler should take what appears to me an exaggerated view of the dignity and position of their favourite group. It is human nature that any subject to the study of which we have devoted ourselves should assume in our eyes larger proportions than it does in the eyes of those who take a wider but less detailed view of it. Hence we see Mr. Butler comparing the Lepidoptera to birds, as if it were a kingdom of equal magnitude, and seeking for equivalents for such groups as the hawks and doves within its limits. Whereas it seems to me that the truer parallel is between the whole class Insecta and Birds, and that the equivalent groups for hawks, doves, &c., are to be looked for, not in one of the sections, but in the whole of the class. He looks for both hawks and doves in the *Lepidoptera*. I find nothing but doves. If you want hawks you must go to the dragon-flies, which are their equivalent; and, of course, if we are only dealing with doves, there is nothing in the known phenomena of hybridisation opposed to such a cross having taken place.

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* See my description of *Parkeria* in the Phil. Trans. for 1869

It is impossible in the brief space that you would allow me, even to glance at the many arguments that I could adduce to show that this is the true position of the *Lepidoptera*. I hope to do so elsewhere. But I would only remind entomologists, especially lepidopterists, of the trifling characters on which their genera have been established, and how difficult it has been to find any generic characters at all. This is frankly acknowledged as the great difficulty attending the study of *Lepidoptera*, consequently characters which would never for a moment be looked on as generic in any other group of animals, are there allowed that value. If any specialist in another group objects, what is the answer? "We have no better characters, and we must do the best we can with the slight ones we possess." Quite right, in a systematic point of view. If the species of doves came to be reckoned by thousands, the ornithologist would just have to do the same thing; but that would not alter the position of doves in the animal kingdom—they would still bear the same relation that they do now to hawks, and be equally open to hybridisation among themselves, indeed, more so; for such great numbers of one type would be a presumption in favour of every mode by which species could be increased having been resorted to; and this by the way is an additional indirect argument in favour of hybridisation sometimes taking place among *Lepidoptera*.

Of course, I do not mean to say that there is nothing more than specific distinction between the Danaids and Nymphalids. I recognise them as good genera, but only as genera sufficiently nearly akin to allow of hybridisation taking place between them—and *ecce signum*—the mimics in question partaking of the characters of each in all respects as other hybrids do.

ANDREW MURRAY

67, Bedford Gardens, Kensington, Dec. 30, 1870

Measurement of Mass

THE favourite definition of *mass* in the text-books seems to be that the mass of a body is the *quantity of matter* it contains. If we had to do with but one kind of matter this would be intelligible, but I am at a loss to know what is meant when it is said that a piece of cork contains as much matter as a piece of lead. The only satisfactory method of explaining what is meant by the mass of a body, is to define it as a constant belonging to the body, which expresses the proportion between the force (measured statically) acting upon it and the acceleration produced; that every body has such a constant is the result of experiment. The mass of a body has no necessary connection with its weight. We employ weight to measure mass simply because gravity is a convenient constant force. If then we adopt a pound as our unit of *weight*, and use g to denote the force of gravity in reference to a foot and a second as the units of length and time, our unit of mass becomes the mass of g pounds, and this is not variable, although the unit of weight employed is variable; since if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound, precisely in the proportion in which gravity at the Pole is greater than gravity at London.

THE REVIEWER OF EVERETT'S "DESCHANEL"

PHOTOGRAPHIC PROCESSES OF THE PRESENT DAY

THE last two or three years will certainly mark an era in Photography, for not only have several novel and important printing methods been discovered during that period, but other processes of less recent origin have of late been so elaborated and improved as to have become at the present moment practical and easy of manipulation. All of these are, without exception, based upon the action of light upon the bichromates of potash and ammonia; in no single case is the use of a silver salt involved—the agent employed for securing the photographic image in ordinary paper printing—and this is, in truth, a point whose value cannot be too greatly insisted on; for the silver print, be it washed and freed as thoroughly as possible from any deleterious bodies, will always suffer, more or less, from attacks of an impure atmosphere, the delicate metallic film of which the image consists being peculiarly liable to change, from the sulphur compounds and other impurities not unfrequently contained in the air we breathe. And even those silver pictures which do not at first show actual traces of fading or discoloration, will very soon be found, on careful examination, to have parted with some of their original brilliancy, and to lack the pristine freshness which always characterises newly-produced albumenised prints.

It is a great step onwards, then, to have at our disposal practical processes in which the employment of silver may be altogether dispensed with, by the substitution of another material of a more permanent character, either in the form of a chromium compound, or, what is better still, in the shape of gelatinous or greasy ink; and so clear and promising does the photographic horizon appear just now in this direction, as to leave little ground for doubting that before long the practice of printing in silver will be generally abandoned.

All recent printing processes rest, as we have before said, on the action of light upon the bichromates, and here we would parenthetically refer to a simple and familiar experiment which will help very materially to simplify our subsequent remarks. The well-known plan pursued by school-boys for printing fern-leaves and other objects by the aid of the sun, will readily be called to mind by many of us, and this simple manipulation it is that forms the groundwork of the whole series of inventions before us. A sheet of ordinary paper, which has of course been sized, or, in other words, received a thin coating of gelatine, is rubbed over with a solution of bichromate of potash; the latter, as we know, when mixed with any organic body renders the same sensitive to light, and the sizing or gelatine upon the paper becomes in this way endowed with excitable properties. Having been dried in the dark, our sheet of paper is next placed in the sun with the fern-leaf, or other object to be copied, pressed down upon it, and the light acting upon all such portions of the sheet as are not covered up, browns the gelatine there and renders it insoluble; the sizing underneath the leaf, and screened therefore from the light, escapes this reaction and remains soluble, and this, on the printing being completed and the paper washed in water, is at once dissolved away, there remaining a white image of the leaf upon a brown ground composed of bichromated gelatine rendered insoluble by the sun's rays. This experiment may be regarded as the key to the whole question of photographic printing, and by bearing it in mind the reader will have no difficulty in at once comprehending the various inventions of the kind just now being made public.

The first method claiming our attention is the so-called carbon process. Photographic printing of this nature in one form or another has been carried on probably for upwards of fifteen years; but in its experimental stage the mediocre character of the results furnished by it were such as to deprive the system of any material support from photographers, and until, in fact, Mr. J. W. Swan, of Newcastle, made known his method, no easy or reliable *modus operandi* can be said to have existed. The plan followed by Mr. Swan was to prepare a warm solution of gelatine and bichromate of potash mixed with some finely divided pigments, such, for instance, as Indian Ink, and apply this mixture in the form of a coating to a sheet of paper, so that when dry, the tissue, as it is called, assumed the form of a thin, black cake with a paper backing. This sensitive tissue was placed under a negative to print in the ordinary manner, the light penetrating in parts to a greater or less degree, and thereby rendering the surface partially insoluble. On removing the tissue from the printing frame, it might, if it were desired, be forthwith washed to remove the soluble portions (as in the case of the fern-leaf experiment), but by so doing the picture would be hard and deficient in detail, and therefore a slight modification is here instituted. Instead of washing away from the face of the tissue, the operation is pursued from the back, the film being in the first place cemented face downwards upon a sheet of india-rubber, and in this condition put into a tank of warm water. The original paper backing of the tissue is in this way at once washed off, as is also every part of the gelatine mixture not rendered insoluble, which latter, constituting the image itself, remains attached to the india-rubber sheet before mentioned. The picture is now sufficiently developed, and indeed quite perfect, except that it is reversed to our view, for we are looking at it, it must be remembered, from the back; this defect is, however, easily remedied by attaching to the image another sheet of paper by means of gum or gelatine, and then dissolving off the india-rubber facing by means of benzole or turpentine, when the finished image is obtained resting upon a support of white paper. The object of washing the carbon tissue from the reverse side and not from the front, or surface exposed to the sun, is to secure the finer details in the picture by fixing at once to a basis such portions of the tissue as may have been but very slightly acted upon, and thus prevent them from being ruthlessly washed away when placed in warm water.

The actual composition of pictures produced in this manner consists of gelatine, pigment, and a stable chromium compound, the gelatine being in a fixed or tanned condition, by a subsequent immersion of the prints in a solution of alum, and thus there is every reason to believe in the permanent character of such prints. A more simple and ready method of carbon printing has been lately invented by Mr. Johnson, and termed the Autotype process, but the principles involved therein are nearly the same.

Passing from printing in permanent pigments, in which, as in silver printing, the aid of light is necessary for the production of each separate picture, we come next to photo-mechanical methods. Of these there may be said to be two kinds partaking of the nature of lithographic and engraving methods. Of the first description we may mention three modes of working, all of which are capable of yielding very creditable specimens of printing: these are Albert-type, the Lichtdruck process, and Edwards's collographic method. The three inventions, which differ from each other and from minor plans of a similar nature only in a few details, are all based on the same principles. A sheet of patent plate glass is in the first place coated with a thick solution of bichromate of potash and gelatine; this film on drying is placed face downwards upon a sheet of black paper in the sun, and in this way the light rays penetrate the glass and act upon the sensitive compounds adherent to its under side. The bichromated gelatine becomes insoluble and firmly cemented to the glass, except on the exterior surface, for the black paper upon which this has rested absorbs the rays and leaves the outer film of gelatine still in a soluble condition. A second coating of the sensitive gelatine mixture is now applied to the former one, to which it adheres perfectly, from the fact of the first surface being unchanged, and upon the second coating an image is printed by means of a negative in the ordinary manner. After printing, the progress of which, by-the-bye, may be watched through the glass, instead of washing the surface and dissolving out all the soluble parts, a sponge dipped in cold water is simply rubbed over it, the moisture being absorbed by the gelatine where it has not been acted upon by light, and is capable therefore of swelling out; those portions of the film, on the other hand, which have been rendered quite insoluble and hard, are unable to take up any water whatever, and remain untouched therefore by the action of the sponge, while other parts again, slightly exposed to light, absorb water just to that degree to which they have remained soluble. In this condition an inked roller is passed over the surface, in the same manner precisely as in lithography, the greasy ink adhering to all the insoluble surfaces (where no water is), and to the other parts in a greater or less degree according to the amount of water present in those places. Thus the gelatinised glass is treated in every sense like a lithographic stone, being moistened, inked, and pressed in the same manner; the resulting print, however, is generally finer than that obtained in ordinary lithography, as the graining of a stone surface is always somewhat coarse, while in the present instance the breaking up of the ink by the minute pores of the gelatine impregnated with moisture is of an exceedingly fine character. Many thousand prints may be pulled off a printing block of this kind before it is destroyed, as the double layer of gelatine imparts a yielding nature to the plate which is not easily damaged; in Germany, in England, and also, we believe, in America, this process of photographic printing is extensively practised.

But by far the most important of all methods yet discovered is the Woodbury engraving process. So simple, and at the same time so perfect in its work, a casual observer cannot but fail at once to appreciate its value. A thin sheet of gelatine is sensitised by impregnation with bichromate solution, and exposed to light under a negative; subsequent immersion in warm water removes the soluble portions from the surface, and we have then a thin gelatine plate upon which the image is represented, more or less, in relief. This matrix, as it is called, is hardened by treatment with alum, and placed when dry in a hydraulic press, in contact with a plate of type metal. Subjected to considerable pressure the metal plate takes the impression of the relief, and thus becomes in every sense an engraved plate, in which the darkest shadows are represented by the deepest hollows, the half-tones by slight undulations, while in the high lights there is no depression at all. The printing off of copies from this engraved plate is very ingeniously contrived. A little pool of transparent gelatinous ink is poured upon a sheet of white paper, and the metal plate is brought down upon the same with some pressure; all superfluous ink is at once pressed out, and after a pause of a few seconds to allow the warm ink to cool and to become set, the plate is again raised, and a beautifully shaded print is the result, in which the shadows and half-tones are formed by layers of ink of different thicknesses. For inasmuch as the ink is of a transparent character, and there is more or less of it deposited upon the paper according to the depths of the hollows in the engraved plate, so the half-tones are rendered with perfect gradation and fidelity, while in the high lights almost all the ink having been pressed away and removed, there remains nothing but the white paper which forms the basis of the print.

By printing at once from many plates (for a gelatine matrix will yield several dozen of them), photographs may be printed at the rate of some thousands daily, without of course the assistance of light in any way. Moreover, the productions are of so perfect and delicate a nature as to be confounded actually with silver prints, being at the same time absolutely permanent. We are glad to say that this method is also being worked practically and extensively in this country, as also in France and America, and will, without doubt, be the process of the future; for it is indeed the only mechanical process by means of which photographs may be rapidly produced, possessing the same degree of excellence as the beautiful, but alas! too fleeting, albumenised pictures.

H. B. P.

PHYSIOLOGICAL LABORATORIES IN GREAT BRITAIN

THE introductory paragraph with which you bring under the notice of your readers the very excellent description of the Physiological Laboratory at Leipsic by Dr. H. P. Bowditch, of Boston, begins with the phrase: "In England we have absolutely no Physiological Laboratory open for students." As this statement appears to me to admit of misconstruction, as leading to the inference that the present neglect of physiology in England is entirely due to the want of opportunities, it seems desirable to place before those of your readers who are interested in the subject, the actual position of this country as regards facilities for this kind of research.

There is, at all events, one institution in London, viz. University College, in which, for many years past, it has been possible for any man desirous of conducting experimental inquiries in Physiology or Pathology to do so; in proof of which I may refer to the experiments of the scientific committees of the Medical and Chirurgical Society on Apnæa and on subcutaneous injections; to my own experiments on the transmission of cholera to the lower animals, and on the influence of the respiratory movements on the action of the heart—all of which inquiries were made in the Physiological Laboratory of University College by persons unconnected with the Institution. In this enumeration I make no reference to the more abundant similar work which has been done by professors and students of the College, because my only object is to show that, as regards London at all events, it is many years since it could be said with truth that there was no Physiological Laboratory open to students.

At the present moment there are laboratories connected with one or two of the principal medical schools in this country to which students are admissible. In Edinburgh the Physiological Laboratory is fitted with all the instruments and appliances for research which are to be found in the laboratories of Germany; and for some time the students have been superintended in their studies by practical teachers, thoroughly versed in those methods of exact research which have been lately introduced into vital physics. In addition to the Physiological Laboratory, which is under the direction of Professor Bennett, the Professor of Medical Jurisprudence (Dr. Maclagan), and the Professor of Materia Medica (Dr. Christison) severally open their laboratories without charge, only requiring those who profit by them to meet the current expenses of research. Further, Dr. Arthur Gamgee, Lecturer on Physiology at the Royal College of Surgeons, has opened a new laboratory in which several separate inquiries are now being carried on. In Edinburgh, therefore, little can be said of want of opportunities; and here again the best proof of the existence of the means is to be found in the results attained, *i.e.* in the laboratory work actually performed by Edinburgh students during the last few years, as, for example, the researches of Dr. Fraser on Calabar Bean, of Dr. McDougall on the action of phosphorus, of Dr. Paton on the active principles of Broom, of Dr. Brunton on Digitalis, of Dr. Keith Anderson on the excretion of urea in typhus, of Dr. Young on the quantity of iron in bile, of Dr. Rutherford (now of King's College) on the vagus nerve, and others which might be mentioned, all of which possess the essential characteristics which constitute scientific value, though differing very considerably from each other in completeness. That so much has been already accomplished affords encouragement for the hope that as soon as the obstacles which still exist in the way of the student have been removed, Edinburgh will stand behind very few of the German schools of medicine in scientific productiveness. Of these obstacles, the most serious is that of expense. The large fees which are demanded, particularly for the physiological laboratory, have restricted the number of workers, the best of whom, as your correspondent, Prof. Stricker, so well pointed out in one of his recent communications, are not to be found either in England or Germany among the well-to-do.

It must be admitted that at the present moment our great London Schools are behind those of Edinburgh as regards means of physiological and pathological research. There are, however, good reasons for anticipating that in a very few years the aspect of things will be entirely changed. In King's College a physiological laboratory already exists, to which I understand students are admitted. I am not aware to what extent it contains the necessary accommodation, but it is certain that those who work in it have at all events the supervision and aid of a teacher thoroughly conversant with the art of investigation. At Guy's, Bartholomew's, and St. Thomas's I hear that similar improvements are at all events in contemplation. At University College, which, as has already been said, has long afforded opportunities not to be had elsewhere, these have been much extended during the present year. The Physiological Laboratory now consists of three rooms, one of which, of large size, is devoted to students, one is employed as a place of research and for the preparation of materials for demonstration, while the third is used for such special purposes as require a separate apartment.

The movement towards a more practical method of teaching the theory of medicine, of which the facts I have referred to afford evidence, is a new one. In the course of very few years it may be confidently anticipated that great progress will be made, and that although we cannot in so short a time hope to compete with the splendid institutions which exist at Leipzig or at Breslau, where spacious buildings, costly instruments, and abundant material, are freely placed at the disposal of the student without charge and without respect to his nationality, or any other consideration except his competency, we may hope to produce results which may be of equal importance for the advancement of Science.

At the present moment, the want which perhaps presses even more than that of laboratories, is that of *workers* in physiology—that is, of men already drilled in chemistry and physics, and prepared to devote a few years of their lives to continuous physiological or pathological research. The reason why such men are wanting is no doubt in great measure that hitherto the opportunities for work have been denied them. Another, and perhaps more efficient reason, is that the statement which is so often repeated in lectures, that medicine is based on physiology, is not really believed or accepted. Consequently, young physicians, instead of devoting their time and energies to research—whether conducted in the hospital wards or in the laboratory—spend the best years of their lives in the collection and exhibition of curiosities from the dead-house (miscalled pathology), in the compiling of masses of useless statistics, or in the performance of other drudgeries, as little conducive to their own improvement as to the advancement of medicine.

If it were not for the want of this scientific conviction, or, if I may venture to use the expression, scientific *faith*, the study of vital physics would make rapid progress in England, notwithstanding all the material obstacles which stand in their way. A dozen years of good work would place us again side by side with Germany, instead of being, as now seems possible, in danger of being overtaken by America.

This country still maintains its superiority over all other European countries in respect of medical and surgical skill, and has reason to be proud of it. But it is to be borne in mind that the men who exercise that *skill were* for the most part educated at a time when we could also compete with Germany in Science. As Science advances, its influence on practice, now so difficult to trace, will increase. If we continue to undervalue it as we have done, shall we not also eventually lose our practical pre-eminence?

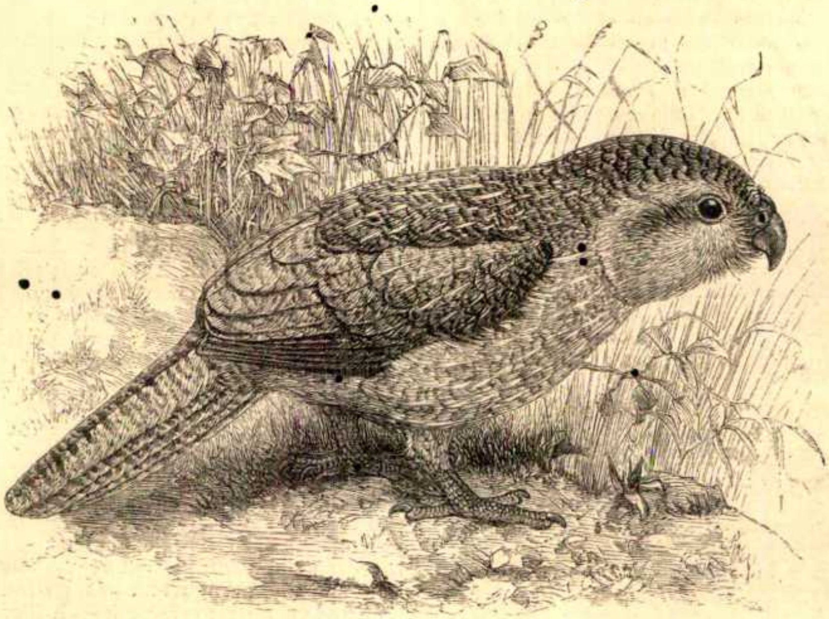
J. BURDON SANDERSON

NEW ZEALAND ANIMALS IN THE ZOOLOGICAL SOCIETY'S GARDENS

AT a recent examination for the Natural Science tripos at Cambridge, one of the pieces of information asked for in vain by the examiners was, I am told, "some account of the chief peculiarities of the Fauna of New Zealand." It so happens that the series of animals of our antipodean colony in the Zoological Society's Gardens is at the present moment unprecedentedly complete, and had the young gentlemen of Cambridge paid them an attentive visit, would have furnished ample materials for a proper answer to the examiners. I propose, therefore, to offer a few remarks upon them, and to make them the basis of some sort of answer to the question above mentioned.

Mammals in New Zealand there are none, except introduced species and bats, so it is not in this class of the animal kingdom that the visitor to the Society's gardens will find examples of New Zealand animals. There are, it is true, traditions and reports of some sort of "native rat"* having been occasionally met with, but no one has yet been able to produce a specimen of it, and after all, when captured, it may turn out to be only a stray individual of that cosmopolitan errant *Mus decumanus*. There are also marine mammals, both seals and whales, to be met with in the surrounding seas. But no terrestrial mammals, except bats, are indigenous to New Zealand, and for a country of such size, and situated in such latitudes, this is certainly a very remarkable and indeed unparalleled peculiarity.

With the next class of Vertebrates, however, the case is quite different. Bird-life, although according to the general evidence of the settlers not individually abundant, cannot be said to be badly represented in New Zealand. Dr. Otto Finsch, to whom we are indebted for the most recent summary of the birds of these islands,† gives 155 as the number of well-determined species hitherto met with, and there are, doubtless, a few more still to be made out, which will probably not long escape the grasp of several excellent naturalists who are now at work on the fauna of their adopted country. Of these 150 species, one third, or perhaps rather more, are found only in New Zealand. But what makes its bird-life still more peculiar, is that the greater part of these 50 species belong to some 17 or 18 generic forms which are quite unknown elsewhere. And several of these forms (such as *Heteralocha*, *Strigops*, *Apteryx*, and *Anarhynchus*) are of the most bizarre and extraordinary character.



KAKAPO, OR GROUND PARROT

Of the Huia bird (*Heteralocha gouldi*) I have already given a notice and figure in the pages of NATURE.* I am not aware that there is any other instance in the class of birds in which the difference between the bill of the two sexes is so great, though something of the same sort is exhibited in the Humming-birds of the genera *Grypus* and *Androdon*. The Huia bird in the Zoological Society's Gardens has recently moulted off its worn and injured plumage, and is now in excellent health and condition.

Of the very singular *Kakapo*, or Ground Parrot of New Zealand (*Strigops habroptilus*), I regret to say we have at present no example in the Society's Gardens. In the summer of last year one of these birds was successfully brought home from the Hokatika district, and was temporarily deposited by its owner in the Regent's Park Gardens, where it remained several months. But we were not able to come to terms as to its fair value, and the bird was consequently removed. The chief peculiarities of the *Kakapo* are its nocturnal habits, its abortive wings (which are nearly incapable of flight), the corresponding non-development of the crest of the sternum, and the possession of a facial disc, which gives it somewhat of an owl-like appearance. It is, however, a true parrot in the most essential part of its structure, and its food is strictly vegetable. During its sojourn in the Zoological Society's Gardens, it was fed principally upon corn and seeds of various sorts. The colour of this bird is of a bright green, freckled with black, which is said to assimilate it exactly to the mosses of the New Zealand forests where it is found. Although disappointed in not retaining possession of the example of this parrot which has been already in the Gardens, we are in great hopes (from intelligence lately received from New Zealand) of the approaching arrival of other individuals of this rare bird.

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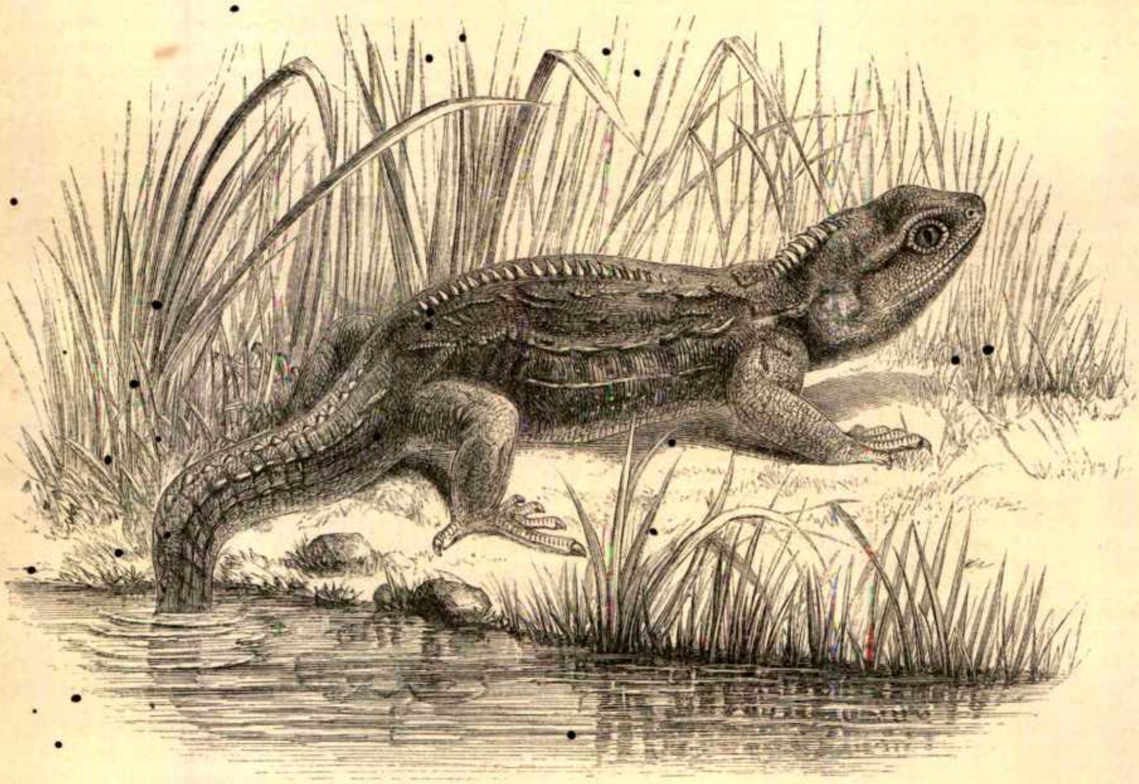
* Vol. ii. p. 146, June 23, 1870.

* See "Dieffenbach's New Zealand," vol. ii., App. p. 185.

† Ueber die Vogel Neu-Seelands. Von Dr. O. Finsch in Bremen. Journ. f. Ornith., 1870, p. 241.

The Kiwi or Apteryx, which I mentioned above as a third remarkable form of New Zealandian bird-life, has long been represented in the Zoological Society's living collection. In December 1857, Mr. Eyre, then Lieut.-Governor of the Colony, presented to us the first example of this remarkable form that had ever been brought alive to Europe. This bird is a female, as has been evidenced by her producing enormous eggs for several years in succession. The first of these eggs was deposited in June 1859, since which time she has usually laid two in the spring of every year, at intervals of about a month between them. The egg of the Apteryx when first deposited weighs about $14\frac{1}{2}$oz.; it is smooth and of a dirty white colour, and measures $4\frac{3}{4}$in. in length by $2\frac{9}{10}$in. in breadth. As the weight of the parent bird is only about 60 oz., it will be seen that the weight of the egg is nearly equal to one-fourth of the bird, a fact, I suppose, quite without parallel in the animal kingdom. Since the acquisition of the female, two additional specimens of the Apteryx, both of the opposite sex, have been received: one in 1864, presented by Major Keane, and a second in 1865, presented by Mr. Henry Slade. The female continuing to produce eggs after the males had been placed in her company, we were in hopes of rearing young Kiwis in the Gardens, especially as on more than one occasion the male, as is the custom among Struthious birds, commenced to incubate. This operation he performed by squatting closely on the egg placed between his feet, so that its long axis was parallel to that of his body. Unfortunately, however, in no case has there been any result, and the eggs when examined have shown no appearance of having been impregnated. And at length our sole surviving male fell a victim to the exemplary zeal with which he performed the duties of incubation. After sitting upon one of these huge eggs for upwards of six weeks, he died, probably from exhaustion, so that the original female, received in 1851, is at present the only representative of the species in the Society's Gardens.



TUATERA LIZARD

Besides the above-mentioned specimens of Mantell's *Apteryx* (which is probably not really different from the original *Apteryx australis* of Shaw) the Zoological Society have recently received a single living example of the undoubtedly distinct Owen's Apteryx (*Apteryx owenni*). This bird arrived in July 1869, having been forwarded as a present from the Acclimatisation Society of Otago. It is readily distinguishable from the ordinary species by its smaller size, spotted feathers, and the softer and more fluffy plumage, but is closely allied to it in general structure and in all other peculiarities.

The fourth bird that I mentioned as one of the more remarkable bird-forms inhabiting New Zealand, has not yet been received in this country in a living state, indeed it is only quite recently that naturalists have become perfectly acquainted with the eccentricity of its structure. It is a small wading bird, allied to the Shore-plovers, but possessing the extraordinary feature of having the end of its bill curved towards the right. By the examination of the chick of the *Anarhynchus frontalis*, as this singular bird is called, Professor Newton has recently shown that this abnormality commences from the egg.* Such instances of asymmetrical structure are very rare amongst the more highly organised animals, indeed I am not aware of any similar instance among birds.

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* Proc. Zool. Soc. Nov. 15 1870, p. 674.

Besides the birds above-mentioned, the Gardens contain the only examples ever received in Europe of the *Kaka* Parrot (*Nestor hypopolius*), another form of bird-life restricted to the forests of New Zealand.

I must now say a few words about the Reptilian life of New Zealand. As regards this branch of the animal kingdom, the Zoological Society's Gardens cannot be said to be very well supplied; at the same time we have now in them a living example of what is certainly the rarest and most singular species of the reptilian class that inhabits these islands. This is the Tuatera Lizard (*Sphenodon punctatum*), of which I have already given a notice in a former article in this journal.* Dr. Günther will, I hope, forgive me for calling it a lizard, for in spite of his elaborate proof that it is utterly different from every other known form of the Lacertian order, I cannot quite agree with him that it is entitled to form an order of itself.

As regards the general character of the reptilian fauna of New Zealand, after mentioning the Tuatera, there is not much to be said. There are, I believe, no representatives of the orders of Tortoises, Crocodilians, or Ophidians to be met with in these islands. Besides the *Sphenodon*, the only other reptiles indigenous to New Zealand are about half-a-dozen small lizards belonging to the families *Scincidæ* and *Geccotidæ*. Some of these are of Australian character, the remainder are peculiar to New Zealand.

Of the class of Batrachians, only one member is known to exist in New Zealand—a frog (*Leiopelma hochstetteri*), not found elsewhere.

Fresh-water Fishes, which must also be taken into account when estimating the peculiarities of a land-fauna, are likewise not very abundant in New Zealand. Two forms, however, deserve special notice, as indicating a former land connection at however remote an epoch, between Australia and South America passing through these islands. These are *Protrotoctes*,† belonging to a group representative of the Salmonoids of the northern hemisphere, and *Galaxias*, belonging to a family allied to the Pikes (Esocidæ) which is likewise found in the rivers of Australia and Antarctic America. Nearly allied to *Galaxias* is a very curious form, recently described by Dr. Günther under the name *Neochanna apoda*,‡ but remarkable for the absence of ventral fins, and its mud-loving habits. This fish is restricted to New Zealand.

The peculiarities of the fauna of New Zealand, so far as is shown by its terrestrial vertebrated animals, may therefore be summed up somewhat as follows:—

1. The absence of all Mammals, except two species of Bats.

2. The presence of numerous forms of Bird-life not known elsewhere, such as *Heteralocha*, *Nestor*, *Strigops*, *Apteryx*, and *Anarhynchus*.

3. The absence of Reptiles, except two genera of lizards and the *Sphenodon punctatum*, which, according to some of the best authorities, has claims to constitute an order of reptiles *per se*.

4. The absence of Batrachians, except one species of frog not known elsewhere.

5. The paucity of fresh-water Fishes—few genera only being known—which are allied partly to Australian and partly to Antarctic American forms.

But before closing our summary, it must not be forgotten to be mentioned that almost within the historical period New Zealand was tenanted by upwards of a dozen species of Struthious birds, constituting a family *per se*, but allied to the Cassowaries and Emus of Australia and the Papuan Islands. We must therefore add as a sixth item:

6. The recent presence of a peculiar family of gigantic Struthious Birds, now extinct—the Dinornithidæ—restricted to New Zealand.

P. L. SCLATER

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* See NATURE, vol. ii. p. 148

† Cf. Günther, Proc. Zool. Soc., 1870, p. 152.

‡ See Ann. Nat. Hist. ser. 3, vol xx. p. 305.

NOTES

IN other columns will be found the reports which have reached us up to the present time from the different sections of the Eclipse Expedition. Notwithstanding the unpropitious weather at nearly all the stations, we trust that some observations have been made which will throw light on still unsolved problems of Solar Physics. It is with great satisfaction that all English astronomers will hear that M. Janssen performed his perilous balloon voyage in safety, and reached his destination in Algeria; although he does not appear to have been rewarded with the opportunity of taking any satisfactory observations.

THE *Journal of Botany* states that the post of Curator of the Botanic Gardens, Calcutta, vacated by the death of Dr. Anderson, has been given to Dr. King, who has done some work in Indian botany, but is comparatively unknown in English botanical circles. This is the best-paid botanical appointment in England or any of its dependencies.

THE following are announced as the probable arrangements for the Friday evening meetings at the Royal Institution before Easter, 1871, to which members and their friends only are admitted:—Friday, January 20th, Professor Tyndall, LL.D. F.R.S.; Friday, January 27th, Professor Odling, F.R.S.; Friday, February 3rd, W. Spottiswoode, Treasurer R. S.—Some experiments on Successive Polarization of Light made by Sir C. Wheatstone; Friday, February 10th, E. J. Reed, C.B.—On Some Fallacies connected with Ships and Guns; Friday, February 17th, James N. Douglas, Engineer to the Trinity House—On the Wolf-Rock Lighthouse; Friday, February 24th, Dr. W. B. Carpenter, F.R.S. &c.—The latest Scientific Researches in the Mediterranean and Straits of Gibraltar; Friday, March 3rd, Capt. Noble, F.R.S.—On the Pressure of Fired Gunpowder; Friday, March 10th, W. Mattieu Williams—On Rumford's Scientific Discoveries; Friday, March 17th, J. Norman Lockyer, Esq., F.R.S. —On the Eclipse; Friday, March 24th, Professor J. Clerk Maxwell, M.A., F.R.S.—On Colour; Friday, March 31st, Professor Max Müller, LL.D.—On Solar Myths.

THE following gentlemen have obtained a first class in Natural Science at Oxford at the last examination:—C. Childs, Scholar of Merton, F. H. Champneys, Brazenose, F. W. Fison, Scholar of Christ Church, S. J. Sharkey, Scholar of Jesus.

THE Natural Science Scholarship at Merton College, Oxford, has been awarded to Mr. Richmond of the Manchester Grammar School, and an Exhibition to Mr. Ferguson of the same school.

PROF. AGASSIZ announces that the Museum of Comparative Zoology at Harvard College, Cambridge, U.S., is prepared to furnish extensive collections of all the rocks and loose deposits found upon and about the Key and reefs of Florida; also complete collections of the Corals in fresh and well-preserved specimens, in exchange for recent and fossil corals from other parts of the world.

THE *Pall Mall Gazette* states that a committee has been appointed, with Captain Beaumont, R.E., M.P., as president, and Lieutenant Grove, R.E., and Mr. Abel, F.R.S., as members, to carry out experiments on the utilisation of balloons for reconnoitring purposes. The former experiments on this subject, which were carried out at Woolwich and elsewhere a few years ago, were not attended with any useful results, and we believe the attempts which have been made during the present war to reconnoitre with balloons—an application not to be confused with the use of balloons for postal purposes—have not been more satisfactory. But the Americans on several occasions employed balloons to reconnoitre with fair success, and Captain Beaumont's committee may be able to throw some new light upon a subject which it certainly seems worth while to work out.

THE frost which has now lasted for a fortnight is the most severe that has been known in England since the memorable one of Christmas, 1860, that is, for exactly ten years. The lowest temperature at Blackheath was 15·3° F. on the night of the 24th December; but in the eastern counties the cold was more intense, being 8° at Hull, and nearly as low at Norwich, Nottingham, and Leicester. The highest minimum recorded by Mr. Glaisher in the *Gardener's Chronicle*, at any English station is 19·0°, at Leeds. In Scotland the minimum varied between 5·0° at Perth, and 19·2° at Aberdeen. The average was slightly higher in Scotland than in England. For the first fourteen days of the frost, the temperature scarcely rose above the freezing-point night or day, a very unusual circumstance in this country.

A SOCIETY has just been instituted under the designation of *The Society of British Archæology*. It originated in a meeting held on Dec. 9th in the rooms of the Royal Society of Literature, the proposed objects being "the investigation of the Arts, Archæology, History, and Chronology of Ancient and Modern Assyria, Palestine, Egypt, Arabia, and other Biblical Lands, the promotion of the study of the antiquities of those countries, and the preservation of a continuous record of discoveries now or hereafter to be in progress." Dr. Birch, of the British Museum, who occupied the chair on that occasion, explained that the proposed society would clash with none of the philological or exploration associations now in existence, but would have a distinct purpose—to concentrate and utilise the scattered materials connected with the geography, arts, and antiquities of the lands of the Bible, and to systematise the progress of Archæological research in England, America, and the Continent. The Society has already received the promise of the support of the most eminent living Biblical investigators, and another meeting will shortly be summoned for its complete establishment.

UNDER the title of "Science Education Abroad," Principal Dawson of M'Gill University, Montreal, republishes his Annual University Lecture, of the session 1870-71. After reviewing the state of scientific education in foreign countries from a Canadian point of view, as exhibited by the present condition of the various institutions for the spread of Science in Great Britain, the United States, Germany, and Switzerland, he contrasts with this the want of Science teaching in Canada. With the exception of two or three small and poorly supported agricultural schools, he states that the Dominion does not possess a school of practical Science, notwithstanding its mining resources, second to those of no country in the world. In the M'Gill University itself some part of Natural or Physical Science is studied in each year of the College course; but this falls far short of providing the full measure of the higher Science education required for the development of the resources of the country.

PROFESSOR M'NAB has been pursuing his investigations on the Transpiration of Watery Fluid by Leaves, to which reference was made a short time since in our columns.* The plant used in all the experiments was the common laurel (*Prunus laurocerasus*), and the fluid to test the rapidity of the ascent lithium citrate. The following are some of the more important results arrived at:—The total quantity of water in the leaves was found to be 63·4 per cent.; but of this, the proportion which could be received by calcium chloride, sulphuric acid, or by the action of the sun, was only from 5 to 6 per cent.; hence Dr. M'Nab calculates the amount of transpirable fluid in the stem and leaves to be between 6 and 7, the amount of fluid in relation to cell-sap to be between 56 and 57 per cent. The rapidity of transpiration he found to be in sunlight 3·03 per cent. in an hour, while in diffused daylight it was only ·59, and in darkness ·45 per cent. in the same time. These experiments were made when the plant had access to water by means of its stem. When the leaves were exposed without any means of supplying themselves again, the following results were obtained:—In a saturated atmosphere in the sun, 25·96 per cent. was transpired in an hour, in a dry atmosphere in the sun 20·52 per cent. In the shade, the numbers were reversed—viz., in a saturated atmosphere nothing, in a dry atmosphere 1·69 per cent. When immersed in water, the leaves absorbed 4·57 per cent. of their weight in seventeen hours, in a saturated atmosphere nothing whatever in eighteen hours. The under surface of the leaf transpired nearly ten times as much as the upper surface.

WE are very glad to see that the second series of penny science-lectures delivered in November in the Hulme Town Hall have been reprinted. They embrace:—Coral and Coral-reefs, by Prof. Huxley; Spectrum Analysis, by Prof. Roscoe; Spectrum Analysis in its relation to the Heavenly Bodies, by Dr. Huggins; and On Coal, by Mr. Boyd Dawkins. The reports have all been revised by the respective lecturers; and being published at one penny each, ought to have a very large sale. We cannot conceive a greater aid to scientific teaching than the circulation of these lectures; both for the information they contain, and as models to lecturers of what scientific lectures to working men should be.

THE last number of the *Bulletin de la Société Royale de Botanique Belgique* contains an interesting paper by M. Devos on the plants naturalised in, or introduced to, Belgium. Of the 1,566 phanerogams recorded for that country, no less than 512 are supposed to have been introduced. Of these 91 are from southern Europe, 137 from the east, 14 from central, and 5 from north Europe; "alpine regions" have furnished 16, 34 are from America, and 5 from Africa; while the native countries of the remaining 210 are unknown. The distribution of each species is traced out with reference to its occurrence in other countries under similar circumstances; and the paper is a valuable contribution to phytogeographical science.

AMONG the curiosities of scientific literature a little work, published a few years since, must find a place. It is entitled "Principles and Rudiments of Botany, delivered according to an Iulian system of arrangement and Iulian method of classification; by C. R. W. Watkins, Gent., late Captain in the Bombay Army." These "principles and rudiments" are here, according to the preface, delivered in language "better adapted for the intellectual amusement and instruction of young persons of both sexes" than that employed in previous works; and "Botanical science" is "rendered more agreeable to students in modern times." The following extract will give a faint idea of the mode in which these promises are fulfilled, and also of the contents of the volume:—"The pink (*Dianthus*) has four or five idola; ten to twenty ikona, and twenty to forty petala. The flowers are few, and di, tri, quinque ligate, and they terminate separately and irregularly. The Sweet William (*Dirythme*) has two idola, ten ikona, and five petala. The flowers are numerous and chorovinkulate, and the mode of gemmation comprises several synterminal and equimarginal chorrythma, or concurrythma. They cannot, therefore, be of the same genus; because the numerical indices, and typical characters of each gemmos, or hermaphral gemm bud of the two kinds of plants, are not symbolical; but differ, as well as the mode of gemmation, more widely than the specific, and physical circumstances of their constitutional, or peculiar veget-organical structure."

THE laws which formerly existed in Scotland, and are still enforced in Denmark, to compel the extirpation of the Corn Marigold (*Chrysanthemum segetum*), have their parallel in New Zealand, in some parts of which it is a punishable offence to allow the growth of thistles. In the colony of Lyttelton proceedings were taken, during the present year, against a gentleman for having neglected to eradicate certain thistles, after having been requested to do so. The defendant alleged that men had been employed for ten days in striving to exterminate them, and that ten donkeys were kept for the sole purpose of eating off the tops of the thistles. The Bench, however, were of opinion that no adequate steps had been taken since a previous conviction, and imposed a fine of 5*s*. per day from that date.

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* See NATURE, vol. ii. p. 515.

WE learn from Dr. Müller's last report of the Melbourne Botanic Gardens that the noxious "Cape Weed" (*Cryptostemma calendulaceum*), is becoming suppressed in his vicinity by the gradually denser growth of lucerne, clover, and grass. The plant was noticed as an inexterminable weed of Australia, by Baron von Huegel, in 1833. It would appear that more than one plant is known as "Cape Weed," as another Composite (*Hypochæris radicata*), is so called in New Zealand, where, in the neighbourhood of Dunedin, it is spreading to a serious extent.

A WRITER in the *Field* for Dec. 17, advocates the cultivation of *Symphytum asperrimum* as food for cattle. He recommends that the plants should be set about two feet six inches from each other; and that the most forward of the leaves should be plucked as they develop. Horses are very fond of it, and it is beneficial in its effects upon them. It is not recommended that it should be made store of, but the leaves should be gathered and eaten fresh. It will be remembered that Prof. Buckman, when at Cirencester, instituted some experiments upon this plant, which led him to the conclusion that it was not specifically distinct from the common Comfrey (*S. officinale*); and it is therefore probable that the latter would be equally suitable for cattle. *S. asperrimum* is a plant of very rapid growth; boiled as a vegetable it is palatable, and in Germany it is a favourite ingredient in salad.

AMONG the various notes upon tree-worship which have lately appeared, no mention has been made of the Cotton-tree (*Eriodendron anfractuosum*). Of this Dr. Macfadyen, in his "Flora of Jamaica," writes as follows: "Perhaps no tree in the world has a more lofty and imposing appearance, whether overtopping its humbler companions in some woody district, or rising in solitary grandeur in some open plain. Even the untutored children of Africa are so struck with the majesty of its appearance, that they designate it the God-tree, and account it sacrilege to injure it with the axe; so that, not unfrequently, not even the fear of punishment will induce them to cut it down. Even in a state of decay it is an object of their superstitious fears; they regard it as consecrated to evil spirits, whose favour they seek to conciliate by offerings placed at its base."

AN early mention of tobacco is that in Hakluyt's "Voyages," by M. Jaques Carthier, in 1534. Speaking of the people of "Hochelaga, up the river of Canada," he says, "There groweth also a certain kind of herbe, whereof in Sommer they make great prouision for all the yeere, making great account of it, and onely men vse of it, and first they cause it to be dried in the Sunne, then weare it about their neckes wrapped in a little beasts skinne made like a little bagge, with a hollow peece of stone or wood like a pipe, then when they please they make pouder of it, and then put it in one of the ends of the said Cornet or pipe, and laying a cole of fire upon it, at the other ende sucke so long, that they fill their bodies full of smoke, till that it commeth out of their mouth and nostrils, even as out of the Tonnell of a chimney. They say that this doth keepe them warme and in health, they neuer goe without some of it about them. We ourselves haue tryed the same smoke, and hauing put it in our mouthes, it seemed almost as hot as Pepper."

As a general rule, plants which are casually introduced to, and become firmly established in, any country, are by no means to be regarded as useful acquisitions; some of the worst weeds of cultivation may be found among them. An exception to this, however, may be noticed in *Lespedeza striata*, "Japan clover," or "wild clover," as it is called in the localities to which it has introduced itself. It has recently sprung up in great abundance in all parts of the Southern States, and has proved a great acquisition to the farmers. The roots, which are long and fibrous, penetrate and flourish even in sandy roads and in yards; and a single root will send out as many as six hundred branches. The *Lespedeza* is a close-growing plant, covering the ground as with a carpet of green, and is taking the place of the sedges and other weeds upon the waste lands and clearings. Cattle, horses, and sheep eat it greedily; and it is in every way an important addition to the fodder-plants of the country.

A CONTRADICTION is given to the reported discovery of coal in the Bellary district in Madras.

ON the 3rd of October the Faculty of Science and Polytechnic School were opened at Quito in Ecuador. There is reason to believe that these establishments of the repudiatory republic are more pretentious than real.

CINCHONA has so fully succeeded in the Neilgherry Hills in India that the first shipment of bark from a private plantation to the extent of 4,000 lbs. is taking place. The Government promoted Cinchona plantations chiefly for the supply of India, but they are already engaging in the home trade.

A RARE discovery has been made near the port of Mejillones in that district of the rainless desert of Atacama belonging to Bolivia, of a spring of fresh water. This has been granted to the discoverer for ten years, and then to become the property of the State.

ON the 18th of October, at 5 P.M., a slight earthquake was felt at Salvador in Central America. There had been heavy rains for some days.

THE Queensland Acclimatisation Society, under the patronage and with the assistance of his Excellency the Governor and other influential persons in the colony, appears to be in a satisfactory condition, both financially and with regard to the work done, as well as the earnestness and ability of its workers. In the park belonging to the society a considerable number of foreign trees have been introduced, and amongst them the splendid tree of Madagascar (*Poinciani regia*), which, we are told, is being raised by hundreds. This success of foreign trees is very gratifying at a time when the question of want of shade trees is a matter of much interest, not only in Queensland, but also in other colonies. The Shola (*Æschynomene aspera*) is likewise amongst the recently-introduced plants which promise success. It is a native of India, and is well known on account of its light wood being used for making the "pitte" hats, so much used in tropical countries. Among British birds introduced by the Society, and which survived the voyage from London, the blackbird, thrush, starling, rook, sparrow, and lark, have been liberated in the Botanic Garden; but at the time the report was drawn up little could be said as to their condition or whereabouts, except that one pair of sparrows were then rearing their second brood in the heart of the city. Large numbers, also, of Chinchilla rabbits, and of the wild English breed, have been turned loose on the islands of Moreton Bay, but care has been taken to keep the different kinds separate.

IT is not a little remarkable that *Corchorus capsularis*, one of the plants yielding the jute of commerce now so largely imported and used in the manufacture of carpets, and also largely used in India for gunny bags, should, though growing wild in Sumatra, not be cultivated in that island. The bags in which Java coffee is exported are made of this fibre. Large quantities of these ready-made bags are annually sent from Bengal, even to Java.

THE ECLIPSE EXPEDITION

As we intimated last week, the weather was more or less unfavourable at nearly all the stations for the observation of the Total Eclipse of Dec. 22nd. We give, in another column, an account of the preparations made by the Sicilian department of the Expedition, received by us. The following account of the results obtained at some of the other stations is compiled chiefly from reports furnished to the *Times* and *Daily News*.

From Cadiz we have an interesting account by the Rev. S. J. Perry, as follows:—

"The situation of San Antonio is found to be lat. 36° 37′ 13″ N., long. 24′ 15″ W. of Greenwich. Time signals were daily received from the San Fernando Observatory, and all our chronometers carefully rated by Capt. Toynbee. Our thirteen observers were distributed as follows: Mr. Moulton, of Christ's College, Cambridge, with Mr. Baines, of Oxford, were to observe with the polariscope at Sanlucar, the extreme W. point on the Spanish central line of totality, and 12 miles N.W. of San Antonio. Near Xeres, 5 miles N.E. of San Antonio, were stationed Mr. P. Naftel, for an eye sketch of the corona, Mr. F. C. Penrose to sketch the same as seen through a telescope, and Mr. Abbay, of Wadham College, Oxford, to observe with the spectroscope. Mr. W. Smyth sketched near Arcos, 17 miles E.N.E. of San Antonio, using a telescope of the same aperture as that of Mr. Penrose. The rest of the observers remained at San Antonio. At this station the spectroscopic observers were Capt. Maclear, R.N., and myself, assisted by Mr. Hostage. Polarisation was to be observed by Mr. Hudson, of St. John's, Cambridge, and Mr. Ladd, optician; and an eye sketch of the corona to be made by Mr. Browne, of Oxford. The weather has been unexceptionally bad ever since our arrival, the only fine day being the 21st. Our observers were therefore spread out as much as possible, in hopes of not failing altogether on account of bad weather. The results justified our anticipations. The fine weather of the 21st lasted but a day, and at two A.M. of the 22nd the clouds and rain returned. At San Antonio a break only came some 48^s after first contact, when a distinct notch was observed on the solar disc. This break was only a change from thick cloud to thin cirrhus, but we were enabled to observe the time of contact of the limb of the moon with several of the more remarkable solar spots. In the north the sky was partially clear, but in the south no part of the heavens was free from cloud. A very striking change of light on the landscape was noticed when little more than three-fourths of the solar disc was covered, and a chill was felt by all. The thermometer observed by Capt. Toynbee fell 3° F. from the commencement to totality, and rose again 1·7° before the end of the Eclipse. The barometer was falling rapidly all the time of the Eclipse, and also afterwards, at the rate of 0·04 in. an hour. The wind was W. by N. true. During totality it lulled, but freshened afterwards with very heavy rain. The moment of totality approached, and no chance remained of even a momentary break in the thin cirrhus that enveloped the sun, and obscured most of the southern heavens. As the crescent became thinner, the cusps were observed first to be drawn out a length of several minutes, and then blunted; the well-known Baily beads were formed, and the corona burst forth more than 20^s before totality. Viewed through a telescope of very moderate dimensions the spectacle was grand, but the cirrhus clouds destroyed almost all the grandeur of the effect for the naked eye. The red prominences were numerous, but none apparently very remarkable; Mr. W. H. Browne, of Wadham College Oxford, considers their colour to have been of a bright yellowish red tint. The same observer notices that the corona was perfectly free from striation, outline distinct, and approximately quadrilateral, but extending farthest in the direction of first contact. The brightest part of the corona appeared to the unassisted eye to be scarcely more than one-tenth of the sun's diameter, fading rapidly when one-fifth, but being still clearly visible at seven-eighths. Some observed two curved rays, but the general appearance was that of a diffuse light interrupted in four places distinctly, and in a fifth faintly, by dark intervals. The corona was white, and rendered faint by the clouds. The darkness was never sufficient to prevent sketching with comfort without the aid of a lamp. Venus alone was visible. Totality ended by the formation of Baily's beads, and the corona was visible to the naked eye 15^s or 16^s after totality. The corona was seen for $2^m\,50^s$, totality lasting less than $2^m\,10^s$. The clouds obscuring the sun appear to have destroyed almost all chance of detecting any except atmospheric polarisation. Mr. Ladd remarked that the polarisation was stronger on the corona than on either the moon's surface or the cloudy sky.

"No report has as yet been received of the polarisation observations at Sanlucar. The observations with the spectroscope were also greatly interfered with by the cirrhus, and the best instrument was rendered entirely useless. The intensity of the light from the corona, as seen through the clouds, could not, I think, have been more than one-eighth of that of the bright moon, if so much, and, consequently, I was unable to detect the faintest trace of light through the three compound prisms I was using. The chances of observing satisfactorily, considering the state of the sky, were greatly diminished by the largeness of the direct image given by the Cassegrain I was compelled to use. Knowing that an unfavourable sky would render observations with a powerful spectroscope quite impracticable, I desired Captain Maclear to observe with a small direct vision Browning spectroscope, attached to a four-inch achromatic by Jones, mounted equatorially. The slit was placed radial at the centre of the east limb, and close to it, and immediately totality commenced the ordinary solar spectrum was replaced by a faint diffused light, and bright lines near C, D, b (or E), and F. No absorption bands. The slit was then removed to a distance about 8′ from the limb, and the same lines remained visible. The centre of the moon was next tried, and the bright lines were still seen, but only half as strong as before. The slit was then placed 8′ outside the W. limb, and the lines became as strong as before, and were C, D, one three-quarters of the distance from D to E, and another half way between E and F. Lastly, placing the slit near the sun (on a prominence) two new green lines, and a very brilliant line beyond F, were added to those already visible, but the line near E may have disappeared. The lines seen on the moon were, I suppose, due to the diffusive power of the cirrhus clouds; and the same may perhaps be true of the apparent coronal lines.

"Mr. Abbay, observing at Xeres with a spectroscope of two prisms of 45°, belonging to Professor Young, saw the bright lines C, D, F; and afterward F, and a line rather more bright than F, at some distance on the less refrangible side of B, C not noticed then. These two observations were, I think, taken at points external to the prominences, but I cannot at present speak with certainty, as no note to that effect is entered in the memorandum I received. A comparison of these observations with those of other observers more favoured than ourselves will doubtless lead to valuable results. Shortly after totality the clouds thickened still more, and nothing further could be observed."

The view of the Eclipse obtained near Arcos is described as very magnificent; a sketch was made there by Mr. Warrington Smyth. At the American station near Xeres there was a break in the clouds, which lasted somewhat more than half the totality. But Lord Lindsay's party was the most favoured in this country, having seen the sun through a rent in the clouds for five minutes, and this time embracing the whole of totality. Mr. O. Airy and

Mr. Hammond, of Trinity College, Dublin, observed at San Antonio, and were kindly assisted by Lieutenant P. H. Worgan and Mr. T. H. Atkinson, of H.M.S. *Lee*.

Another correspondent, from Cadiz, writes to say that Lord Lindsay succeeded in taking several excellent photographs from a vineyard belonging to Mr. Campbell, half way between Port St. Mary's and San Lucar.

The American party at Xeres saw the totality for about a minute.

From Gibraltar, Mr. R. M. Parsons sends the following report :—

"The party that left England for the purpose of observing the Total Eclipse of the sun at Gibraltar—namely, Messrs. Carpmael, Gordon, Lewis, Buckingham, Beasley, Harrison, Anson, Abbatt, Talmage, and myself—disembarked from Her Majesty's ship *Urgent* on the 14th Dec., and all but the three last-mentioned proceeded to Estepona, a village in Spain, about thirty miles north-east from Gibraltar, and situated in the central line of totality. By this division of the party an additional chance was afforded of observing the phenomenon in case of bad weather, and Estepona offered the advantage of some 13^s longer time of total obscuration than Gibraltar, a condition very desirable for the particular class of observations required by some of the party. The weather at Gibraltar was wet and cloudy almost from the time we landed until the day before the Eclipse, when a strong breeze W. by N. gave a beautifully clear sky, which lasted till about midnight. Mr. Talmage, the director of the Leyton Observatory, and I, determined to observe from the Moorish Castle if the atmospheric conditions of the 21st should hold good on the 22nd ; but I arranged to receive constant telegrams of the weather from Europa Point on the morning of the 22nd, and conveyance was provided to move the instruments at the latest practicable period, in case any other position afforded better chances of success. Mr. Talmage was to take angular measurements of Saturn, if seen through the corona, Mr. Abbatt to sketch the corona, and I was to examine it with a polariscope. The westerly wind increased in force on the 22nd, but brought with it scud and dense clouds across the Bay from the Spanish mountains ; everywhere these clouds were massed in the sky, separated by small intervals of hazy blue. The last telegram was received from Europa at a quarter-past eleven, forty minutes before commencement of totality, stating 'sky quite overcast, heavy clouds moving south-east, sun hardly visible.' This, together with the circumstance that the Rock did not appear to affect the clouds which were moving under the influence of a westerly wind, led me to conclude that the chances of good vision were equal at any position on the Rock. After waiting on the Line Wall for these telegrams, at which place I failed to observe the first contact, while Mr. Talmage failed in the same endeavour at the Moorish castle, I joined him there, leaving Mr. Abbatt with his telescope erected on the flat roof of a house about a quarter of a mile west of the castle. The cloud which caps the Rock of Gibraltar, the summit of which is 1,396ft. above the sea, during east winds, or Levanters, leaves this comparatively low level clear. About 30^s before the commencement of totality, a hazy blue break in the clouds enabled us to see the thin bright crescent of the sun, but unfortunately this patch of hazy blue sky, which favoured others for a few seconds, came a little too early in front of our position, and it was followed by a dense cloud, behind which the entire phenomenon of totality was hidden from us. The darkness was considerable, but not so great as when I observed the Total Eclipse of 1860, at Nisqually, in an unclouded sky. Then a lamp was necessary to enable a white-faced pocket chronometer to be read ; yesterday I could see the divisions distinctly at the distance of eight inches without such aid. Mr. Abbatt had the good fortune to see the corona and some red prominences, but only for about two seconds before they were lost in the same dense cloud ; he estimated the breadth of the corona at about a sixth part of the moon's radius. Professor Newcomb, of the United States' Expedition, was able to see all four contacts, and to take several measurements that were necessary for the work he has in hand ; he also caught a glimpse of the corona, but says he could make no use of it.

"Mr. Lewis states that the party at Estepona only saw the total phase through a break in the clouds for about 10^s or 15^s, when it was covered by light cloud. Mr. Buckingham, at Estepona, states that they had there heavy rain ; he could take no photographic pictures, but Mr. Carpmael had observed three bright lines in doubtful positions, and Mr. Lewis found the corona polarised ; the rest of the party had negative results. Mr. Harrison, who was distant a mile from the others, did not see the total phase. Mr. Anson had not time at the moment to sketch what he saw, but probably may be able to do so from memory, and Mr. Efson, who had then joined the party, had no opportunity of obtaining satisfactory observations."

Mr. Abbatt reports :—"In no part did the corona or the prominence extend beyond 1-8th or at most 1-6th of the moon's radius beyond the limb. I thought the moon darker than the sky. I noticed four high red prominences—there were more, but when, for an instant, I took my eye off the telescope a dense black cloud had obscured everything till the narrowest streak of the sun appeared on the western side, and nothing but the ordinary phenomenon of a partial eclipse was to be seen. The darkness during totality was not so great as I expected it would be. Two stars were seen, one near the sun and the other overhead, but I can get no further information as to their exact position."

Another observer writes as follows :—"The eclipsed orb presented itself through a rent in the clouds not greater in area than ten times that of the disc of the moon's shadow. That part of the opening which was above the eclipsed orb was clear like the sun at twilight, and in it were visible to the naked eye the planets Venus, Mercury, and half a dozen stars. The remaining part was covered with a thin haze. The moon's shadow appeared to the eye, assisted by a somewhat weak binocular glass, to be a dark circular disc with an even boundary and of uniform shade. Within the corona, and touching the circumference of this shadow, appeared five or six spots of brilliant carmine, varying in form and size and at irregular distances apart. Two of these spots, or 'red flames,' as they are called, on the eastern side of the disc, and at about 55° and 80° respectively from the vertex, seemed decidedly the largest and most prominent ; they were tongue-shaped, and protruded about 1-6th the width of the corona. In their neighbourhood the corona was brightest and widest. There, too, the rays of the corona appeared to be gathered more distinctly into groups than elsewhere, faint shadows being visible between the groups. The corona consisted of brilliant rays of extremely faint prismatic hues ; these rays at first sight appeared pretty evenly distributed all round, but closer examination seemed to detect the fact of there being bundles of rays in nearly regular groups. The width of the corona was about 1-8th the apparent diameter of the moon's shadow. It was very nearly concentric with the disc of the shadow ; its boundary was well defined, but 'jagged ;' the perimeter, except opposite the two most prominent red flames above-mentioned, where the boundary slightly protruded, was circular."

From Seville, we have a report from M. E. A. De Cosson :—

"The Eclipse began at 10.30 A.M. (Seville time). At 10.45 one-sixth of the sun's diameter was obscured ; at 11.15 one-half ; and at 12 the Eclipse was total. At 12.10 it began to rain, and the sun was lost to sight until the conclusion of the eclipse, which occurred at 1.30 P.M. The eclipse was total for 70^s and the effect was very striking, the whole sky becoming of a lurid purple and very dark. The birds were hushed and frightened, and the charcoal burners' fires in the mountains, some five miles distant were plainly visible."

At Oran the weather appears to have been still more unpropitious than at the European stations. For the observations with the spectroscope, this section of the Expedition was provided with a 6-inch equatorial, of eight feet focal length, by Cooke, of York, lent by the Royal Observatory at Greenwich, to which was adapted an extremely ingenious recording spectroscope, the invention of Dr. Huggins, who himself observed with it; the management of the telescope devolving upon Captain Noble. A second smaller instrument, of 4 inches aperture, with a somewhat similar recording apparatus, was entrusted to Mr. W. Crookes, F.R.S., the Rev. F. Howlett attending to the telescope itself. The polariscopic observations were undertaken by Captain Salmond, R.E., and Lieutenant Collins, R.E.; the instrument of the former being attached to a telescope of about thirty inches focal length and $2\frac{1}{2}$ inches aperture; that of the latter to another of 6 inches aperture. General observations, sketching of the corona, &c., were to have been made by Mr. Carpenter, of Greenwich Observatory, with an exquisite 3-in. Dolland equatorial; by Mr. Hunter, with a 4-in. of rougher construction; by Lieut. Wharton, R N., of the *Urgent*, with a $2\frac{3}{4}$ achromatic, mounted temporarily equatorially; by Admiral Ommaney, with a small table telescope; by Lieut. Ommaney, R.E., with a binocular field-glass, on cards arranged for the purpose; and by the ship's officers generally, with such telescopes as they could command. The 6-in. and 4-in. equatorials were erected under a tent; the rest were employed more or less in the open air. Prof. Tyndall had established himself on a neighbouring fort, some 500 or 600 yards from the rest of the party, with a 5-in. telescope by Dallmeyer, which had been used by Mr. Dela Rue at the Total Eclipse in Spain. His observations were to be of a general character.

The atmospheric conditions in Oran in December are exceedingly like those of a very wet and windy English autumn, the day of entering into port having been the only moderately fine one experienced at all. The wind particularly was tremendous. On the 21st the observatory tent was blown down, and it was only by great exertions that the instruments themselves were saved from destruction.

The morning of the 22nd broke very doubtfully, heavy driving clouds obscuring the sun. The party were, however, all at their posts betimes, and arranged their various instruments long before the predicted instant of first contact. The clouds obscured the sun at this particular time; but soon, through a rift in them, the dark limb of the moon was seen encroaching on the sun's disc. Observation after this became very trying, as it was only possible at distant intervals to glimpse the progress of the moon, as she gradually advanced in her path. Spectroscopic observation, or even polariscopic observation, was practically impossible. Finally a glimpse was caught of the sun some eight or nine minutes before totality as a very thin crescent, and then every chink and cranny in the clouds closed up, and one dense and impenetrable pall covered the earth. Nothing remained now but to go out of the tent and observe the effect in the open air. It was certainly very striking. As the moment of totality approached, the western sky turned of an awful livid purple, the clouds over head assuming a black tint. As the shadow swept over the earth, the eastern sky became obscured, while the western horizon lighted up like the grey dawn of a dull autumn morning. At no time, however, during the totality, was it difficult to see the seconds on the face of a watch. The light was that of a cloudy evening, about an hour after sunset. One curious effect was observed, the apparent contraction or closing in of the celestial vault. Speedily, however, the dawn in the western horizon spread over the heavens; but the sun did not reappear again for at least half an hour after totality. M. Janssen, the French astronomer, who escaped, at the risk of his life, with his instruments in a balloon from Paris, was stationed on a mountain some seven miles from Oran. He can have seen nothing whatever of the phenomenon which he had braved so much to observe.

Another account from the same station states that the earlier stages of the Eclipse were seen and a photograph taken, but ten minutes before totality a dense bank of clouds came over the sun, which extinguished all hope, and rendered useless the laborious preparations of many weeks. M. Janssen was of course equally unsuccessful. Though the darkness was almost appalling as it rapidly increased up to the moment of totality, still the scene was deprived of a large part of its impressiveness by the dense mantle of cloud which concealed nearly the whole heavens.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, December 15.—Prof. Frankland, F.R.S., Vice-president, in the chair.—The following gentlemen were elected Fellows:—P. T. Atkinson, R. Koma, J. F Stark. Mr. Perkin, F.R S., read a paper "On some new Derivations of Coumarin." The author succeeded in obtaining the following new bodies:—

Dibromide of Coumarin	$C_9\ H_6\ O_2 . Br_2$
Dichloride of Coumarin	$C_9\ H_6\ O_2 . Cl_2$
α Bromocoumarin	$C_9\ H_5\ Br\ O_3$
β " "	"
α Chlorocoumarin	$C_9\ H_5\ Cl\ O_2$
β " "	"
α Dibromocoumarin	$C_9\ H_4\ Br_2\ O_2$
β " "	"
Tetrachlorocoumarin	$C_9\ H_2\ Cl_4\ O_2$
Coumarilic acid	$C_9\ H_6\ O_3$
Bromocoumaric acid	$C_9\ H_5\ Br\ O_3$
Sulphocoumaric acid	$C_9\ H_6\ O_2 . SO_3$
Disulphocoumaric acid	$C_9\ H_6\ O_2 . 2SO_3$

Dibromide of Coumarin.—A solution of coumarin in carbon disulphide is mixed with a similar solution of bromine, and the mixture, after leaving it to stand for twelve hours, allowed to evaporate spontaneously. When dibromide of coumarin is treated with an alcoholic solution of potassic iodide it becomes brown, and on evaporation deposits needles apparently consisting of a mixture of iodine and coumarin crystals, α *Bromocoumarin.* A simple process for the preparation of Bromocoumarin is to decompose the dibromide of coumarin with alkalis, the following reaction taking place:—

$$\underbrace{C_9\ H_6\ O_2\ Br_2}_{\text{Dibromide of coumarin.}} + KHO = \underbrace{C_9\ H_5\ Br\ O_2}_{\text{Bromocoumarin.}} + K\ Br + H_2\ O$$

α Bromocoumarin, when left in contact with cold alcoholic ammonia, decomposes with formation of ammonic bromide, and a non-crystalline sticky mass easy soluble in water. Heated with potassic hydrate it yields potassic bromide and a new acid. α *Dibromocoumarin.*—At a previous occasion Mr Perkin prepared this body by heating in a sealed tube to 140° C. a mixture of one part of coumarin, two parts of bromine, and four or five parts of disulphide of carbon. He since found, however, that this process is greatly improved by the addition of iodine to the mixture, as it is then only necessary to heat the sealed tube for four or five hours in a bath of salt and water to complete the reaction. The fusing point of this substance is 183° C. and not 174° C as had been previously given. β *Bromocoumarin.*—The hydride of sodium bromosalicyl when submitted to the action of acetic anhydride, yields a quantity of hydride of bromosalicyl and a body which, when crystallised from alcohol, yields colourless flat prisms, the analysis of which showed it to be monobromocoumarin, $C_9\ H_5\ Br\ O_2$. It greatly differs in properties from the bromocoumarin previously described, its fusing point being 160° C, or 50° higher, and when boiled with alcoholic or aqueous potassic hydrate it does not decompose with formation of potassic bromide, but simply dissolves like ordinary coumarin. β *Dibromocoumarin.*—On treating the hydride of sodium *di*bromosalicyl with acetic anhydrate in exactly the same manner as for the preparation of β bromocoumarin, a beautifully crystalline product is obtained of the composition $C_9 H_4 Br_2 O_2$. It is not the same body as that obtained by acting on coumarin with bromine and iodine. It fuses at 176° C., and is not decomposed by boiling with a solution of potassie hydrate. Mr. Perkin has, therefore, designated it as β dibromocoumarin. *Dichloride of coumarin*—A solution of coumarin in chloroform absorbs chlorine gas, only minute quantities of hydrochloric acid being formed. On allowing the solution to evaporate spontaneously, after the chlorine has been passed through it for an hour or two, a syrupy product is obtained very like new honey This is the dichloride of coumarin. From its products of decomposition, there can be no doubt that it possesses the formula—

$$C_9 H_6 O_2 Cl_2$$

Chlorocoumarin.—A mixture of one part of coumarin and three parts of pentachloride of phosphorus, when mixed and heated in a retort placed in an oil bath, slowly react upon each other as the temperature rises, and when the oil has reached about 200 C. the product becomes a dark brown liquid. During this reaction, a volatile liquid consisting chiefly of terchloride of phosphorus distils over. The contents of the retort after treatment with water, becomes a pasty mass of crystals, which is first purified by distillation, and then by several crystallisations from alcohol. Its analysis gave the formula $C_9 H_5 Cl O_2$. It fuses at 122°—123° C., and, when heated, possesses an agreeable aromatic odour. *Tetrachlorocoumarin.*—Chlorine gas when passed through a solution of coumarin and iodine in tetrachloride of carbon is rapidly absorbed, hydrochloric acid being evolved. If the gas be passed for two or three hours a quantity of a reddish body separates; on evaporating the product so as to separate the tetrachloride of carbon an oily residue is obtained, the red substance having fused with the impurities. On mixing this with alcohol, it soon becomes a white paste. On pressing this in a small linen bag, a white product is obtained which is further purified by being several times crystallised from spirit. The numbers of the analysis lead to the formula—

$$C_9\ {}_2H\ Cl_4\ O_3$$

It fuses at 144°—145° C. Bromocoumarin, when boiled with a solution of potassic hydrate, decomposes, yielding potassic bromide and the salt of a new acid, which has the formula $C_9 H_6 O_3$. Mr. Perkin proposes to call it *Coumarilic* acid. It fuses at 192°—193° C., distils with partial decomposition, but sublimes undecomposed when gently heated. It is monobasic, and forms well defined salts with the alkalis, the alkaline earths, lead, silver, mercury, and iron. *Bromocoumarilic acid.*—It is prepared like the above, but substituting α dibromocoumarin for bromocoumarin. It possesses the formula $C_9 H_5 Br O_3$, and fuses at 250° C. *Sulphocoumaric acid.*—It is obtained on digesting a mixture of about one part of coumarin and five parts of fuming sulphuric acid in the water bath for an hour or two. From the analysis of its salts, the formula of this acid when anhydrous is $C_9 H_6 O_2 SO_3$. Mr. Perkin has prepared ammonic, potassic, sodic, baric, and strontic sulphocoumarinates. *Disulphocoumaric acid*—On heating a mixture of about 8 parts of fuming sulphuric acid and 1 part of coumarin to a temperature of 150° or 160° C. for an hour or two, the produced will contain two sulphoacids, viz., sulphocoumaric acid and disulphocoumaric acid. Its barium salt has the formula—

$$C_9 H_4 O_2 Ba'' 2 SO_3.$$

—The next communication was by Dr. Debus. "On the formula of glyoxylic acid," Dr. Debus showed that this acid ought to be written $C_2 H_2 O_3$ and not $C_2 H_4 O_4$. He considers it in reality to be the aldehyde of oxalic acid. Among other reasons for this view he quoted its behaviour towards the bisulphites. Dr. Odling was of the same opinion. He saw the aldehydic character of glyoxylic acid in its property of combining with one atom of water or of ammonia, or ethylic chloride, &c. Mr. Perkin defended the formula $C_2 H_4 O_4$, quoting among other evidences for the correctness of his views, the behaviour of glyoxylic acid when treated with phosphoric pentabromide; instead of losing water and being converted into CO, it takes up three atoms of bromine.

TAUNTON

Somersetshire Archæological and Natural History Society, December 12 —W. E. Surtees, president, in the chair. In a paper by Mr. Cecil Smith on some rare birds found in the immediate neighbourhood of Taunton, a few observations were made on the following birds, specimens of all of which were produced. The first mentioned was the Pied Flycatcher, which had been killed in an orchard near French Weir, close by the town. Though rare in this and other southern and western counties, it was much more common in more northern counties, especially in the Lake district, where it was a summer visitant, remaining from April to November—it had, however, been taken as late as the middle of October at the Scilly Islands, when on its southern migration. The next bird mentioned was White's Thrush, of which four, or at most five, British specimens had been recorded. The specimen produced, which brought this bird within the limits to which Mr. Smith's paper was confined, had been killed at Hestercombe Wood, in January 1870; it was shot at by mistake for a woodcock. Of this bird it was observed that although it might be difficult to account for its presence in England, or even in Europe, there could be no doubt as to its identity, as this and other British killed specimens did not differ in the slightest degree from those brought over from Japan and the East. On the difficulty of accounting for its presence in England, Mr. Smith observed that the journey performed being almost entirely over land, the reason given for the appearance of many, especially of American birds on our shores, that being driven out of their usual migratory course, and out to sea by strong gales, they found no place of rest before reaching our shores, was not applicable. Neither would the supposition that it had got mixed up with flocks of other perhaps nearly allied species on what had been a common breeding ground, and accompanied them, be applicable. Still less would the theory hinted at in Yarrell that the occasional appearance of these Eastern birds in Europe is not so very strange, since as many as 114 species are enumerated by Temminck as common to Europe and Japan; but of these 114 species the great majority are inhabitants of both countries, and not wanderers from one to the other. One instance, the almost universally distributed Turnstone, was taken as an example, which, instead of wandering to such an enormous extent, appeared to be equally content, whether on the muddy shores of Somerset or at the Cape of Good Hope or in Japan. Perhaps, after all, the appearances of White's Thrush were to be attributed to a truant and vagrant disposition in the bird itself, which might lead it far from his own home. Probably the habits of the bird contributed slightly to its rarity in Europe, as it appeared to keep much out of sight in woods and plantations, and it had consequently, as in the present instance, been shot by mistake for a woodcock. The next bird mentioned was Tithy's Redstart, which was considered worthy of notice as having occurred so close to the town as Gulmington-lane. The difference between this bird and our summer visitant, the common redstart, was pointed out, and specimens of each shown. The peculiarity of this bird being a winter rather than a summer visitor to these islands, was remarked on especially, as it is a regular summer visitor to the middle and northern parts of Europe. Also its choice of localities, namely, rough, rocky situations, such as the Parson and Clerk rock at Teignmouth (at which place Mr. Smith had several times seen it in the month of November), in which it differed much from the common redstart, which preferred gardens, orchards, and hedgerows. The difference of the eggs was also remarked upon, those of the present species being white instead of the well-known blue of the common redstart. The occurrence of one specimen of the Sevin Finch, within the town of Taunton itself, brought this bird within the range of the paper, Mr. Smith observing that in the Birds of Somerset he had thrown some doubt upon this specimen, supposing it might be an escape, the time of year at which it was killed, the end of January or beginning of February, more than the rareness of the bird itself, leading to this doubt; he felt, however, bound to admit that there did not appear about the bird itself any signs of its having been in confinement. The other British specimens appear to have occurred in the summer between April and October, as might be supposed from its being an inhabitant of the south of Europe, and growing scarcer as we get farther north. The similarity to the Siskin was also mentioned, and specimens of this bird produced for the purpose of comparison, the difference in the shape of the beak being pointed out as the most reliable distinction, especially by candle light, which considerably increased the difficulty of distinguishing the colours. The Little Bittern and Baillon's Crake were also mentioned, and a specimen of each produced, both having been killed nearly in the same place, some rushy pools by the side of the river in Priory Fields. Attention was called to the similarity between the Little Bittern and its big relation, in the absence of feathers at the back of the neck. Mr. Smith added that he had seen by chance, in the shop of Mr. Petherick, the bird stuffer, a specimen of the Woodsandpiper, which had been killed by Mr. Petherick himself, on the 9th of May last, as near as Cheddar, which would bring it quite within range for mention in the paper

Manchester

Literary and Philosophical Society, November 29.—R. Angus Smith, Ph.D., F.R.S., vice-president, in the chair. Mr. R. D. Darbishire, F.G.S., exhibited a series of palæolithic instruments from the valley of the Little Ouse, and explained (after Mr. J. W. Flower, Q. J. Geolog. Soc. xxv. 449) the general features of the district and the deposit of the beds and the implements.—Mr. W. Boyd Dawkins, F.R.S., indicated the age of these deposits as related to the period of the existence of *Elephas primigenius* in the district of the south-east of England and the adjoining portions of the bed of the German Ocean and the north-west portions of France.—"The Tails of Comets, the Solar Corona, and the Aurora considered as Electric Phenomena," by Prof. Osborne Reynolds, M.A. Although the tails of comets are usually assumed to be material appendages which accompany these bodies in their flight through the heavens—and the appearance they present certainly warrants such an assumption—yet this is not the only way in which these tails may be accounted for. They may be simply an effect produced by the comet on the material through which it is passing, an effect analogous to that which we sometimes see produced by a very small insect on the surface of still water. We see a dark spot, and on looking closer we find a small fly or moth flapping its wings and creating a disturbance which was visible before the insect which produces it. There is nothing else that we can conceive their tails to be, so that they must be one or other of these two things: either (1) material appendages of the nucleus, whether the material be limited to the illuminated tail or surround the comet on all sides; (2) matter which exists independently of the comet, and on which the comet exerts such a physical influence as to render it visible. There can be no doubt that if these tails are matter moving with the comet, this matter must be endowed with properties such as we not only have no experience of, but of which we can form no conception. This alone would seem a sufficient reason for rejecting the first hypothesis. Moreover, on the second hypothesis there is no difficulty in the immense velocity with which these tails are projected from the head or whirled round when the comet is in perihelio. For to take the "negative shadow" as an illustration, here we should have a velocity of projection equal to that of light, and the only effect of the whirling would be a slight lagging in the extremity of the tail, causing curvature similar to that which actually exists. And whatever the action may be, if its velocity of emission or transmission be sufficiently great, this effect will be the same; but whether this hypothesis is to be rejected because involving assumptions beyond conception or contrary to experience, must depend on the answers to the following question: Do we know, or can we conceive, any physical state into which any substance which can be conceived to occupy the space traversed by comets could possibly be brought so as to make it present the appearance exhibited by comets? Now I think the answer must be in the affirmative, and that we may leave out the terms conceive and conceivable. For electricity is a well-known state, and gases are well-known substances; and when electricity, under certain conditions, as in Dr. Geissler's tubes, is made to traverse exceedingly rare gas, the appearance produced is similar to that of the comets' tails; the rarer this gas is, the more susceptible is it of such a state, and so far as we know there is no limit to the extent of gas that may be so illuminated. Hence we may suppose the exciting cause to be electricity, and the material on which it acts and which fills space to have the same properties as those possessed by gas. What is more, we can conceive the sun to be in such a condition as to produce that influence on this electricity which should cause the tail to occupy the direction it does. For such an electric discharge will be powerfully repelled by any body charged with similar electricity in its neighbourhood. The electricity would be discharged by the comets on account of some influence which the sun may have on them, such an influence being well within the limits of our conception. The appearances of the comet in detail, such as the emission of jets of light towards the sun and the form of the illuminated envelope, are all such as would necessarily accompany such an electrical discharge. In fact, if the possibility of such a discharge is admitted, I believe it will explain all the phenomena of comets. As to the possibility, or even the probability of such a discharge, I think it may be established on very good grounds. The tails of comets may or may not be one with their heads; but whichever is the case, it is certain that the difference in the appearance of comets and of planets indicates some essential difference either in the materials of which these bodies are respectively composed, or else in the conditions under which their materials exist. Now from the motion of comets we know that their heads follow the same laws of motion and gravitation as all other matter, and therefore we have good evidence, so far as it goes, that comets and planets are similarly constituted as regards materials. And since the appearance of a comet changes very much as it passes round the sun, any assumptions with regard to the material of comets, in order to account for their difference from planets, could not account for the variety of appearance the same comet presents at different times. On the other hand, the conditions of comets and planets must necessarily be very different, from the extreme difference in the shapes of the orbits they describe. Each planet remains nearly at a constant distance from the sun (whatever that distance may be), so that the heat or any physical effect the sun may have upon it will also be constant; on the comets its action must change rapidly from time to time, particularly when the comet is in certain parts of its orbit. Hence we may say that the temperature and general physical condition of planets is nearly constant, and that of comets for the most part continually varying. From these reasons it seems to me not only possible, but probable, that these strange visitors to our system are clothed in electrical garments with which the regular inhabitants are unacquainted. The electricity must after all depend on the composition of the comet, for known substances do not all show the same electrical properties. Hence by assuming comets to be composed of various materials, we have a source to attribute the different appearances presented by the different individuals. To the same source we may attribute the irregularity in the direction of their tails and the lateral streamers they occasionally send out. Secondly, I think this electrical hypothesis is supported by the to me seeming analogy between comets, the corona, and the aurora; an analogy which suggests that they must all be due to the same cause. They may be all described as streams of light or streamers, having their starting point more or less undefined, and traversing spaces of such extent and with such velocities as entirely to preclude the possibility of their being material in any sense of that word with which we are acquainted. The aurora has long been considered as an electric phenomenon, and recently the same effect has been produced by the discharge of electricity of very great intensity through a very rare gas, there being no limit to the space which it will thus traverse. This being so, why should not the tails of comets and the corona also be electric phenomena? Their appearance and behaviour correspond exactly with those of the aurora, and there is surely nothing very difficult in imagining the sun which is the source of so much heat being also the source of some electricity. Neither will there appear anything wonderful in the electricity of comets when we consider that of the earth. We must not look on our inability to explain the cause of such an electric discharge as fatal to its existence, for we cannot any more explain the existence of the electricity which causes the aurora. If we cannot explain from whence these electricities come, we can at least show that the conditions which are most favourable to the development of the aurora exist in much greater force on the comets than they do on the earth. The greatest development of the aurora borealis takes place at the equinoxes. There is a cessation in summer, and another in winter. Now, the equinoxes are the times when the action of the sun on our northern hemisphere is changing most rapidly. Hence the condition favourable for the aurora is change in the action of the sun. The same thing is pointed out by the diurnal variation in the electricity of the atmosphere. Now, as has been already shown, the change in temperature on the comets is incomparably greater than it is on the earth, and its variation corresponds with the variation in the splendour of the comet. Angstrom has also shown that the light from the aurora, the corona, and the zodiacal light, are all of the same character, or all give the same bright lines when viewed through the spectroscope, and that these lines correspond to the light from no known substance. This indicates that whatever this light may be, the incandescent material is the same in all cases; or may we not assume that it is the medium which fills space that is illuminated by the electric discharges? This would be supported by the fact that the light from the heads of two small comets indicated carbon, whereas that from the tails only gave a faint continuous spectrum. For an electric discharge would first illuminate the atmosphere of the comet, or even carry some of the solid material off in a state of vapour, and then pass off to the surrounding medium. Thus while the spectrum from the head would be that of cometary matter, the tail would be due to the incandescent ether. I would here suggest that gas, when rendered incandescent by electricity, may reflect light—it will certainly cast a shadow from the electric light—and if this be the case, part of the light from comets' tails may after all be reflected sunlight. At any rate, it is certain that the appearance of streamers, the rapidity of change and emission, the perfect transparency and the wave-like fluctuations which belong to these phenomena, are all exhibited by the electric brush; in fact, the electric brush will explain all these appearances which have defied all attempts at explanation on a material hypothesis. I have only to add that the main assumption involved in the electric theory is that space is occupied by matter having similar electrical properties to those of gas; and I would ask, is it not more rational to make such an assumption than it is to attribute unknown and inconceivable properties to cometary matter? Theories, even if founded only on rational speculation, often, I believe, prove very useful, inasmuch as they afford observers a definite purpose in their observations—something to look for, something to establish or to refute; and I publish these speculations of mine at this particular moment in the hope that they may perchance serve such a purpose.

PHILADELPHIA

American Philosophical Society, October 7.—Dr. Binton made some observations on a Mazahc theological work and grammar, indicating probable resemblances between the language of that nation and that of the Aztecs. He also described a grammar of the Moska nation of New Granada, prepared by Father Lugo.—J. A. Macneil described the ruins and other remains of the ancient nations, which he had discovered during several expeditions in Nicaragua, Costa Rica, Chiriqui, and Chiapas. He made especial reference to an extensive series of ruins he had discovered near the boundaries of Chiriqui and Costa Rica. One of the buildings was 600 feet in length, and 25 to 30 feet elevation. Among other sculptures he observed a well-executed stone alligator of large size.—Prof. Cope exhibited the remains of a new cretaceous tortoise, of the genus *Adocus* Cope, to be called *A. syntheticus*. He explained that he had been able to establish more fully the characters of the genus *Adocus*; that it was found to possess an intergular shield as in the Pleurodira, but had not the sutural union of the inferior pelvic elements with the plastron of that type. He said that these characters had been heretofore known as correlatives from the cretaceous period to the present day, and that this genus presented us with the first exception to the rule. The genus was therefore regarded as a generalised type, and to be elevated to the rank of a family. Prof. Cope exhibited a metatarsus of *Taelaps aquilungus*, the first known, and said it proved the distinctness of those elements from each other in that type, and their slenderness, taken collectively. The specimen was an external one, without trace of a rudimental one outside of it; that its measurement, 16 inches, was indicative of a length of 18 inches to the median metatarsus, a length he had already assigned to it on theoretical grounds. Prof. Cope read a paper entitled, "Contribution to the Ichthyology of the lesser Antilles." Two new genera were described in it, viz., *Eleutheractis* and *Cryptotomus*.

Academy of Natural Sciences, November 8.—Prof. Leidy characterised three species of extinct turtles, obtained by Prof. Hayden's party from the tertiary deposits of Wyoming. Two were named *Emys Haydeni* and *E. Jeanesi*. The third, partaking of characters of the genera *Chelydra* and *Dermatemys*, was named *Baena arenosa*. A lacertian, as large as the largest of our living Iguanas, was characterised from the greater part of the bones of a skeleton imbedded in a rock of tertiary age, from Wyoming. The vertebræ have the characteristic ball and socket-joint to the bodies, but they are devoid of the zygosphene and zygantrum. The teeth are compressed conical, slightly curved, sharp-pointed, and trenchant. The remains were referred to a species with the name of *Saniwa ensidens*. The names *Baena* and *Saniwa*, according to Prof. Hayden, are those given to the turtle and lizard by one of the aboriginal tribes of the Upper Missouri.

VIENNA

I. R. Geological Institute, December 6.—Dr. Laube presented a memoir on "the Echinidæ of the Upper Tertiary Deposits in the Austro-Hungarian Empire," which will be printed in the fifth volume of the Memoirs of the Institute. The total number of distinct species is 37, six of which belong to the lower, and 31 to the upper part of the Mediterranean formation.—M. F. Foetterle "on the Sarmatic formation in the Bukowina and Northern Moldavia." It occupies an enormous space in both countries, and is easily divided in two members, an upper formed of yellow sand and sandstone, and a lower which consists of blue clay. Both contain fossils in abundance. The clay is everywhere permeated with very thin veins and layers of fine sand, which give free access to water, and thus cause the greatest difficulties for the railroad between Czernowitz and Jassy, which for long tracts passes over the clay, and is damaged by very long continued falls of rain.—M. Th. Fuchs reported on the Fauna of the Congeria-beds of Tihany and Kup in Hungary.—M. Ch. v. Hauer communicated the analysis of fire-proof clay of Fohnsdorf (Styria) which forms there a layer 9 feet thick in the browncoal basin. It consists of a hydrosilicate of alumina and magnesia, and belongs to the so-called soap-stones.—M. E. Tietze gave notice of the discovery of fossiliferous beds belonging to the brown Jura at Boletin, in Servia, and of neocomian and turonian beds in north-eastern Servia.—M. M. Neumayr presented a memoir on the jurassic flint-limestones of the Carpathians.—M. D. Stur exhibited a magnificent collection of eocene fossils from the environs of Vicenza, which had been purchased for the Museum of the Institute.

DIARY

FRIDAY, January 6.

Geologists' Association, at 8

SATURDAY, January 7.

Royal Institution, at 3.—Burning and Unburning: Prof. Odling (juvenile lectures).

SUNDAY, January 8.

Sunday Lecture Society, at 3.30.—Malta and the Maltese, with a visit to St. Paul's Bay: Dr. Carpenter.

MONDAY, January 9.

Royal Geographical Society, at 8.30

TUESDAY, January 10.

Photographic Society, at 8.
Ethnological Society, at 8.—On the Prehistoric Remains in Brittany: Lieut. S P Oliver, R A—Exhibition of Stone Implements from Queen Charlotte's Island: Dr. Hooker, C B.—On a Cairn near Cefn, St. Asaph: Rev. D. R. Thomas, M.A., and Mr. T McK Hughes, M.A.

WEDNESDAY, January 11

Geological Society, at 8—On the older Metamorphic Rocks and Granite of Banffshire Mr. T. F. Jamieson, F G.S—On the Connection of Volcanic action with Changes of Level: Mr J. J Murphy, F G S.—On the Geology of the neighbourhood of Malaga: Don M de Orueba.
Royal Microscopical Society, at 8—On the Anatomy of *Ascaris lumbricoides*: Mr B. T. Lowne, M R C S—On the use of Colloid Silica in preparing Crystals for the Polariscope: Mr. H. J. Slack.

THURSDAY, January 12.

Royal, at 8 30
Society of Antiquaries, at 8 30.
London Mathematical Society, at 8.—On Systems of Tangents to Plane Cubic and Quartic Curves. Mr J. J. Walker.—On the Order and Singularities of the Parallel of an Algebraical Curve: Mr. S. Roberts.

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THURSDAY, JANUARY 12, 1871

THE NEW HOSPITAL OF ST. THOMAS

FEW more marvellous creations of constructive art have burst in rapidly maturing beauty upon the eye than the noble *vis-à-vis* which now faces the Palace of Westminster, and looks across the Thames at it from the opposite stretch of Embankment, rendering the site which is centred by Mr. Page's graceful bridge, one of the most remarkable that is to be found in the chief cities of the world. The Hospital may possibly be held to be subordinate to the Palace in dignity and grandeur of external form; but in two particulars it must be admitted to be in no way inferior to its rival. It is dedicated to a purpose of highest and purest beneficence, the alleviation of human suffering, and to unceasing conflict with one of the most potent of the powers of physical evil; and it is a *chef-d'œuvre* of perfection and completeness for the accomplishment of the end to which it is destined. Within thirty-one short months this vast building has been so far advanced under the hands of a staff of nearly 900 workmen, that it is now quite possible to take a comprehensive view of the purpose and plan of its designers, and fairly to contemplate in its most advanced form the idea of what a public Hospital should be in these days of scientific development and conquest.

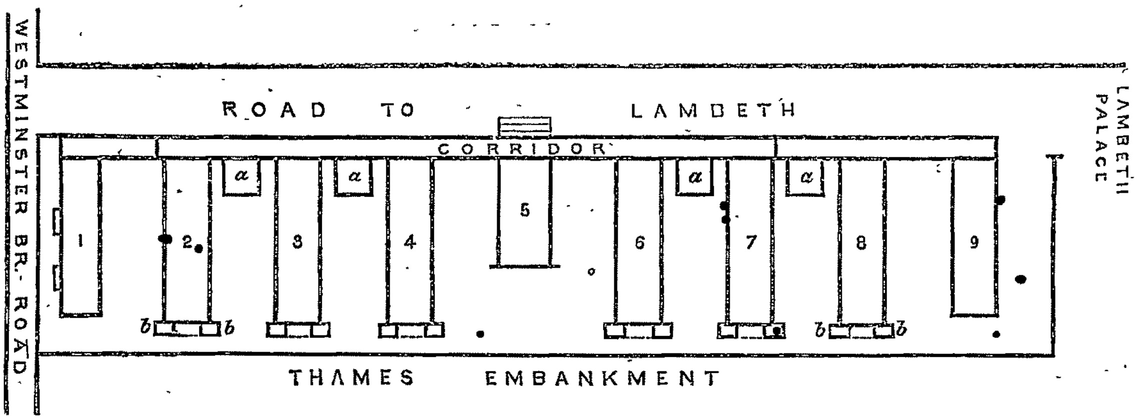
It is a matter of notoriety, that after a period of perplexity and doubt,—during which it seemed at least problematical whether the old Hospital of Edward VI., which had been ejected from its primary home near the southern end of London Bridge by the remorseless demands for increased railway access on that side of the metropolis might not be scattered into disjointed fragments for want of a sufficiently spacious central site, where its functions might be efficiently and conveniently resumed after the old fashion and upon the old scale,—it has been found practicable to give it a new habitation in Lambeth, in a position in no sense inferior in promise of direct usefulness to the one it previously occupied in the borough of Southwark, and in many particulars with a marked and unquestionable accession of advantages in the change. In accomplishing this task, it was wisely determined, after due deliberation upon all the interests involved, so to use the great opportunity as to show to the world what is required by the present conditions of sanitary and medical science in a Hospital erected for the study and cure of disease and casual injury. This, of course, could only be accomplished at some cost in the matter of money and space. But it was held that a very considerable measure of compensation might be at once effected by the adoption of very perfect organisation and very complete mechanical contrivance; and that beyond this any money outlay which establishes a model of perfection in Hospital construction and arrangements, must be admitted to be a wise and sound investment for the community on other grounds.

The most casual observer of the external aspects of this vast pile of building will at once perceive that the fundamental idea of the plan is the breaking up of the structure into a series of subordinate blocks, which must allow of the most thorough and ready permeation of fresh air to every part of the inhabited interior. The *beau ideal* of the ward of a Hospital is that it shall be a spacious room, constructed with due regard to the number of inmates it is allowed to accommodate, open on all sides to the fresh blasts of Heaven. The problem in this individual case has been to determine how several hundreds of inmates can be lodged in a building placed in a densely inhabited part of a great city, without violence being done to this fundamental necessity. In the new Hospital of St. Thomas nine distinct blocks of buildings have been spread along the immediate Embankment of the river, from the end of the bridge at Westminster to the Archbishop's Palace at Lambeth, in such a way that they have the open space overlooking the broad channel of the Thames at one side, and a roomy thoroughfare connecting the Westminster Bridge Road with Lambeth at the other. These blocks are of elongated form, their longest dimensions lying transversely to the course of the river, so that their ends look down upon the stream; their sides being severed by intermediate areas of clear space. Each block, in the main, is simply a stack of long single wards with windows at each side, placed one upon the other. But these wards are bulbous, or enlarged, at the river ends, for the sake of architectural effect, and for purposes of convenience which will be hereafter mentioned. But for almost the entire extent of their longest dimensions they are purely single long rooms pierced by spacious windows at both sides. On the ends opposite to the river these blocks are, in the lower flats, connected together by corridors contrived for the purpose of interior communication; but, throughout the upper flats the isolation of the blocks, and, therefore, the permeability to free air, is complete. Further reference will have presently to be made to the admirable way in which the work of necessary communication has been managed.

In broad outline the plan of the arrangement is, therefore, that which is presented to the eye in the sketch on the following page.

With respect to these blocks, it may be stated that No. 5 is the Central Hall, with entrance from the Lambeth Road, and the Chapel. No. 1 is the administrative block, consisting of the Governor's rooms and the Treasurer's residence, and No. 9 is the Museum and Medical School. The blocks 2, 3, 4, and 6, 7, 8, are, therefore, the Hospital proper. Each of these blocks consists of four flats, with an attic story above, and a basement story beneath. But the first floor in block 4 is appropriated to the accommodation of the linen and to the matron's use; and the corresponding floor in block 6 is absorbed by the kitchen and cooking apparatus. Block 8 is the compartment reserved apart for contagious and infectious diseases, and is differently arranged to the other Hospital blocks. There are therefore four large Hospital wards in blocks 2, 3, and 7, and three large Hospital wards in blocks 4 and 6. Each of these large wards in the three upper flats will accommodate 28 beds. The first floor wards in blocks 2, 3, and 7 are necessarily of somewhat smaller size, and are designed for 20 beds. All the patients' wards taken together, including those of block 8, and sundry small private wards scattered about the building, afford ample accommodation for six hundred indoor patients.

The principal channel of communication between the several blocks of the building is one long corridor on the ground flat or floor. This corridor runs the entire length from the administrative block (No. 1) to the block for contagious diseases (No. 8). But the portions of the corridor which lie between blocks 1 and 2, and between blocks 7 and 8, take the form of an open colonnade. For the rest of the distance, it is intrinsically an internal passage. The open, or colonnade, portion which leads to block 8, the assigned seat of infectious disorders, is carefully cut off from the rest of the corridor by closed glass doors, so that all contamination of the other blocks of the building by the infected air is simply impossible. The open air-space which intervenes is ample for the neutralisation and destruction of atmospheric infection of any kind. The marvellous extent of space covered by this hospital is perhaps best estimated by stating the actual length of this corridor. The continuous length of the spacious passage is 916 feet from end to end. A very pleasant and convenient communication between the several blocks is effected on the second floor by a casemented passage, which runs along the main corridor. The communication for the third floor is along the open flat roof of this casemented passage; and above this there is no communication at all between the blocks. The effect of the light and airy outlook, giving the impression of altogether unrestricted lightness and freshness, which is encountered in passing along these higher passages of communication, is very charming and agreeable. There is scarcely anything in the arrangements of the buildings which is more striking and pleasant to an observer upon a first visit. The open passage at the top is guarded by a balustrade, which is very profusely ornamented by large urns made of artificial stone; a material which has been largely employed in the ornamental parts of the structure. This compound, which is a special patent, is formed of dissolved flint, mingled with sand, the material being then saturated with silicate of potash under exhaustion or pressure. It is expected that this artificial stone will possess very enduring qualities, but from the present aspect of these urns the writer of these lines inclines to think that the material yet needs further evidence of endurance and success before it can be held to have established the character at which it aims.



In addition to these corridors of communication, there is a still longer passage in the basement, extending quite from the administrative block to the Museum and Schools at the farther end of the structure, and giving immediate access to the department for washing linen, and to the Anatomical Schools and mortuary receptacles which lie beyond under the shadow of the old walls of Lambeth. There is also a sunken but open-air way running from end to end of the building immediately within the parapet trenching upon the river-embankment, which gives still further facility for the transport of heavy material. This channel of communication is very ingeniously and completely masked from observation both from the building itself and from the external space.

The important and interesting details relating to the arrangements which have been made within the large wards themselves to fit them for their beneficent work, must be reserved for another article.

R. J. M.

THE COLLECTION OF INVERTEBRATE ANIMALS IN THE FREE PUBLIC MUSEUM, LIVERPOOL

IN October 1861, when the Natural History collections presented to the town of Liverpool by the grandfather of the present Earl of Derby were removed from Duke Street to the building which they now occupy, the question arose, how should the museum be made as fully as possible to answer the requirements of the population by whom it was to be supported under the provisions of the Library and Museum Act.

The Curator, Mr. Moore, whose invaluable services are too well known to require further notice on my part, having on his hands, besides the duties of general superintendence, the re-arrangement of the extensive series of Mammalia and Birds, together with preparations for the reception of a similar series of Fishes and Reptiles, availed himself of my offer of assistance in obtaining and arranging a collection of Invertebrate animals, our stock of which at that time included little beyond some corals and a few very miscellaneous specimens.

The accommodation available for the proposed collection consisted of the central areas of a suite of five rooms

27 feet in breadth, the total length being 250 feet. Space was thus provided for eighteen table-cases, each 10 feet long, set transversely. One important point was therefore settled by the shape of the building. The series had to be conformed to a linear arrangement. In some respects this was a serious disadvantage. The classes of Invertebrate animals cannot well be represented in a single ascending or descending series. Probably it would not be possible on any symmetrical plan to indicate their proper positions relatively to each other; but some palpable incongruities might be avoided by the use of table-cases on a ground-plan, resembling in form a tuning-fork. The Protozoa, as the stem, pass naturally enough on one side by the Rotiferæ and their allies to the Annelida, Echinodermata, Crustacea, and Insecta; and on the other by the Cœlenterata to the Molluscoida and Mollusca; the greater size of the specimens forming the latter prong of the fork, compensating for the vastly more numerous species in the former. The importance of a suitable ground-plan for cases in Museums seems to be much underrated. When a class of students visit a museum frequently, the localities of cases containing special groups become indelibly impressed upon the memory. Why should not this be turned to good account?

In preparing the first scheme of the collection, it seemed to me essential that plain, and moderately simple, printed descriptions of the life history of the animals should accompany the specimens, but, as it was clearly impossible to describe every species, or even every genus, it became necessary to fix on some mode of associating in groups a number of species to which the descriptions might apply. Such divisions as "classes" and "orders" were manifestly too large; whilst "families" varied from a single genus, including a solitary species, to an army of more than a thousand genera, *e.g.* the Cerambycidæ and Curculionidæ in the Coleoptera. It was with some regret that the idea of attaching a readable sketch to each division of a given rank in recent systems of classification was relinquished, but it was found to be impracticable; and the life history sketch thus became the foundation of the system eventually adopted. Whether it might be a few species, or a genus, or a family, or an order, that seemed to afford suitable scope for a paragraph of readable and instructive matter, it was decided that such a group should be segregated, so as to form the unit of the series. Eventually, in order that the sketches, which it was proposed to print for the purpose on tablets, might all be in positions where they could conveniently be read, it was found to be expedient that each group, or unit, should occupy an equal space; and as the blocks on which the table-cases rested were to be fitted up with trays or drawers, twelve of which would occupy the table-case without loss of room, these trays or drawers were adapted as the receptacles and boundaries of the groups.

The drawers measured twenty-seven inches in length by sixteen inches in breadth, and their number in the eighteen table-cases, when completed, would be 216. Then arose the problem, how best to divide the twenty classes of Invertebrate animals into 216 groups, each of which should be capable of affording materials for a biological notice, such as might be read with interest by any intelligent visitor.

The entire plan of the table-cases, and the limits of most of the groups, were committed to writing before any considerable advance had been made in procuring specimens. In one respect this circumstance was found to be very advantageous—our *desiderata* were at once well-defined. It was an object that each of the groups should be illustrated by carefully selected specimens, and until this could be attained, other acquisitions need not be sought for. In making purchases, such an object steadily kept in view exercises a powerful influence against the seductive attractions of "great bargains," which often turn out to be great misfortunes to a Museum. Moreover, in accepting donations, it is sometimes convenient to be able to refer to a fixed plan. Where room is scanty, as in most Museums, nothing is more subversive of order, or more fatal to an instructive arrangement, than the gift of a collection, coupled with a stipulation that it must be displayed in some special way. It is far better to forego the possession even of a valuable series of specimens, than to sacrifice order for their sake.

The number of groups, 216, will, no doubt, appear to have been determined simply as a matter of convenience. To a certain extent this is true. After a careful reference to the best available authorities on each of the Invertebrate classes, in which much assistance was afforded me by the many valuable scientific works in the Free Public Library of Liverpool, and by the catalogues of the collections in the British Museum, it seemed probable that most of the prominent forms in all the classes might be exhibited in pairs, with their names attached in very legible type, in an area less than a thousand square feet; and that they might appropriately be disposed in 240 groups, occupying twenty table-cases. For these, the suite of five rooms above referred to would have been sufficient, but two large circular stoves occupied the room of two table-cases, and the groups had to be reduced to 216 in number, instead of 240, as in the original design.

For constant exhibition to the public, the series may perhaps be regarded as quite sufficiently extensive. Four table-cases contain the Protozoa and the Cœlenterata. Seven are given to the Molluscoida and Mollusca, in which department the collection includes representatives of about eleven hundred out of the thirteen hundred genera and sub genera adopted by Messrs. H. and A. Adams in their work on "The Genera of recent Mollusca." Three cases are occupied by Echinodermata, Annelida, and Crustacea. This is by no means in proportion to the other parts of the series, and here it is that the want of the two absent table-cases is most felt. Four cases hold the Arachnida, Myriapoda, and Insecta, in which all the orders are fairly illustrated, except Strepsiptera. Stylops has not yet arrived—perhaps this may meet the eye of some friend who, for love, money, or specimens, may be willing to supply the deficiency.

It is hardly necessary for me to point out the difficulties and disadvantages which must attend an attempt to form a collection in which the whole of the Invertebrate classes are divided into a given number of equal groups. If all very distinct forms are to be exhibited, some groups must be heterogeneous in composition, but not necessarily very many. Such forms as Pycnogonum, Forficula, Siphonaria, Sagitta, Cydippe, &c., may have to appear as interlopers; but the printed tablet may explain the irregularity of their position, and render the disadvantage simply a negative one,—in such cases the plan cannot afford much help to the memory. On the other hand, the tray or drawer containing an entire group can, with the utmost facility, be moved, to be re-arranged, to illustrate a lecture, or to occupy a different position in the series.

In the present unsatisfactory condition of "classification," probably the only thoroughly scientific mode of conveying information respecting an assemblage of organic forms, is that adopted by Professor Huxley, Professor Rolleston, and others, of describing completely a single included species; but this method seems more suited for students than for a mixed company, such as have visited our institution since October 1861, during which period the admissions to the Liverpool Museum have exceeded four millions one hundred and sixty-two thousands. The mode of arrangement adopted within the groups will be described in a subsequent notice.

HENRY H. HIGGINS

UTILISATION OF SEWAGE

A Digest of Facts relating to the Treatment and Utilisation of Sewage. By W. H. Corfield, M.A., M.B. Oxon, Professor of Hygiene and Public Health at University College, London. Prepared for the Committee of the British Association. (London: Macmillan & Co. 1870.)

DR. CORFIELD, now the Professor of Hygiene and Public Health in University College, London, after having been a most distinguished student in the old University of Oxford, has put before the world in a well and large printed volume of something less than 300 pages, a clear, readable, and reliable *résumé* of the "Great Sewage Question." The labour which has been thus expended in lightening the labours of others can be adequately judged of by but few persons; but amongst those few may perhaps be reckoned individuals who, like the writer of this review, have for their sins or through their foolishness, been entrapped into serving on the drainage committees of Local Boards, and have felt themselves compelled, in the way of expiation, to purchase, if not to peruse, the hydra, or rather the medusa-brood of blue books which parliamentary commissions and privy council offices are so constantly giving off. Had Professor Corfield always given chapter and verse, page and paragraph, for his citations from the vast number of volumes to which we allude and he has referred, he would have put his claim to credit on the score of painstaking laboriousness more prominently before the eyes of his readers, though he might not thereby have made the reading of his work much the easier for them. As it stands, his book is eminently easy of comprehension, and we will, without further preface, say a few words as to the general outlines of the ground he professes to cover in it.

The first 103 of the 282 pages of which the book consists, are taken up by an account, which is partly archæological, and partly, we regret to say, as yet not so, of certain systems for dealing with refuse which all alike labour under an amenability to an objection which our author, like ourselves, would appear to judge to be fatal to them. This objection he thus states (pp. 59, 60)—"The question, in fact, to be solved would appear to be with all the methods which require hand and cart labour: how can the refuse matters be kept as long as possible without being positively dangerous to health? instead of, as it should be, how can they be got rid of as fast as possible? This consideration at once stamps all methods of removal by scavenging, and must of itself bind them to a false principle, and lead to their condemnation." They are rightly, we would suggest, called systems of Conservancy, professing, as they do, to *keep* something awhile, which it would be better to lose at once. This portion of the professor's book is closed with a quotation from the "First Report of the Rivers' Pollution Commissioners" relating to one of those methods which at the present moment would appear to enjoy a considerable popularity; and this quotation we will follow his example in reproducing, observing by the way that to the words "First Report of the Rivers' Pollution Commissioners," there should have been added the words, "appointed in 1868, published in 1870, p. 50," to save readers the trouble of referring to another Blue Book put out by another set of Commissioners appointed in 1865. The quotation is to the following effect:—"Add to those circumstances the enormous aggravation of all the difficulties of the plan, when not 50 but 5000 households have to be provided with the necessary appliances, and are induced to work them properly, and we can have no hesitation in pronouncing the dry earth system, if superior for institutions, villages, and camps, where personal or official regulations can be enforced, entirely unfitted to the circumstances of large towns."

With his fifth chapter, p. 104, Dr. Corfield begins the history of the particular sanitary apparatus which is known on the continent as the "*Cabinet Anglais*," and with the various modifications, applications, and bearings, agricultural and hygienic, of the means for the water carriage of refuse, the rest of the book is filled up. Prof. Corfield is something of a physicist and of a chemist, and, thirdly, of a biologist; and it is to be expected, and will be found, that he is not ignorant, firstly, that water-carriage is the cheapest of all modes of carriage; secondly, that ammoniacal gas is dissolved in, and most tenaciously held fast by when dissolved in, one-thousandth of its own volume of water; and that, thirdly, this same chemical element, "the valuable constituent of sewage *par excellence*," can, when thus carried to land bearing crops, be taken up by them and used by them in their synthesis of albuminates for us animals. The obvious corollary of these rudimentary truths is the acceptance of the principle of the disposal of sewage by irrigation, to the rejection, except under exceptional conditions, of all others; and this corollary our author thus states for us (p. 176), "All other systems than that of removal by water go upon the principle that it is not dangerous to leave excretal matters, either in a crude state (pail closets) or mixed with some absorbing or deodorising material (various other forms of closet) for a certain time in or about houses. The fundamental principle being obviously a wrong one, it is not to be wondered at that such systems continually fail. . . . The water carriage system, on the contrary, sends all the refuse matters at once to a distance in the cheapest manner possible by the mere action of gravity. . . . Figures are stubborn things to deal with, and the sanitary benefits already attained by this system are so astonishing that we have a right to demand from those who would supplant it, proof of much better results of some other method, and not mere doubtful probabilities."[*]

In this connection we must note the omission from this valuable digest of any mention of Mr. Baldwin Latham's newly-invented, most simple, and most efficacious machinery for straining off, or rather for dredging out, of the entire mass of the sewage of a town those more coarsely divided, which are also the most grossly offensive and the least useful, of excretory products, together with the floating non-excremental rubbish, such as corks and other "properties" of the complex compound in question. Prof. Corfield, as a disciple of the late Dr. Daubeny, to whose work he refers with a very proper reverence, is too good a botanist not to be aware how hurtful it is to the vital operations of plants, in disinfecting and rebuilding up decaying organic compounds, for them to have their leaves and stems besmeared over with adherent viscosity; this non-transpirable envelope being as really injurious to them as the sight of coherent masses of filth is æsthetically disgustful to us; and both these difficulties Mr. Latham's invention has removed. Croydon, we learn from the guide-books, is not inaccessible from London; and "possesses," to quote further from the same authorities, "many objects of interest for the intelligent visitor." We trust Dr. Corfield will follow the example of the health officer of Bombay, who, in a sanitary tour, the results of which are given us in the "Report of the Measures adopted for Sanitary Improvements in India, from June 1869 to June 1870," visited Croydon, and has reported (p. 232) most favourably upon the particular "object of interest" in question, and its successful coadaptation of the turbine of the dredging machine and of the Archimedean screw.

With a few disjointed remarks we will conclude our notice of this useful work. Dr. Corfield, like most men who can calculate, is an adherent of the "separate system" as regards the rainfall and the sewage proper. Mr. Menzies' name, however, has somehow or other slipped out of his pages (159, 160), where he treats of this improvement on the older plans for sewage. We feel ouselves bound in our rate-paying capacity, to say nothing of any other, to emphasise the name of the Windsor sanitarian, knowing how much his writings have saved us in brickwork.

It is half amusing, half melancholy, to have to note how preachers of Hygiene have, like preachers of higher things, to "become all things to all men, that by any means they may save some." If Dr. Buchanan (see p. 170) is quoted in one place as telling us in a single sentence what another sanitarian professes to tell us in a whole volume (*Der Einfluss der Wohnung auf die Sittlichkeit*), viz., "that the progress made by the inhabitants of certain towns inspected, in decency, cleanliness, self-respect, and morality, was at least as striking as the improvement in their health measured by the mortality returns;" we find at another the preacher of Hygiene (see p. 25) continually pointing out that sickness is the chief cause of the non-payment of rent, and appealing to witness after witness, who assure him in voices trembling with pathos, and in language worthy of such sentiment, that "rent is the *best got* from healthy houses."

Professor Corfield's book will, we doubt not, shortly appear in a second edition. In this second edition we shall hope to find a bibliography such as that which Varrentrapp has appended to his work "Ueber Entwasserung der Stadte." In this bibliography we shall hope to find Pettenkofer's papers duly and chronologically catalogued, it being the bounden duty of all sanitarians to help in propelling the sphere of Munich Hygiene out of the penumbra in which it is at present into full sunlight. To his bibliography our author will do well to superadd an index, and in correlation with his index should be, from page to page of his text, references given to the pages of the memoirs he quotes. It is not everybody who possesses the often Brobdignagian Blue Books of sanitary commissioners; but those who do possess them like to have these references, and those who do not, will not be much harassed by their insertion. We have suggested the making of these additions to the end of this book; they will cost their writer much, and save his readers some trouble if carried out. We will suggest the making of an addition to the beginning of his book, and that in the shape of a motto which was suggested to ourselves by one of the first of living scholars as an appropriate one for the Thames Conservancy. It may be found some seventy lines short of the end of Hesiod's "Works and Days," and runs thus:—

Μηδέ ποτ' ἐν προχοῇ ποταμῶν ἅλαδε προρεόντων
Μηδ' ἐπὶ κρηνάων οὐρεῖν, μάλα δ' ἐξαλέασθαι,
Μηδ' ἐναποψύχειν·[†] τὸ γὰρ οὔτοι λώϊόν ἐστιν.

OUR BOOK SHELF

Choice Stove and Greenhouse Plants Vol. II. By B. S. Williams. (London: Williams, 1870.)

PURELY horticultural works are somewhat out of the range of this journal, but in the present book, besides the usual practical cultural instructions, the author has thrown in much valuable information on the native habitats and uses of the plants enumerated. The palms, being a large order and such general favourites, occupy a large portion of the book, and the following may be taken as an example of its style:—

"*Borassus flabelliformis.* Of this noble palm, a native poem, in describing its beneficial properties, records nearly one thousand uses to which its products may be applied. It is a gigantic tree, reaching eighty feet or more in height, and two feet in diameter; the leaves are nearly circular, and plaited like a partially open fan, and have about seventy ribs, which radiate from a common centre. As young plants (which are the only specimens of this genus existing in this country), they are exceedingly handsome, but they are very rare and of very slow growth. The sap produces a very intoxicating toddy, from which sugar of superior quality is made and imported into this country, while its leaves are used for making hats, baskets and mats, umbrellas, fans, bags, and also in the manufacture of a very nice kind of matting for floors, as well as for thatching, &c. It is found principally near the sea, on low-lying sandy tracts, widely distributed throughout Asia." An excellent but very much condensed chapter on "Palms and their Uses" is likewise appended.

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[*] See further, p. 226 to p. 282, in which pages the detailed facts are given as verifiable at many places, from Edinburgh, where irrigation has been practised for a couple of centuries, to Milan, and in which we are told that the famous chemist, Dumas, whilst inspecting the well-known Barking Farm, exclaimed, "oui, l'eau doit être la charette de l'engrais."

[†] The word ἐναποψύχειν is not a common one. Its meaning is well given by Mr. Simon in his last Report (Twelfth Privy Council, Medical Department, 1870), p. 16, *à propos* of the habits of the "polite natives" of Wakefield.

We avail ourselves of the opportunity of noticing this book, because, while it is intended to circulate chiefly amongst gardeners, both professional and amateur, it seeks to convey such information on the real value of the plants, which, we think, should in all cases be a point in a gardener's education. J. R. J.

A Cyclopædia of Quantitative Chemical Analysis. By Frank H. Storer, A.M., Professor of General and Analytical Chemistry in the Massachussetts Institute of Technology. Part I., pp. 112. (Boston and Cambridge: Lever, Francis, and Co. London. E. and F. N. Spon, Charing Cross, 1870).

THIS book is a compilation of all the known methods of quantitative analysis. The processes and necessary apparatus are minutely detailed, the descriptions being reproduced from the various handbooks of chemistry and from the original memoirs. The labour entailed by such a work must necessarily have been very great, and its value is much increased by the numerous references to the original descriptions of the processes. This part extends as far as the article on carbonate of silver, from which some notion of the extent of the whole work may be obtained. The principles on which the analytical methods depend are shortly stated in each article, and under these headings are described the methods employed, and the precautions to be observed, the whole being arranged in separate paragraphs for facility of reference. This work promises to be very useful as a book of reference, and will enable the analyst to select without much labour the process most suitable to the work in which he is engaged. We recommend this book to the attention of analytical chemists, being convinced that it will be found to contain much valuable information in a very convenient form.

A Series of Chemical Problems for Use in Colleges and Schools, adapted for the Preparation of Students for the Government Science and Society of Arts' Examinations. By T. E. Thorpe, Ph.D., Professor of Chemistry in Anderson's University, Glasgow. With a Preface by Prof. Roscoe. Pp. 67. (London: Macmillan and Co.; Manchester: J. Galt and Co., 1870.)

THIS little book contains a number of useful tables and descriptions of the modes of calculation made use of in chemical science, illustrated by examples. Each section is followed by a series of questions, for the most part original, but some of which are selected from the examination papers of the Science and Art department and from the Owens College calendar. The subjects treated are Weights and Measures, Thermometric Scales, Correction of Volumes of Gases, Specific Gravity, Percentage Composition, Quantities of Reagents necessary to form certain Products, Combination and Decomposition of Gaseous Bodies, Determination of Atomic Weights, Calculation of Empirical Formulæ, and of the Results of Analysis, Specific Heat, Latent Heat, and Calorific Power. The collection of questions will doubtless be useful to students preparing for examination, and to teachers endeavouring to familiarise their pupils with the details of chemical investigation.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Professor Max Müller and the Insulation of St. Michael's Mount, Cornwall

THOUGH very much gratified at the fact that something from my pen has prompted Prof. Max Muller to give us another "Chip from his German Workshop," I was by no means prepared for the mode of treating materials which he has adopted in the Chip to which I refer—his paper on the "Insulation of St. Michael's Mount."[*]

As the author states, I read a paper to the British Association in 1865, at Birmingham (not Manchester as he supposes), and in April 1867, delivered a Friday evening lecture at the Royal Institution; each having the same title as his paper just mentioned.

With the exception of fifteen lines in the Report of the Association, the first was never printed either *in extenso* or in abstract. I conclude from the Professor's paper, however, that he saw a notice or report of it in some newspaper or journal; but, if so, I can only say that it was neither written nor corrected by me, nor with my knowledge, and that I decline to be responsible for it.

The lecture in 1867 was delivered from very brief notes, but an abstract of it was subsequently written by myself for the "Proceedings of the Royal Institution," and, printer's errors excepted, contained my opinions on the question.

It is clear from Prof. Max Muller's paper that a copy of this abstract was in his possession when he wrote his article. Indeed, the "short account" of the Mount which he "quotes" from me is from it, and not from the paper of 1865. Though substantially correct, this quotation contains three errors which may as well be set right in passing. On page 330 "very high water" and "very low water" (lines 9 and 10) should be "every high water" and "every low water," and "the total isthmus" (line 13) should be the "tidal isthmus".

In the paper of 1865 the following points were assumed:—(1) that the old Cornish name of the Mount was "Cara clowse in Cowse;" (2) that it had been correctly translated as the "hoar rock in the wood;" (3) that the name was appropriate when given; and (4, on the authority of Dr. Boase and Dr. T. F. Barham) that Florence of Worcester expressly stated that "the Mount was formerly five or six miles from the sea, and enclosed with a very thick wood." Though fully aware that each of these points might be open to question, I supposed them to have, at least, a fair amount of evidence in their favour, and hence concluded that the insulation of the Mount had taken place since the introduction of the old Cornish language into the district. Now, such insulation must have been the result of the encroachment of the sea merely, or of a more or less general subsidence; and my object was to show that it was the latter. In order to do this I attempted to dispose of the first hypothesis—insulation by encroachment without subsidence. A careful personal investigation of the Mount and the mainland, and the evidence of an old intelligent native, led me to the conclusion that to take the average retrocession of the cliff at ten feet in a century would probably be an excessively high estimate, and that, even at this rate of waste, "the hypothesis of insulation by encroachment only, appeared to demand the belief that at least twenty thousand years ago Cornwall was inhabited by men who spoke a language which prevailed in the same district to within a very few centuries of our own time, and which, from its similarity to the Welsh, might be said to be spoken still by a large population within our own island." Believing this conclusion respecting the antiquity of the Cornish language to be utterly untenable, I at once rejected it, and, with it, the hypothesis of insulation by encroachment merely, remarking of it that it "squandered time most lavishly."

I am, of course, delighted to find myself supported by Prof. Max Muller in the rejection of this vast antiquity of the Cornish language, for he tells us (p. 364) that it "would completely revolutionise our received views as to the early history of language." It is strange, however, and probably only to be accounted for by his trusting to a newspaper report of my paper, that he supposes that, instead of rejecting it, I have "adduced evidence in support" of this great antiquity (p. 354). The point of my argument was that the hypothesis of "insulation by encroachment without subsidence" could not be admitted, *because it led to an untenable philological conclusion.*

Turning next to the hypothesis of insulation through subsidence—the only alternative consistent with the assumptions made at the beginning—I proceeded to show that the numerous submerged forests which skirted the western coasts of England, and of which a good example in the Mount's Bay had been described by Dr. Bouse in 1822,[†] were to the geologist sufficient and satisfactory proof of a general subsidence of the country; and then pointed out that whilst, on the one hand, this change of level could not have occurred within the last 1,900 years, since, about 9 B.C., the Mount was described by Diodorus Siculus in terms which apply admirably to it at present; on the other hand, it could not have taken place in times geologically remote, since the forests consisted of plants still indigenous to the district, and contained remains of beetles retaining all their beautiful colours, as well as the horns of the red deer, which man had fashioned into tools; that, in short, there was nothing compelling the belief that the subsidence happened very much before the time of Diodorus.

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[*] "Chips from a German Workshop," vol. iii. pp. 336—357 (1870).

[†] Trans. Roy. Geol. Soc. of Cornwall, vol. ii. p. 129 *et seq.*

The paper concluded thus:—"A careful consideration of all the facts of the case, as well as of the related phenomena, points decidedly to the conclusion that, since Cornwall was inhabited by a race speaking the old British language, St. Michael's Mount was a hoar rock in a wood, and that its insulation resulted from a general subsidence of the country."

From the foregoing sketch it is obvious that, at that time, I supposed the subsidence to have taken place not much more than 2,000 years ago, and this was well understood by Sir C. Lyell and others, who took part in the discussion on the paper. Indeed, the eminent geologist just named, to whom I soon after sent the manuscript, after speaking of the attention I had given to the question (Principles, vol. i. p. 543, 1867), adds, "It is a somewhat forced hypothesis to assume that, whereas a retrospect of nineteen centuries displays to us the Mount geographically the same as it is now, yet shortly before that time, when Cornish was spoken, there was a sinking down and submergence of a wooded-tract;" thus stating his dissent from the view which, as he knew, I then held.

After reading the paper of 1865, I devoted considerable attention to the literature of the subject, and in the lecture of 186 stated that the tradition of the Mount having been five or six mile from the sea, and enclosed in a very thick wood, was first mentioned, not by *Florence* of Worcester, who died in 1118, and who nowhere alluded to the Mount, but by *William* of Worcester, who visited Cornwall about 1478, or 360 years further from th period to which the tradition was supposed to point, thus rene dering the tradition itself of very much less value; that the alleged old Cornish name assumed so many forms, and there was so much uncertainty about its exact import, as to render it improbable that it had any value as evidence; and that the submerged forest in Mount's Bay was known much earlier than I had supposed, having been mentioned by Leland, 1533-40.

The object of the lecture was to show that there had been a general subsidence of the country, that this was prior to the time of Diodorus, and that the era of the cavern deposits in South Devon was much earlier still. In fact, the insulation of the Mount, which was held to synchronise with this subsidence, was used as the first, or most modern, of a series of stepping stones leading backwards towards the era of the ancient Cave-men of Devon.

The printed abstract of the lecture closes with a recapitulation, which contains the following passage:—"Nineteen centuries ago it (the Mount) possessed a safe harbour, so that its insulation must have been effected long before; it was at one time unquestionably a hoar rock in a wood, but in all probability it had ceased to be so long before any language now known to scholars was spoken in the district. Prior to its insulation was the era of the growth of the forests now *submerged* along our entire seaboard," &c.

I am not quite sure to what Prof. Max Müller refers when, speaking of this lecture, he says, "Mr. Pengelly has somewhat modified his opinion" (p. 333). If to the opinion that the insulation of the Mount was due to subsidence, he is unquestionably in error, as I have never wavered on this point. If to that of the old British language having been spoken in Cornwall 20,000 years ago, my reply has already been given—"I never held it." But if it be to the opinion that there was a fair amount of evidence in favour of the traditions of the enclosure of the Mount in a thick wood, and of its alleged old British name; instead of modifying, I had discarded it in 1867, and with it, as a matter of course, the necessity of believing, on the one hand, that the Cornish language must have had an antiquity of 20,000 years; or, on the other, that the subsidence took place but little more than 2,000 years ago. It is unnecessary to say how very much I am gratified at finding the traditions discarded also by Prof. Max Müller (see p. 355 *et seq.*)

Before concluding, I may state that in July 1867, I read a paper to the Devonshire Association, under the title of "The Antiquity of Man in the South-west of England," which was simply an amplification of the lecture of the previous April, and was printed *in extenso* in the following October.[*] It contained a few points of interest which had come to my knowledge after the Royal Institution lecture was delivered; such as the fact that the earliest mention of a British name was made, not by Carew in 1602, but by Norden in 1584 and Camden in 1586, who concurred in giving it as "Careg Cowse," which the first rendered the *Grey rock*, and the second *Rupis cana;* the fact that the name occurred in two different forms in Carew; and the fact that there was some error in William of Worcester's statement about Pope Gregory's grant to the Church on the Mount in the year 1070, there being no Pope Gregory at that time.

It is not my intention at present to enter on a consideration of the question, "Have geologists," as Professor Max Muller supposes, "left it doubtful whether the insulation of the Mount was due to the washing of the sea-shore, or to a general subsidence of the country?" or, "May not the Mount have always been that kind of half-island which it certainly was 2,000 years ago?" My object is simply that of correcting an error into which the Professor has fallen respecting my opinion, apparently in consequence of using an anonymous and probably incorrect report of a paper read in 1865, instead of an authorised abstract of a lecture delivered in 1867,—an error, however, which can scarcely be regretted, since to it we owe a "Chip" of great interest and value.

W. PENGELLY

Lamorna, Torquay, Dec. 26, 1870

Glycerine Solutions of Pepsin and other Substances

IN NATURE of December 29, Prof. M. Foster calls attention to the method of making glycerine extract of pepsin pursued by Von Wittich, and remarks with reason that the means hitherto adopted for preparing pepsin for medical purposes are clumsy and inefficient. There is, however, one exception, a mode of preparation which has long been in use, and which is by no means inefficient. This will be found to possess some practical advantages over the process of extracting the fresh mucous membrane with glycerine, while from it the glycerine solution can be prepared quite as pure and clear, and as strong as by maceration.

As long ago as 1858 (Archives of Medicine, vol. i. pp. 269-316) I described a method of obtaining the active digestive material from the pig's stomach, which answers perfectly, and has been employed in practice ever since. It simply consists in quickly drying the mucus expressed from the stomach glands upon glass plates.[*] The dried mucus is then powdered and kept in stoppered bottles. It retains its properties for years. Eight-tenths of a grain will dissolve *one hundred grains* of coagulated white of egg.

Now, from this powder is easily prepared by solution in distilled water a perfectly clear and colourless digestive fluid of great activity, which *can be readily filtered.*

Some years ago I found great advantage from subjecting tissues to the action of a very small quantity of this solution in glycerine, and keeping the whole at the temperature of 100° for some hours. By this process the elements of the tissue were softened, and could be dissected from one another readily for examination under the highest magnifying powers.

No doubt there is much to be learnt concerning the nature of the action of such substances upon tissues by the use of glycerine solutions. For microscopical work glycerine is of more use than any other medium. Not only may various substances be removed from tissues, but others may be introduced, and the tissue subjected to the action of various reagents without destroying it. In fact, the action may be regulated with the greatest nicety. Nearly all the tests required in microscopical examination may be dissolved in glycerine ("How to Work with the Microscope," p. 297, 1867) and tissues of the most delicate character may be preserved in it, and will retain their microscopic characters for years, *if care be taken to obtain the best and strongest glycerine.*

LIONEL S. BEALE

Tails of Comets, Solar Corona, and Aurora

UNDER this heading, in your issue of 5th inst., you report a paper by Prof. Osborne Reynolds, M.A., read at a meeting of the Manchester Literary and Philosophical Society, Nov. 29 last. This paper sets forth that the tails of comets, the solar corona, the aurora, and the Zodiacal lights are due to the ether which "fills" space. Comets' tails, as stated by the Professor, in his paper, are an effect due to the medium through which it passes being heated and illuminated by the comet; and that the other phenomena are also due to the ether.

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[*] Trans. Devon Assoc., vol. ii. pp. 149—161. 1867.

[*] This Pepsin is prepared for medical purposes by Messrs. Bullock and Reynolds, 3, Hanover Street, Hanover Square.

I rely that you will do me the justice to allow space for me to remind your readers that the theories which Prof. Reynolds so emphatically calls *his*, were propounded and published by me sixteen years ago, and noticed by most of the press throughout the world.

The following quotation from my pamphlet (now out of print) will enable your readers to judge of the correctness of these statements:—

"It was noticed, in reference to 'Biela's comets,' that the smaller one, which I call the *tertiary* comet, and which travelled in a separate or distinct orbit, that a kind of *rein*, or stream of light, joined the two heads, the stream of light being *larger* as it approached the head of the *larger comet*. This stream of light, I think, proves most satisfactorily that the 'tails' of comets are nothing more than the illuminated or heated *medium* through which the comets pass. The *cylindrical* appearance which these 'tails' sometimes present, I believe is occasioned by the *rotary motion* of the comets (or infant worlds) on their respective axes. *The increased number of 'tails'* to a comet is occasioned by the interposition of one or more secondary or tertiary comets, intercepting the 'tail' of the superior body, the direction of which 'tails' would be determined by the position of the said intercepting bodies, and the variety of *appearance* in connection with the point from which they were viewed," &c.

Bayswater, Jan. 9 J. BEDFORD, Ph.D.

The Artificial Introduction of Plants

THE remarks on page 142 of your number of December 22, on the proposal of the Manchester Field Club to introduce plants foreign to the district, ought to be printed in red letters. The geographical distribution of plants is not the least interesting branch of botanical study, and is, besides, important in its bearings upon other natural sciences, such as geology and meteorology, and as such has formed the subject of laborious and intelligent research among various eminent naturalists.

Amateur botanists can carry on their favourite pursuit in two widely-different ways: they can play at science, and so amuse themselves to their own satisfaction, may be, but with little advantage to what they propose to admire, or they may patiently and conscientiously work and observe within their own sphere of research, and thus be able to render, as occasion serves, very real service to science at large, more, perhaps, than they are aware of at the time, and certainly to earn very genuine pleasure for themselves.

Not that I mean to insinuate that the Manchester Club are playing at science, but I warmly agree with your remarks that a mistake is being made by them in this respect. The instance is not a solitary one, even in my limited experience. Not long ago I found myself protesting against the notion of an amateur botanist (in England), who was endeavouring to introduce a species into a new locality. This is about as detrimental a proceeding in its way as that of the wanton eradication of a species from a neighbourhood.

I write *con amore*, for I happen to live and botanise in a part of Europe lying off the line of railways, therefore little visited, but possessing an interesting and somewhat peculiar flora, and am not unfrequently applied to by eminent botanists for information as to the real existence in the district of plants alleged with more or less of truth to be indigenous here.

Fiume, Austria AN AMATEUR BOTANIST

Science Teaching

IN an article in NATURE, December 29, 1870, on "Science at School Boards," the teaching capabilities of this country appear to me to be under-rated; I refer especially to the following passage:—"We would advise that some attempt be made to teach some quantum of Natural Science somehow. The present masters will probably be utterly ignorant of any branch of Science!"

If such is the fact, I would ask what have the training-schools been doing for years past? Most of these schools have university men as teachers, or men who have obtained a lectureship; and surely they have turned out students of two years' residence, capable of teaching one or more of the elementary branches named by the writer of the article referred to.

Does Dr. Lankester really think that the majority of the trained certificated masters of this country are incompetent to teach elementary physical geography? Many of these masters possess, I believe, a certificate for teaching Science, in virtue of having passed an examination under the Science and Art Department; and surely the examiners employed by the Department are such men as even Dr. Lankester would not ignore.

I feel confident that so far as teachers are concerned the matter is not so bad as Dr. Lankester imagines. The Revised Code checked all science teaching in elementary schools, but only let the Committee of Council accede to the appeal now made, and they will find plenty of masters able and desirous of teaching the elements of Science in our elementary schools.

There are two obstacles to be overcome before good results can be obtained. There must be better and ampler teaching power employed in our schools, so as to give the master the opportunity to carry out consecutive teaching; and then people generally must be made aware of the importance of Science to the artizan class, parents must be taught to appreciate the efforts made for their children. I will venture to say that more than half the work of the best teachers of this country is neutralised by the indifference or ignorance of parents.

Wisbech, Jan 3 SAMUEL H. MILLER

The Frost

I REMARK in the "Notes" of your last number, it is stated that "the lowest temperature at Blackheath was 15·3° F., on the night of the 24th December."

Now, assuming the correctness of the instrument from which this observation was taken, the locality must be much more protected from frost than that in which I reside. I have two good registering thermometers placed in a N.E. aspect, at about twenty-five or thirty feet from the ground—the one an upright mercury (Beck, Cornhill), the other a horizontal spirit tube (Hughes, Fenchurch Street), and the readings, which correspond exactly, were as follows:—

On the night of 23-24 Dec. 12° F., and at 7 A.M. on the 24th 14°. The maximum temperature (about noon) on the 24th was 24°, and at 11 P.M. the mercury had dropped to 16°.

On the night of the 24-25th the minimum was 9° F., at which point the register stood as late as 7 A.M. on the 25th, and even at 10 A.M. it had only risen 1° (10° F.)

I send you these observations, which I believe to be very correct, as they may be of some interest to meteorologists in our neighbourhood.

Blackheath, Jan. 9 JOHN CAREY

[It will be seen that these figures correspond very nearly with those given in our "Notes" this week.—ED.]

ON Sunday morning, 1st inst., a standard terrestrial radiation thermometer (exposed here on the previous evening) registered the unusual low temperature of 6·3 °F. or 25·7° below the freezing point.

The instrument was placed a few inches above snow covered grass on a gravelly soil, and exposed to nearly the whole sky.

JOHN JAMES HALL

Temporary Meteorological Observatory,
Fulwell, Twickenham, Jan 5

Sharks announcing their own Capture

LIEUT. C. H. TAYLOR, of H.M.S. *Cossack*, in his Remark-book for 1869-70, alludes to the following ingenious mode of making sharks announce their own capture. It appears that the island of Johanna, at the north end of the Mozambique Channel, is frequently visited by numerous sharks, whose flesh is esteemed as an article of food by the natives, who also prize the skin and oil for domestic or commercial purposes.

The Johanna men, however, being too lazy to fish in what might be termed a legitimate manner, have recourse to floating traps, with line, hook, and bait, and supporting above water a pole and basket. A bight of the line near the raft is attached to a bolt or toggle, which, when in place, keeps the pole in an upright position, but the moment a strain is brought on the line by the fish being hooked, the toggle is withdrawn, causing the pole and basket to fall, which is a signal to the people on shore that the prey awaits their coming. C. F. MCDOUGALL

Hydrographic Office, Admiralty

Extraordinary Meteor

THE following account of an extraordinary meteor occurs in a letter I received from a brother who is a missionary stationed in Agra. He does not give the exact place where he was at the time, but it must have been very near to Agra. The letter is dated Agra, 24th November, 1870. A missionary from Allahabad was with him when he saw it. ROBERT GRYSON

Mills Hill, Chadderston, near Manchester

"Agra, Nov. 24, 1870

"I recently saw a marvellous meteor. I was in camp, and had risen for an early march a few minutes before 3 A.M. on Nov. 4th. I was standing under the shade of a cluster of trees, when a sudden flash of light fell around. Two or three camp fires were blazing near, and at first I thought it might be a sudden flare up from one of them, but on casting my eyes up towards the heavens, I saw a large oval light, stationary. It appeared to be composed of a large number of irregularly-shaped, differently-sized stars, yet so closely packed as to form one light, yet giving the whole a sort of dappled appearance. At first I was struck dumb with amazement—thought it must be some mental illusion, or that my eyes were playing me false. But as I gazed it remained steadily fixed. ——— of Allahabad was with me. I roused him. He was soundly asleep, and some seconds passed in waking him up. In the interval it appeared to have been lengthened, nearly, though not quite, by a straight line, and as we gazed it assumed the shape of a large magnet, with the upper limb rather shorter than the other. It then gradually expanded, diminishing in brightness as it increased in size, assuming a wavy, serpentine form, though keeping much to a horse-shoe shape, until it became so attenuated as to be no longer visible. It must have continued in sight five minutes. It was seen by all the servants, and one of them cried out '*Bhagwauka seela hae*,'* by which he appeared to mean that in his opinion the Almighty was amusing Himself with fireworks; literally, 'It is God's sport or amusement.'"

NATURAL SCIENCE AT CAMBRIDGE

THE following is a list of the Scholarships and Exhibitions for proficiency in Natural Science which are likely to be offered in Cambridge during the present year:—

TRINITY COLLEGE.—One or two of the value of about 80*l.* per annum. The Examination will be in Easter week, and will be open to all undergraduates of Cambridge and Oxford. Should one Scholarship only be assigned, preference will be given to the candidate who shows the greatest proficiency in Physiology and the allied subjects. Further information may be obtained from the Rev. E. Blore, Tutor of Trinity College.

ST. JOHN'S COLLEGE.—One of the value of 50*l.* per annum. The Examination (in Chemistry, Physics, and Physiology, with Geology, Anatomy, and Botany) will be on the 21st and 22nd of April, and will be open to all persons who are not entered at the University, as well to all who have entered and have not completed one term of residence. In this College, moreover, Natural Science is now made one of the subjects of the regular College examination of its students at the end of the academical year (in May); and Exhibitions and Foundation Scholarships will in consequence be awarded to students who show an amount of knowledge equivalent to that which in Classics or Mathematics usually gains an Exhibition or Scholarship in the College. In short, Natural Science is on the same footing as Classics and Mathematics, both as regards teaching and rewards. Further information may be obtained from the Rev. T. G. Bonney, Tutor.

CHRIST'S COLLEGE.—One or more, in value from 30*l.* to 70*l.*, according to the number and merits of the candidates, tenable for three and a half years, and for three years longer by those who reside during that period at the College. The examination will be on March 28, and will be open to the undergraduates of this College, to non-collegiate undergraduates of Cambridge, to all undergraduates of Oxford, and to any students who are not members of either university. The candidates may select their own subjects for examination. Besides these there are three other Exhibitions perfectly open, which are distributed annually among the most deserving students of the College.

CAIUS COLLEGE.—One of the value of 60*l.* per annum. The examination will be on March 30, in Chemistry and Experimental Physics, or Zoology, with Comparative Anatomy and Physiology, or Botany, including Vegetable Anatomy and Physiology.—Scholarships of the value of 20*l.* each, or more if the candidates are unusually good, are offered, for Anatomy and Physiology, to members of the College.—Gentlemen elected to the Tancred Medical Studentships are required to enter at this College; these studentships are four in number, and the annual value of each is 113*l.* Information respecting them may be obtained from B. J. L. Frere, Esq., 28, Lincoln's Inn Fields, London; and respecting the other scholarships, from the Rev. N. M. Ferrers, Tutor of the College.

CLARE COLLEGE.—One or more of the value of 50*l.* per annum. The examination (in Chemistry, Chemical Physics, Comparative Anatomy, and Physiology, and Geology) will be on March 23, and will be open to students intending to begin residence in October.

DOWNING COLLEGE.—One or more, according to the merits of the candidates, of the value of 40*l.* per annum. The examination (in Chemistry, Comparative Anatomy, and Physiology) will be in March, and will be open to all students not members of the University, as well as to all undergraduates in their first term.

SIDNEY-SUSSEX COLLEGE.—Two of the value of 40*l.* per annum. The examination (in Heat, Electricity, Chemistry, Geology, Physiology, Botany) will be in October, and will be open to all students who may enter on the College boards before October 1.

Although several subjects for examination are in each instance given, this is rather to afford the option of one or more to the candidates than to induce them to present a superficial knowledge of several. Indeed, it is expressly stated by some of the colleges that good clear knowledge of one or two subjects will be more esteemed than a general knowledge of several.

Candidates, especially those who are not members of the University, will, in most instances, be required to show a fair knowledge of Classics and Mathematics, such, for example, as would enable them to pass the Previous Examination.

There is no restriction on the ground of religious denomination in the case of these or of any of the Scholarships or Exhibitions in the Colleges or in the University.

Further necessary information may be obtained from the Tutors of the respective Colleges.

It may be added that Trinity College will give a Fellowship for Natural Science once, at least, in three years; and that most of the colleges are understood to be willing to award Fellowships for merit in Natural Science equivalent to that for which they are in the habit of giving them for Classics and Mathematics.

EXPLORATION OF THE PERENE (AMAZONS) RIVER

ON the 28th November the Peruvian Government published the report of the expedition sent to examine the capabilities of the Perene river and neighbouring country, and the slope of their territory towards the Atlantic. This river, which is a branch from the Vcuyali, rises near Tarma, the capital of the Department of Junin, and flows through the country of the Chunchamayo Indians, a wild tribe, hostile to the Peruvians and but little known to them. The soundings were found to give from three to five fathoms in depth and the width was about 100 yards. The course of the river was east for 1,000 yards from its confluence and then north.

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* I cannot be quite sure of these words, not knowing the original language in which they were spoken.—R. G.

In the valley of Chunchamayo the forests consist of trees of extraordinary size, and the villages are numerous. According to Padres Amich and Sobrevista, the Indians belong to the tribe of the Amayos. The chief object of search of the expedition was the Cerro del Sal, or the Salt Hill, from which the Indians obtain that necessary article. The expedition consider they ascertained the region of it in a sandstone formation.

The expedition observed plantations of cocoa (coca?) and Indian corn, and on the banks of the river fishing huts, with nets and fittings for catching and salting fish. What was most striking was an iron furnace of a square form, about two yards high, and five feet each way, constructed of bricks eighteen inches long. It was worked with a double bellows, and supplied with coal, wood, and pounded ore. Water for the tanks was brought from a height in landers of bark hides. There were about twenty or thirty hundredweight of cast-iron. The whole excited the admiration of Mr. James Greg, owner of the Lima Iron Foundry, who accompanied the expedition.

The party fully ascertained the presence of salt, and considered they had found the vein of salt reputed to be ten miles in length, as they found it consisted of earth with particles of salt and quartz crystals. On digging down they found that the salt increased.

The Indians use the bow and arrow, and seemed much afraid of firearms. The commander recommends as the best mode of subduing them that the Salt Hill shall be taken possession of. It is considered that a navigable point has been found about 200 miles from the capital City of Lima, and by means of which it can obtain communication with the Atlantic. The Government is strongly urged to send a more powerful expedition.

HYDE CLARKE

BRITISH DIATOMACEÆ *

IT is now some years ago since Mr. Van Voorst published for Messrs. Smith and Beck two volumes of a "Synopsis of British Diatomaceæ," by Prof. William Smith, of Cork, which were beautifully illustrated by Mr. Tuffen West. The latter volume of this Synopsis was published in 1856, and even then the number of new forms of these minute silicious Algæ had rendered necessary the preparation of a supplement which, however, owing to the death of the author, never appeared. Since 1856 many and important works and memoirs on the diatoms have been published, and not only have the pages of the Quarterly Journal of Microscopical Science contained numerous writings on this subject, but workers like Rabenhorst, Grunow, and Cleeve have continued to add to not only the number of species, but to the amount of our knowledge of the Diatomaceæ. We venture, nevertheless, to think that the time had not quite come to write anew a history of the British Diatomaceæ. The difficulty of finding good specific characters remains just as great as it was when Smith's work was published, and the number of local lists recorded has been too few to give us anything like an idea of the geographical distribution of these forms; still we should be sorry not to welcome one of Mr. Van Voorst's series of British Natural History works, a series of which we have every reason to be proud; almost their only drawback being incidental to the method in which the works of the series are published, viz. in numbers, by which it too often happens that there is a want of uniformity between the earlier and later portions of the work.

Dr. Donkin's work is to consist of two parts, "the first will be introductory, and contain a full account of the Diatomaceæ and a new classification based on their structure and mode of development;" and the second, which is to be published first, "will be synoptical, and give an accurate and succinct description of all the known British genera and species. The synonyms of each species will be fully given *with the view of rendering the work more valuable for the purpose of reference.*" There strikes us as being something quaintly simple in the words we have italicised. Fancy an apology for giving synonyms, as if they were something or another that people did not care about!

Part one (pp. 1 to 14 and plates 1 to 4) now lies before us. The execution of the plates disappoints us. We know that the artist's right hand is very far from having lost its cunning, and yet the figures here are, to our mind, much inferior to the figures by the same hand in Smith's work. 500 diameters, too, is a large scale for the amplification, and will only unduly increase the number of plates—it does not appear either to have contributed to greater distinctness of detail. Plate 3 is open before us. Fig. 3 we should consider scarcely recognisable; and fig. 7 is greatly less true to nature in both outline and proportion than one also of the same species before us by Grunow.

The first family treated of in the text is the Naviculeæ. No synonyms are given to the genus *Navicula*, but we learn from the diagnosis and from the remarks in the text that *Pinnularia* Ehrb., as emended by Smith, is reduced to the rank of a synonym of this genus; so that Navicula will be a genus very full of species, and we venture to suggest that there will be great difficulty in many instances in determining whether to refer a species to the section with flattened or convex valves. The initials M. V. are used to express a view of the frustule, exhibiting the *median connecting zone*, and the *margins of the valves;* this we welcome as an improvement.

Perhaps this is hardly a fitting place to criticise very closely the species given in this part. Opinions may differ very widely indeed as to what is a species among these little Algæ, still we think it questionable if *N. didyma* var. Smith, Synop. vol. i. p. 53, quoted as a synonym to *N. smithii* De Breb. is not greatly nearer *N. smithii* var. *fusca* Ehrb. Again, we do not think that *N. hyperborea* Grun. is identical with *P. fusca* Ehrb.; it differs both in outline and details, and unless the difference in striæ is to be altogether given up, *Pinnularia forficula* O. M. cannot be regarded as synonym of *N. suborbicularis* Ehrb.; *N. nitescens* Ehrb., and *N. suborbicularis* Ehrb., are rightly kept as separate species, and we also approve of the separation of *N. subsalina* from *N. amphisbæna*. *N. latissima* Ehrb. is described as having striæ distinctly *granular*, how then can *Pinnularia divaricata* O.M., striæ distinctly *costate*, be regarded as a synonym? It will not do to separate some forms and bring others together exactly for the same reason. The habitats given are most meagre, and the work presents a contrast in this to the rest of Mr. Van Voorst's series. Even in cases where the author could have given Irish localities for rare and interesting forms, he has passed them over, and if we had not found a reference or two to the Lough Mourne Deposits in the County Down, we should have fancied that no Irish localities were to be given.

We have been thus candid in our notice because we believe it possible with care to remove from this work the signs of its being slightly premature. It only needs to keep to its promise and give the *synonyms* in full, and to give the *habitats* in full, at least for uncommon forms, and lastly, to make the figures more distinct, to make this "Natural History of British Diatomaceæ" a very valuable work.

W.

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* "The Natural History of the British Diatomaceæ." By Arthur Scott Donkin, M.D. Part 1. Nov. 1, 1870. (London: J. Van Voorst, Illustrated with Plates by Tuffen West, F.L.S.)

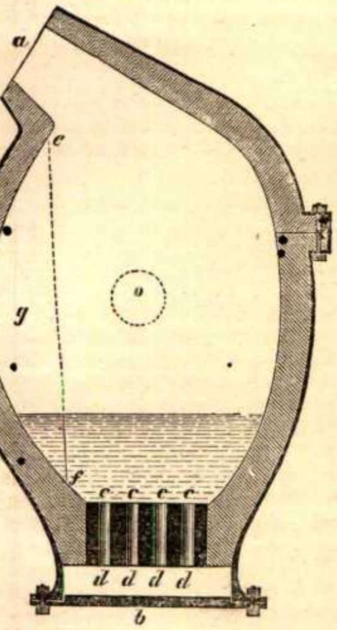
PAPERS ON IRON AND STEEL

No. II.—The Bessemer Process.

In this paper I propose to describe the general phenomena of the Bessemer process, and then to examine the chemical actions producing these phenomena and the changes they effect in the material operated upon.

In the first place the pig-iron is melted in a suitable furnace, usually in that form of furnace known as the "cupola." The melted iron is run from this by means of moveable troughs into the "converter," which is a pear-shaped spouted vessel, lined with fire-clay, "ganister," or other refractory substance.

This pear-shaped vessel, a vertical section of which in the upright position and without mechanical details is represented in the annexed figure, is truncated at the lower end, and thus a flat circular bottom is formed. This bottom, which is readily detached and renewable, is fitted with longitudinally perforated fire-clay cylinders shown in section at *cd*, *cd*, *cd*, *cd*, each perforation or clay tube being about one-half or three-quarters of an inch in diameter, and all communicating with the space *d d*, into which opens the blast tube from a powerful blowing engine. The number of these blast holes varies from fifty or sixty to a hundred or more, according to the size of the converter.



The converter is mounted on trunnions so arranged that it may turn on a transverse axis crossing about the middle of the vessel, as shown by the dotted circle *o*. The turning is effected by hydraulic machinery, controlled by levers readily worked by a man who stands on a platform in full view of the converter. In order to receive the charge of melted iron, the converter (the lining of which has been previously raised to a bright red heat) is turned over so that the dotted line *e f* becomes horizontal, and corresponds to the surface of a full charge. The belly *g* of the converter is so curved that it shall in this position retain the whole charge without any of it reaching the blast holes at *f*, or the mouth at *e*, and yet allow the whole charge to be readily "teemed" by turning the converter a little further down.

When the full charge is thus received in the belly of the converter, the blast is turned on, *after which* the converter is turned to the upright position, as shown in the figure, and the melted metal then stands directly over the perforated bottom. As will thus be seen, all the fluid metal above the openings is now resting upon a bed of air, and is only prevented from falling through by the blast being maintained at a pressure exceeding the falling force of the column of liquid above it. It would fall through these orifices into the blast-way and do serious mischief should the blast be stopped or slackened for an instant, or should the converter be turned upright or overcharged before the commencement of the blast. An accident of this kind but rarely happens, though it is by no means an unknown casualty.

The "blow," as it is termed, now commences; the hundred streams of air tear through the pool of melted iron, and a huge flame roars furiously from the mouth of the converter. At irregular intervals magnificent cascades of brilliant coruscating sparks are belched forth, and the dazzling spray as it dashes against the walls of the flame-shaft rebounds with redoubled splendour, each glowing globule being shattered by the shock and bursting into rescintillating fragments. The loud-bellowing blast roars on monotonously, but the flame becomes brighter and brighter continuously, and grows in length and breadth as it increases in brilliancy, until at the end of about ten minutes it attains its maximum, when its splendour is painful to the eye, and yet so fascinating that few who see it for the first time can turn their dazzled eyes away. The spark eruptions still burst upwards from time to time, and still dash against the brickwork and the ground, and still reverberate in fiery splinters, but their appearance has changed. They are now no longer red hot, or yellow hot, or white hot, but have a curious purple luminosity different from anything one has ever seen before. If it is daytime and the sun shining, the sunlight out of doors has a sickened partial-eclipse aspect when viewed directly after gazing at the flame, and at night the ordinary gas lights appear red and smoky.

After five or ten minutes' continuance of this maximum splendour, the flame is seen to contract somewhat, and presently the ponderous vessel turns a very deliberate summersault, the flame disappears, but the uninitiated spectator is startled by a new display; for as the converter rolls smoothly over, it disgorges a continuous stream of sparks which its rotation spreads out in a fan-shaped volley, extending from end to end of the building, and reaching the roof, descends in a broad sheet of fiery hail. This is the transformation scene which concludes the first part of the performance; for now the dazzle of the flame and the roar of the blast ceases, and a general lull intervenes.

The trough from the cupola is now swung round to the mouth of the converter, a red glow is seen to creep along it, and starry sparks dance above as it advances. This is the spiegelcisen coming from its cupola by the same path as conducted the main charge. The spectator should now change his position, and if possible find a standing place from which he may look into the mouth of the converter. At first he will distinguish nothing but a yellow glare, but by steadily fixing his gaze, he will presently, and rather suddenly, distinguish the surface and limits of the pool of melted metal. He will see that as the spiegeleisen pours into it, a furious ebullition takes place. At the same time a great mass of pale blue flame issues from the mouth of the converter, but with a quiet, leisurely waving that contrasts curiously with the previous roaring jet of white flame. This flame has but very little intrinsic luminosity, yet at night it lights up all the surrounding objects with a singular brilliancy, a sort of exaggerated theatrical moonlight effect, which is the most remarkable to a spectator outside, who on a misty night sees the long streams of ghostly light pouring through every opening of the building in pallid beams, that under favourable conditions may be traced for above a quarter of a mile. I have seen them projected in bright discs upon the face of low clouds, and visible through the whole of their intermediate course.

When the flow of spiegeleisen has ceased, the trough is moved aside and a large counterpoised arm bearing the "ladle" is swung round upon an hydraulic piston, which forms at the same time its axis and lifter. The ladle, a large lined iron pot, is adjusted under the mouth of the converter, which is now tilted a little more, till the melted metal is poured out in a thick brilliant white-hot stream accompanied from time to time with great slabs of cinder of a darker colour which float upon its surface as it pours, and form a thick scum covering the contents of the ladle. When all the fluid metal is poured into the ladle, the converter is tilted over till completely inverted, and the remaining viscous mass of cinder drops out in a glowing heap upon the floor.

During these proceedings a set of workmen have been preparing the moulds in which the ingots of steel are to be cast. These moulds are of cast-iron, nearly cylindrical, being larger at bottom than top, and open at both ends. They have lugs or handles at top by which they are lifted. They stand upon a tile, and are well packed round the bottom with sand to prevent the outflow of the melted steel. While the blow was proceeding these were arranged in an arc of a circle whose radius exactly corresponds with the length of the arm bearing the ladle.

The ladle is now swung round and adjusted till it stands directly over the first of this row of iron vases, and a plug is released by which a hole in the bottom of the ladle is opened. Through this the steel is poured into the ingot. When the first is filled the plug is closed, the ladle swung round to the second mould, and so on, till all the steel is thus cast into ingots, the size of which varies with the kind of work for which the steel is required. A thin steel plate is placed on the top of each casting immediately the mould is filled, and over this a bed of sand is placed, and speedily and firmly pressed down.

As soon as the ingots have solidified, and while they are still glowing, the moulds are lifted off them by means of an hydraulic crane, and afterwards the ingots are picked up by tongs attached to the same machinery, and are carted away, all red hot, to the hammer-shops, where they are thumped and rolled or otherwise tortured into their required forms of rails, tyres, plates, &c.

The above are the leading phenomena of the Bessemer process; the chemical actions producing them, and the changes wrought in the pig-iron and spiegeleisen, will be treated in another part of this paper.

W. Mattieu Williams

NOTES

Owing to Mr. Lockyer having been summoned to Malta to give evidence at the court-martial on the commander of the unfortunate *Psyche* (which we regret to hear has not been saved), we are unable to give a detailed report of the proceedings of the Sicilian Eclipse Expedition. We understand that Mr. Brothers, who was stationed at Syracuse, obtained five photographs of the Eclipse during totality. One of these shows the corona "as it was never seen on glass before." At Augusta very little was seen; but at Syracuse, the southernmost station of all, the clouds which concealed the earlier stages of the Eclipse, passed away from the sun about five minutes before totality, "disclosing," writes Mr. Brothers, "a scene I shall never forget." Next week we shall hope to be able to give a complete account of the results of the Expedition, and their bearings on any increase of our knowledge of Solar Physics.

Professor Carl Gustav Bischof, who died at Bonn on the 29th of November last, was equally distinguished as a chemist and a geologist. He was born in 1792, near Nüremberg, in Bavaria, and was consequently at the time of his death in his seventy-ninth year. In 1810 he entered the University of Erlangen, where the lectures of Prof. Hildebrandt induced him to devote his study to chemistry and physics. In 1816 he succeeded to his master's position, and brought to a conclusion his "Lehrbuch der Chemie." In 1822 he removed to Bonn, in which University he occupied the position of Professor of Chemistry from that time till his death. Shortly afterwards, however, he began to pay more attention to subjects connected with chemical and physical geology, publishing a large number of treatises of sterling merit, an enumeration of which is given in the *Geological Magazine* for January. In 1841 his "Physical, Chemical, and Geological Researches on the Internal Heat of the Globe," were published in London, and in 1854 an enlarged translation of his "Lehrbuch der chemischen und physikalischen Geologie," was issued by the Cavendish Society. He was a Foreign Member of the Geological Society of London, and had received from that body the gold Wollaston medal.

The following courses on Anatomy and Physiology at Cambridge are announced for this term:—Zoology and Comparative Anatomy, three days a week, by Prof. Newton; Anatomy and Physiology, three days a week, by Prof. Humphry; Practical Anatomy, three days a week, by Prof. Humphry and Mr. Carver; Practical Histology, by Mr. Martin, under the superintendence of Prof. Humphry, once a week, with a Microscopical Demonstration once a fortnight; Physiology, three days a week, by Dr. Michael Foster, with Practical Instruction in the Physiological Laboratory daily.

Professor P. M. Duncan will shortly commence a course of Lectures at King's College, London, which is open to the public, and to those who are going in for examinations. It will comprise ten lectures on the Principles of Biology in its relation to the Succession of Life on the Globe, five on Astronomical Geology, and the rest on the Principles of Geology. The course will open on Saturday, Jan. 28, at 11.30 A.M., and will be continued through the Lent, Easter, and Michaelmas terms.

Dr. E. Symes Thompson will deliver two lectures at the Gresham College, Basinghall Street, "On the Circulation of the Blood," on Saturday the 14th and Monday the 16th of January, at seven o'clock, which will be free to the public. It is proposed next term to continue the course "On the Organs of Respiration and Circulation in Health and Disease."

In answer to several inquiries, we may state that the penny lectures delivered at the Hulme Town Hall by Prof. Huxley and others, to which we referred last week, are published by Messrs. Heywood, of Manchester.

We stated in our last number the lowest temperature recorded at Blackheath during the recent frost to have been 15·3° F. on the night of December 24. More recent tables published in the *Gardener's Chronicle* give the minimum as 9·8° on the 25th. From December 22nd to January 4th the temperature at Blackheath ranged between 6·9° below the mean of fifty years, on January 4th to 18·7° below the average on December 25th. The minimum above mentioned occurred at 7 A.M. on Christmas Day, being lower than any temperature since the morning of Christmas Day 1860, when it was 8°. In the midland and eastern counties, where the cold was most severe, it is feared that much injury has been done to vegetation, especially to the evergreens. In Paris the frost has been equally intense. In a paper recently read before the French Academy of Sciences, it was stated that during December the temperature only rose above the freezing point on nine days. In the fifty years from 1816 to 1866 the average temperature for the month of December has been 3·54° above zero C. or about 38° F. The average temperature of this last December has been 1·07° below zero C. or a little above 30° F.

Engineering for Jan. 6 gives a ground plan and elevation of the proposed building for the united accommodation of the Society of Arts, Statistical Society, Royal Colonial Institute, Iron and Steel Institute, Institute of Actuaries, Ethnological Society, Anthropological Society, Photographic Society, Social Science and Law Amendment Society, Juridical Society, Victoria Institute, Royal Archæological Society, Meteorological Society, East India Association, and others which will not find a home in Burlington House. The proposal is to adapt for the purposes of these Societies the block of buildings, No. 4, Westminster Chambers, Victoria Street, together with a triangular piece of vacant ground, about a fifth of an acre, now lying waste in the rear of the Westminster Chambers. With these resources it is calculated that ample room will be obtained for the needful offices, reading-rooms, &c., as well as for libraries and museums, and a large lecture theatre capable of holding 1,200 persons.

THE Meteorological Office is now issuing daily Wind Charts of the British Isles, which are published in the *Mercantile and Shipping Gazette*. The chart indicates the direction and force of the wind at a number of different stations on the coast of Great Britain and Ireland, and will be found of great value to all interested both in shipping and in meteorology.

WE have received a copy of the Inaugural Lecture delivered by Prof. M'Coy at the opening of the Industrial and Technical Museum of Victoria, at Melbourne, to which is appended a short sketch of the contents of the Museum. The most important collections are illustrations of the manufacture of glass and of pottery, a series of 1,300 specimens of Victorian rocks and minerals, and 500 specimens of New Zealand rocks; a phytological collection to illustrate the natural productions of the Australian colonies, and a series of Australian ores, slates, building-stones, &c. Courses of lectures on technical subjects are delivered at the Museum.

THE first number has been issued of a *Journal of the London Institution*, containing short reports of lectures which have been delivered and a programme of proceedings in the future. In future numbers it is proposed to devote considerable space to Notices of New Books presented to the General Library, to Bibliographic and Scientific Notes and Queries, and to Records of Laboratory Work.

WE have received the first number of *The Quarterly Journal of the Amateur Mechanical Society*, the object of which is stated by the honorary editor, the Rev. J. Lukin, to serve as a medium of mutual information upon all points connected with mechanical manipulation. The best idea of its nature will be given by a list of the contents of the first number: The Rise and Progress of the Society; Eccentric Turning; Mensuration and Enumeration of the Impalpable and Invisible; Medallion Machine; Fancy Turning in Box-wood; and Breechloading Fire-arms. Several of the articles are illustrated.

THE December number of the *American Entomologist and Botanist* completes the second volume, and the publication will now be suspended for a twelvemonth, intending to be renewed again after that period. In the meantime the botanical editor, Dr. Vasey, will conduct a botanical department in the *Journal of Agriculture*, published at St. Louis.

THE Natural History Society of Montreal has just issued its Annual Report, comprising a sketch of its proceedings for the year ending May 1870. Although the number of members has decreased during the year, the Society has, nevertheless, shown considerable activity, and many valuable papers have been read at its meetings on Geology, Zoology, and general subjects, some of which have appeared in the *Canadian Naturalist*, the organ of the Society. A very important work is now contemplated by the Society outside its immediate sphere of action, viz., the dredging of the Gulf and River St. Lawrence. Application has been made to the Dominion Government for a free passage for the dredgers in one of their ordinary cruisers. It is feared, however, that this application may not be successful, and that the Society may have to draw upon its private resources for the necessary expenses.

THE *Homeward Mail* gives reports of the earthquake that was felt in Scinde in October last. On October 28 two shocks are reported from various places. One report says "the earth quaked for fifteen minutes from east to west, and the people felt sea-sick while the pitching continued," and another ascribes it to the fact that the dwellers in Upper Scinde must have specially incurred the displeasure of the gods.

HEAVY rains, followed by severe frost, have produced their usual effect in altering the shape of the land. We have accounts of two extensive landslips, one at Whitby and one at Mevagissey in Cornwall. At the former place, "a large part of the cliffs, supporting twelve houses, has fallen into the harbour, and it is feared that more will fall." At the latter place about 500 to 600 tons of rock were precipitated to a depth of fifty feet, and much damage was done. Exactly a year ago, in December 1869, a great landslip was reported near Nantmel in Radnorshire. The mass of earth which fell was of enormous size, and did not become stationary till it had travelled half a mile. Much damage was done, though no lives were lost.

DR. HENRI VAN HEURCK publishes in French, under the title of "Observationes Botanicæ et Descriptiones Plantarum novarum Herbarii Van Heurckiani," the first fasciculus of a description of new and undescribed species contained in his herbarium. The descriptions are drawn up by several eminent botanists, and the herbarium, one of the richest in the world, containing collections from all quarters of the globe, results from a fusion of those of Sieber, Baron von Reichenbach, and Dr. Van Heurck, to which numerous important additions have been made by purchase and otherwise. The volume is offered in exchange for other botanical publications.

DR. L. PFEIFFER of Cassel, has recently published the first part of a "Synonymia Botanica locupletissima Generum Sectionum vel Subgenerum ad finem anni 1858 promulgatorum." Such a synonymy is much wanted by botanists who may be working at any particular order or genus. The value, however, for the ordinary systematist, is considerably decreased by the adoption of the singular arrangement of Endlicher in preference to those in use in Hooker and Bentham's "Genera Plantarum" or in De Candolle's "Prodromus."

MR. THOMAS MEEHAN read a paper before a recent meeting of the Academy of Natural Sciences of Philadelphia on the Compass Plant (*Silphium laciniatum*). He confirms the statement of earlier observers, that when the plant first comes up, and until the leaves become large and heavy, there is an unmistakeable tendency towards the north. When, however, winds and rains have once borne them in different directions, they have no power of regaining the points lost. Hence the statement made by some, that they have examined the plant in its native habitat, and found no such tendency.

THE *Pharmaceutical Journal* for December 31st contains an interesting article on the trade in leeches. The annual value of the leeches imported into this country decreased from 27,068*l*. in 1853 to 7,067*l*. in 1869; the largest quantity coming *formerly* from Hamburg, but more recently from France. These figures probably represent an import at the present time of two million leeches annually, besides the home supply. The leeches annually employed in France may be taken at thirty millions, the largest portion being produced at home. The South and West of France and North of Africa still produce enormous quantities.

A farmer in the neighbourhood of Bordeaux has recently converted waste land worth 300 francs per annum into enclosures for leeches, which now bring him in 25,000 francs annually. Besides the well-known brown and green leeches, a communication to the Société Zoölogique d'Acclimatation of Paris, by M. de Filippi, describes a new genus, *Hæmentaria*, from Mexico and the Amazon, which possesses the valuable property of leaving no mark on the skin to which they are applied; acting not by biting but by suction.

AN interesting paper on the introduction of Maize into China, by Dr. Hance and Mr. W. F. Mayers, appears in a recent number of the *Pharmaceutical Journal*. The authors are inclined to believe that Asia may rank as a native country of this cereal; "the remote date assigned by Chinese records to its introduction, and the circumstance that the introducer is unknown," being, in Dr. Hance's judgment, "irreconcileable with the supposition that it was brought to this country by the Portuguese, their first arrival here under Ferdinand Perez d'Andrada being in 1517, and the earliest notice of maize in European literature dating later than 1530. Mr. Mayers gives translations of passages from Chinese authors bearing upon the subject, as well as facsimiles of early Chinese engravings of the maize and millet.

MR. EDWARD NEWMAN, in the *Field* for December 31, directs attention to the fact that during 1870 there has been an unusual immigration of quails to this country, and that a still more unusual number have stayed to breed; and desires to acquire materials for what he terms "a census of quails" during this exceptional year. The statistics particularly desired are: The number of quails bagged, and the dates; the number of nests found; the number of eggs in each nest; and especially remarks as to the period and direction of flight, both on the arrival and departure of the migrants. The occurrence of the quail in Britain in such unusual numbers is, as Mr. Newman observes, a matter of great ornithological interest.

MUCH good would accrue to our peasantry and working classes generally, if a better knowledge of the value of Nature's products were diffused amongst them. Blind prejudice prevents the proper application of a host of "unconsidered trifles." We in England might take a lesson from what is done in Sweden by a Public Society, who, during a time of scarcity of food, and for the purpose of diffusing a knowledge of the edible Fungi and Lichens of that country, prepared, published, and distributed to the public schools no less than 10,000 copies of a pamphlet on the Fungi illustrated with coloured figures, and 4,000 copies of one on Lichens illustrated by actual specimens.

THAT the *Boehmeria nivea*, or China Grass fibre, will, ere long, become a regular article of import to this country, is highly probable, both from the fact of the recent reward of the Indian Government for the invention of machinery suitable for its cleaning and preservation: and from the nature of the plant being such that the climate and soil of many of our colonies are quite suited for its culture. There is no doubt that it is well adapted for a variety of uses, and could, by careful preparation, be applied to purposes for which our present commercial fibres, though to some extent used, are nevertheless unsuited. The Chinese bestow an immense amount of care and labour upon its preparation, hence the very fine fabrics which are produced in that country. In Sumatra, also, much care is given to its cultivation and preparation: the stems are usually cut when they are about six feet high. They are sometimes allowed to dry before the fibre is taken from them, but the most common practice is to take it as soon as the stems are removed from the ground. A viscid gum is found on the stem which, in Macassar, is scraped off and used as a mild sort of arrow poison. In China three crops of the stems are usually obtained in one year, but the second crop is considered to yield the best fibre.

ON THE GEOLOGY OF NOVA SCOTIA[*]

THE author, in treating on the Laurentian Rocks of Arisaig, Nova Scotia, discovered by him in 1868, referred to the occurrence of pebbles of diorite, syenite, and granite in the conglomerates of the Lower Carboniferous area of the townships of Arisaig and Antigonish, and the highly micaceous character of their grits and sandstones. He had experienced difficulty in accounting for these appearances. It appeared singular, especially, that the mica seemed to increase in those strata in proportion as they became removed from known granites. The lithological character of the discovered Laurentian band appeared to account satisfactorily for the occurrence of those constituents of the Lower Carboniferous strata. There are some gneisses and porphyritic diorites; hornblendic rock in great variety; serpentines, black quartzite strata with veins of quartz, with abundance of crystals of mica (some would be disposed to call them granite veins); white syenite with stripes of green felspar and red syenite, both very sparingly hornblendic. South of these lies a Carboniferous area which seems to overlie the Laurentian band unconformably. This area is bounded on the south by a subtriangular band of metamorphic Arisaig, or middle and upper, Silurian rocks. This band is disposed in two anticlinal folds with an intermediate synclinal. The author designates it the Antigonish Sugar-loaf Band, so named from a prominent mountain of 710 feet elevation. The extreme breadth of this band, *i.e.* N. and S., is about five miles. The axes run easterly and westerly. The S. side of the Carboniferous area referred to rests unconformably on the N. side of the northern anticlinal, the strata in contact being Lower Carboniferous conglomerates. This area extends to St. George's Bay and Cape St. George on the Gulf of St. Lawrence; it is basin-shaped, and is said to contain seams of coal. This area may be called the Arisaig area. South of the Silurian area lies the Antigonish Carboniferous area. The lower part of this area consists of conglomerates, limestone, and gypsum. The conglomerate lies unconformably on the Silurian slates of the S. side of the southern anticlinal. The slates dip < 55° S. 5 E.; the Lower Carboniferous conglomerates and limestones dip < 30° S. 35 W. The observations made are at variance with two theories that have been advanced by different geologists to the effect—1st, that the Upper Silurian and Devonian formations of Nova Scotia have been thrown into a few *great* folds, synclinal and anticlinal; 2nd, that the Carboniferous and underlying Devonian or Upper Silurian formations acquired their present positions simultaneously, the mountains having had a thick Carboniferous saddle, which had been subsequently removed by denudation. This theory supposes that the Carboniferous areas of Nova Scotia had been once united with each other and those of other countries. The Nova Scotia areas that are now separated have always been so, the only connection ever existing having been merely geological.

The author discovered an interesting outcrop of Laurentian syenite in the Silurian area. This forms, in conjunction with limestone, a noticeable hill of 300 feet elevation, in the middle of the Antigonish area of Carboniferous limestone and gypsum. This syenite is seen to a large extent in direct contact with limestone of Lower Carboniferous age, having abundance of cyrtoceras, conularia, dentalium, and *Leperditia okeni*. The limestone and its fossils have not been altered by contact with the syenite, showing, as a consequence, that the syenite had its existing constitution when the limestones were formed upon it in the bottom of the sea of the Lower Carboniferous era. A specimen from the summit of the hill in the collection of rocks in the Provincial Museum is reddish, like specimens in the same collection from the Arisaig Laurentian rocks. It is more hornblendic, and shows green mica, like that of a specimen of granite from a mountain in Baddeck, Cape Breton. The author made interesting observations on supposed Laurentian rocks in the Island of Cape Breton, which lies to the N.E. of Nova Scotia, being separated from the latter by a narrow strait called the Gate of Canso. In the Nova Scotia department of the Paris Exhibition of 1867, there was a specimen of serpentine from a rock at St. Annez, Cape Breton. Prof. Wyville Thomson detected in this specimen supposed eozoonal structure. The author lately received specimens of granite from White Head, Aspy Bay, Cape Breton; and also from a position seventeen miles S., and seventy-three W. from White Head. He also referred to the existence of auriferous slates, like those of Nova Scotia, at Middle River,

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[*] Abstract of a paper read before the Nova Scotian Institute of Natural Science by the Rev. D. Honeyman, D.C.L., F.G.S., &c., Professor of Geology in the Provincial Museum.

Cape Breton. He observes that, if a line were drawn from the granite mountain at Big Baddeck already referred to, bisecting the granite district at Aspy Bay, the Middle River gold-field would be five miles distant from the line on the one side, and the St. Anne serpentine (eozoönal ?) three miles distant from the line on the other side. Here we have what is supposed to be Laurentian serpentine, granite, and auriferous argillite in no respect different from the argillite of Wine Harbour and other gold-fields of Nova Scotia, all in close conjunction. The existence of Laurentian eozoonal serpentine in this locality is in accordance with a *forecast* of Dr. Sterry Hunt, to the effect that a line from the Arisaig Laurentian to Newfoundland will pass through Cape Breton. We may now expect, he observes, to find limestone with eozoon there; and, on the contrary, the same facts appear to be at variance with his "Terra-Novan" theory. As the local name Arisaig has been applied by Dr. Dawson to the Middle and Upper Silurian of Nova Scotia, the author would suggest that, as Cape Breton appears to be *in a manner* the meeting place of the Laurentian of Arisaig, Nova Scotia, and the granites and argillites of the Nova Scotia gold-fields, the local term "Cape-Bretonian" should be adopted as their designation, and that "Terra-Novan" should be reserved for other countries. The one term is equally euphonic with the other, and much more ancient. It was observed that there was a great gap between the Laurentian of Arisaig and its Middle and Upper Silurian and Devonian. In searching for formations to fill the gap, it was necessary to look to the gold-fields of Nova Scotia. The evidence of fossils was much desiderated in the investigation. The grits and argillites of the gold-fields were lithologically different from the Middle and Upper Silurian and Devonian (fossiliferous or metamorphic), and stratigraphical evidence showed what the author regarded as constructive unconformability. He expects in the further prosecution of investigations which are to be recorded at a subsequent meeting of the Institute, to be able to bring the evidence of fossils indirectly to his aid, and to point out direct sequence. It was observed that the upper and middle Silurian rocks of Nova Scotia had as yet failed to show gold even in the very smallest quantity—that various localities having metamorphic slates and quartz-veins of Clinton or Middle Silurian age had received a short-lived celebrity in the provincial newspapers, but the report had invariably been found incorrect. The author hailed the decisions of Prof. Hind in reference to the age of the Greneid (granite) grit and argillite of the gold-fields, and considered that he had rendered very important service in completing the Azoic (or Eozoic) and Palæozoic systems of Nova Scotia.

LETTERS FROM CENTRAL AFRICA[*]

SERIBA GHATTAS IN DJUR, *July* 29, 1870

AFTER an absence of nearly eight months I have arrived here once more, considerably reduced in bulk in consequence of the privations and fatigues which I have had to undergo, but otherwise thoroughly well and active. A poultry yard and a milch cow, which I intend to provide myself with, will, in addition to a few weeks' rest, restore my lost strength completely. The journey to the Niam-Niam country, which I undertook as the guest of my friend, Mohammed Abu Tsammat, with his ivory caravan of 300 men, and whose acquaintance I made during the river journey, was successfully completed, as we had no losses to deplore, except a few female slaves who were taken away whilst fetching water; and besides the wounding of the leader, Mohammed, only one of my people was injured by an arrow, which struck him in the arm, but fortunately the wound was speedily healed.

The climate of the country traversed by us is an exceedingly salubrious one, and my people as well as myself enjoyed the best of health. I had only reason to complain, and that bitterly, of two things, viz. the numberless, excessively tedious, and disagreeable passages across the rivers, rivulets, and swamps, and the want of a sufficiency of food, which I experienced during the whole of the journey. In the southern part of my route such passages occurred every quarter of an hour, taking sometimes hours to complete. My donkey, which I have brought back in thorough good health, was consequently of little or no use to me, as I should have had to dismount continually. The waters are here, contrary to the otherwise steppe-like character of the country through which we journeyed, invariably surrounded and overshaded by dense masses of trees; a small footpath leads through the thicket; broken boughs and stems of trees from three to four feet thick lie about in all directions, over which it is necessary to clamber or stumble. Wading up to the hips in the black mud of the swamp, and at the same time passing through the prickly bushes, especially the Pandanus and Kotany (Calamus or bamboo) which fetched blood at every step, I was unfortunately not able, like Speke, to take my clothes under my arms, as the hands were as essential as the feet in helping one forward. My large hat was my only clothing. At length, when I had crossed over, clean water had to be sought for ablutions; then when I had got rid of the black tint, which made me look like a moor, I had frequently to remove leeches, of the thickness of one's finger, which had fastened themselves to my legs. How greatly I regretted not being able to dispense with my trousers, at least, to avoid the excessive annoyance which the constant dressing and undressing caused, but the sensitive epidermis of the adult European does not so easily accustom itself to the roughness of the path, and the sudden changes in temperature require to be guarded against as carefully as a Russian summer does.

Our dietary arrangements were, as I have already hinted, but of a very moderate character. Amongst the real Niam-Niam people there was, it is true, durrah corn, and upon the outward journey there was abundance of root-vegetables, such as cassavi, colocasia, and admirable yams (on the return journey, unfortunately, all these had been devoured or returned to the earth); but, on the other hand, there was an utter absence of cattle, the only flesh that could be obtained being that of fowls. At King Munsa's there were goats, but no corn. If I had not occasionally found time for hunting—in which, upon my return journey, I was successful in meeting with large numbers of antelopes—I must have starved. This want of provision was the more keenly felt, through the constant partial immersions sharpening the appetite; and on account of the difficulties of the march we were only able to have one meal a day, so that one's stomach was never thoroughly satisfied. My butter, which I had exhausted, I was compelled to substitute with goat-fat, and later on even with oil. Fortunately I always preserved a stock of tea and salt.

I travelled from here to Seriba Sjabbi, several days' journey to the south-east. After travelling for days through nothing but desert, we reached the territory of the principal chief of the Niam-Niams, called Nganji, with whom Abu Tsammat stands upon a friendly footing. Farther on, we passed through a district which is quite under the subjection of the latter, and governed by a former Niam-Niam soldier placed there by him. A Seriba and thirty warriors suffice to maintain his authority in this tolerably populous district. From here we traversed the country under the rule of the powerful chief Uando. Notwithstanding threatening rumours, we found him peaceably inclined, and he offered me as a present a large pot containing the entrails of an elephant a hundred years old, which my people, to whom I handed the delicacy, assured me was very tough and rather high. After passing through another desert for several days, we reached the territory of the Mombuttu King, Munsa, whose residence was the most southern point reached by me, situated a little beyond the third degree north latitude. The southern part of it lies on the great Uelle river, which appears to me to be the upper Chari, flowing into the Tschad lake, and which resembles the Blue Nile, near Chartum.

I could fill volumes were I to relate all my experiences at the court of this wild brown Cæsar, covered all over with red copper spangles, and looking like a well-furnished kitchen; of his numerous wives, painted in all the colours of the rainbow; of his immense palace, resembling a railway station, one of the rooms of which, and where I was first received, being 100 feet long by 50 feet broad, and 40 feet high. It would be impossible for me, however, to pass over in silence the horrible cannibalism which is here, as well as among the real Niam-Niams, everywhere in vogue. Munsa dines off human flesh every day of his life; the Mombuttu people make regular battues upon the wilder negro races in the south, where those that are killed are at once cut up, the fat is melted down, and the flesh dried. Those that are captured are driven off to be slaughtered at convenience.

The Niam-Niams are thrown more upon their own resources. If, however, there should happen to be a cessation of internecine feuds, they attack the Nubian caravans, although it should be to their interest to keep the peace, as they are well paid for their ivory and provisions with copper and glass beads, and their chiefs receive rich presents. It is true the Nubians are not so philanthropic in their seribes, but in respect to the Niam-Niams, nothing can be said against them, as hostilities would destroy the object they have in view. The buried ivory cannot be discovered by any divining rod; there are no cattle to be stolen; and the women and children always hide themselves at once, and in time, in the impenetrable thicket of the woods, so that no booty is to be obtained in slaves. It is, therefore, however improbable it may sound, the Niam-Niams who, entirely through their horrible lust for human flesh, commence the war. "Flesh, flesh!" is their war-cry, and a few female slaves, at least, who have lost their way in fetching water, are sacrificed to their cannibalism.

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[*] Translated from the *Cologne Gazette*.

The journey back was commenced by the same route. On touching upon Uando's territory once more, alarming rumours reached us. It was said that this chief had brought out the whole of his force of warriors to bar our passage; in fact we discovered that the first villages we came to were deserted; armed Niam-Niam warriors lurked everywhere in the tall grass, and approached within range of our guns. But they did not show themselves particularly desirous of entering into hostilities with us. At one of the next villages, where Abu Tsammat received from the head man some ivory which he had left behind him upon the outward journey, several Niam-Niam men pressed their services upon us as guides or parlementaires. I was fully convinced of the existence of treachery, and vainly endeavoured to persuade Mohammed to seize some of these spies, and hold them as hostages. He had to repent it bitterly. After a short time they proved themselves to be assassins sent out by Uando, as the chief fondly imagined that the caravan would fall into his possession upon the death of the leader. Mohammed rode in front upon his mule; close behind him came the Niam-Niams. I followed a few paces behind them, and carried my gun myself, whilst Mohammed, according to custom, had his carried after him. All at once I heard shots, and saw Abu Tsammat fall from his saddle covered with blood. One of the Niam-Niams had given him a thrust with his spear; the assassins made off, and were lucky enough to escape, notwithstanding the shots that were sent after them, as there could be no question of pursuing them into the thicket. At the next village halt had to be made for the purpose of rest. The place was almost entirely in flames, and an entrenchment was made from the *débris* of the houses. Fortunately, the wound of the leader of the caravan, although a very severe one, it having been increased by drawing out the barbed point of the spear, was not very deep. With a number of entomologist's pins which I happily possessed, I managed to sew it up, and in three days' time the wound was nearly closed, and would have been soon completely healed if Mohammed could have kept himself quiet. During our enforced stay at this place we were frequently alarmed by demonstrations on the part of the enemy, but they could not summon up resolution to attack us seriously.

Busier times soon followed. The most serious part of the journey was the passage across the rivers, which, although we now followed a more easterly direction in order to avoid several of them, was occasionally used by the Niam-Niams for an attack upon us. The noise and shouting may well be imagined, when for instance a female slave completely disappeared with her burden in the flood, the beating of the Nubian soldiery, the clatter of the pumpkin shells and kettles; all this increased by a hail of arrows hurled by unseen hands from the adjacent thickets. However, we passed through without any loss; the enemy did not venture upon approaching near enough to hurl his costly iron projectiles, but contented himself with bamboo arrows with heads of hard wood. Another division that was allied to us, but which had separated itself from us on the outward journey, was not so fortunate, as whilst it was endeavouring to join us upon the return journey, it was attacked during the passage over the river by an overwhelming force. The leader and several of the soldiers were killed outright, others were severely wounded, so that the company was compelled to leave many valuable articles behind, in order to get out of the swamp as quickly as possible, and thus secure its retreat. After a very fatiguing march, I thus at length reached Seriba Tsabbo once more, where I intend resting for some weeks to recruit my strength, and to complete my collections and correspondence. Upon the road I had to cross once more the river Tondji, the passage of which I have made so frequently. As there is an utter lack of boats, all the baggage has to be conveyed on little rafts, each of which is steered by a swimming negro across the raging torrent. I can only express satisfaction with the result of my journey, although the direct distance travelled was not very great, being from here to Munsa's town about seventy-five German miles. I became acquainted with races, which, until very recently, had never come into contact with European and Oriental civilisation in the slightest degree, and who had developed for themselves a perfectly independent state of cultivation, so strange and uncommon that one imagined himself in a new world when among them. Not a scrap of European clothing, not a single glass bead remains with Mombuttu to remind one of the connection opened up by Mohammed Abu Tsammat a few years ago. Extraordinary to relate, there was no trace whatever to be found there of the great lake mentioned by Piaggia, and previously by von Heuglin, although we met with various tribes of the Niam-Niams, and were well supplied with interpreters. I have naturally laid down my route carefully, have made a collection of words of the different languages spoken by the races visited by me, and have taken the dimensions of numberless individuals, amongst others, several of the Acku dwarfs, whom I met at the court of Munsa, and one of whom I took away with me as my faithful attendant. The remains of the Mombuttu feasts furnished several skulls for my collection. The booty in plants was also a very extensive one. I have made up my mind, upon important grounds, to remain here for another year, and to make another journey into the Niam-Niam country, but this time by a westerly route, in order to clear up several remaining doubts as to the geography of this country, which was never traversed before me by a single European.

G. SCHWEINFURTH

SCIENTIFIC SERIALS

Poggendorff's Annalen, 1870, No. 8—The following are the contents of this number: (1) "Thermochemical Researches" (sixth, seventh, and eighth parts), by Julius Thomsen. This forms the conclusion of Thomsen's researches into the thermal effects of the neutralisation of acids, and ends with a collective statement of results. The experiments relating to what the author calls chemical "avidity" are likely seriously to modify commonly-received views of chemical action, showing as they do that the heat of combination between acids and bases is not a measure of their tendency to combine. (2.) "Researches relating to Electrical Discharge," by W. von Bezold. Experiments relating to the propagation of sudden electric waves in branched conductors. The author finds, among other results, that the velocity of such waves is independent of the material of the conductor; his experiments also indicate the existence of electrical phenomena analogous to the reflexion and interference of waves. (3.) "On the Electro-motive Force of the Voltaic Arc," by W. von Bezold. Edlund has shown that the electric light plays the part, not merely of a resistance interposed in the circuit, but also of an inverse electro-motive force. Von Bezold attempts an explanation of this fact, founded on the consideration that the discharge between the carbon-points must be periodic instead of continuous, and therefore their difference of tension a variable magnitude, whose maximum exceeds the electro-motive force corresponding to the resistance of the arc and the mean strength of the current. (4.) "On the Theory of the Electrophorus Machines and of the Supernumerary Conductors," by P. Riess. (5.) "On the Specific Heat of Water in the neighbourhood of its maximum density," by L. Pfaundler and H. Platter. The authors determined the specific heat of water between 0° and 11° C. by mixing weighed quantities at known temperatures between these limits, and observing the temperature of the mixture. From their results, they calculate an empirical formula containing the fourth power of the temperature. Taking the specific heat at 0° as 1, they find that at 1°·25 it is only 0·9512, while at 6°·75 it is 1·194, and at 11° it is again as low as 1·0298. (6.) "Acoustical Studies of Flames," by E. Villari. The author found that the tone of a vibrating tuning-fork was reinforced when brought near to a large gas-flame. When the flame, which was thus thrown into sympathetic vibration, was looked at through radial slits in a rapidly revolving opaque disc, it was found that, if the rate of rotation of the disc bore the proper relation to the rate of vibration of the fork, the flame appeared to be divided by stationary bands showing alternate maxima and minima of brilliance. When the rate of vibration was changed, but all other circumstances remained unaltered, the distance between the bands was found to vary inversely as the rate of vibration. (7.) "On the ratio of transverse contraction to longitudinal extension," by Heinrich Schneebeli. The author has applied Kundt's mode of measuring the length of stationary waves to the comparative measurement of the rates of the torsional and longitudinal vibrations of steel rods, and hence to the determination of the ratio between the transverse contraction and longitudinal extension produced by stretching forces: the mean results agree closely with those obtained by Kirchhoff and Okatow, as well as by Everett (Phil. Trans. 1867), from experiments on flexure and torsion. (8.) "On the compensation of an optical difference of path," by J. L. Sirks. An investigation of the condition under which the interference tints produced by polarised light passed through a thin plate of crystal can be achromatised by a compensating plate of selenite. (9) "Rejoinder to Dr. Most," by L. Boltzmann, relates to the second law of thermodynamics. (10.) "A contribution to the doctrine of Molecules and to the theory of Electricity," by C. Lorenz. An attempt to calculate the absolute number of molecules in a milligramme of water, founded upon Weber and Kohlrausch's absolute measurement of the electro-chemical equivalent of water and on the difference of potentials required for its electrolysis. (11.) "A contribution to the theory of Terrestrial Temperature," by O. Frölich. A discussion of Poisson's expression for the internal temperature of the earth at small depths below the surface, as a function of the time and the superficial temperature. (12.) "Remarks on the 'Bohemian Diamond,'" by Prof. V. L. von Zepharovich. The author states that only *one* diamond (not several, as has been implied in some reports) has been found in Bohemia; that this was discovered in a workshop in Dlaschkowitz, where pyropes (garnets containing chromium) are ground and bored with the help of diamonds; and that it is not yet ascertained how it came to be among the pyrope-sand in which it was found. (13.) "A remarkable stroke of Lightning," by Dr. J. G. Fischer. By examining the position of the magnetic poles in various pieces of iron and steel which were magnetised by the passage of the discharge, the author ascertained that the direction in which the negative electricity passed was downwards into the ground. (14.) "On the ratio of the specific-heat of air at constant volume to its specific heat under constant pressure," by Dr. Witte. The author concludes, on experimental and theoretical grounds, that this ratio is not constant, but is a function either of the temperature, or of the pressure, or of both. (15.) "On the minimum of prismatic deviation," by A. Kurz. (16.) "An easy mode of preparing a liquid for the production of Plateau's Equilibrium-figures without weight," by Rudolph Bottger.

The *American Naturalist* for December opens with a paper on the Flora of the Prairies, by Mr. J. A. Allen, in which he gives an interesting sketch of some of the peculiarities of the primitive flora of the Upper Mississippi prairie in northern Illinois, and central and western Iowa, not inaptly termed "the Garden of the West." He remarks that the breaking and turning of the soil at once exterminates a number of the previously dominant species, and instead of lingering as troublesome weeds, the more hardy exotics that through man's influence assume an almost cosmopolitan habit, usurp their places, the cereals, the cultivated grasses, and the noxious weeds of the old world, thoroughly crowding out the original occupants of the soil. Dr. W. Stimpson follows with an article on the Distribution of the Marine Shells of Florida; and Mr. A. S. Packard with one on the Borers of certain Shade trees. Spring time on the Yuron, by Mr. W. H. Dall, gives an account of the sudden advent of summer in that territory. Mr. A. S. Collins on the Impregnation of Eggs in trout-breeding will be interesting to pisciculturists in this country, explaining the principle of a new process pursued at the trout ponds in Caledonia, N.Y. The usual space is devoted to reviews and miscellaneous intelligence, and we have some further details of papers read at the Troy meeting of the American Association.

The *Journal of Botany* for January has increased the amount of its contents by a rearrangement of its type, without any corresponding increase in price. We are glad to observe that it is intended to devote the journal more exclusively in future to British botany, thus supplying a want long felt by workers in this department. In the present number there are several articles of interest, including a description (with plate) by Mr. Worthington Smith, of a new species of fungus gathered in Messrs. Veitch's cool fernery at Chelsea; Observations on the genus *Pottia* (of Mosses), by Mr. W. Mitten; a few notes on Mr. H. C. Watson's Compendium of the "Cybele Britannica," by the Hon. J. L. Warren; and a Monograph of the genus *Xiphion*, belonging to Iridaceæ, by Mr. J. G. Baker. There is also a useful epitome of Dr. M'Nab's important paper on the "Transpiration of Aqueous vapour by Leaves," to which we have already referred; and the column of short Notes and Queries will be found interesting and valuable.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, January 3.—Professor Huxley, F.R.S., V.P., in the chair.—Prof. Flower exhibited and made remarks on a mounted skull of the Common Sturgeon (*Acipenser sturio*), from the Museum of the Royal College of Surgeons, in which the cartilaginous portions had been replaced by a wooden model.—Mr. Tegetmeier exhibited and made remarks on a specimen (in the flesh) of a female of the Great Bustard (*Otis tarda*), which had been killed on the 29th ult. near Feltham, in Middlesex.—Mr. Gould exhibited and made remarks on a skin of Lady Rosse's Touraca (*Musophaga rossæ*), just received in a collection of birds from Loanda.—Mr. Wallace read some extracts from letters received from his brother, Mr. J. Wallace, containing remarks on the habits of a species of Lizard (*Phrynosoma*) and Rattlesnake (*Crotalus*), as observed in California.—A tenth letter was read from Mr. W. H. Hudson, on the ornithology of Buenos Ayres.—A letter was read from Mr. E. P. Ramsay, giving particulars respecting the habits of the new Australian Mud-Fish (*Ceratodus Forsteri*).—The Secretary read extracts from some correspondence between himself and Mr. G. W. des Vœux, Administrator of the Government of Santa Lucia, as to the best method of destroying the Poisonous Serpents (*Craspedocephalus lanceolatus*) found in that island.—Mr Sclater exhibited and made remarks on the horn of the male Rhinoceros, which that animal had torn off in the Gardens on the 10th August last.—Mr. Flower read some notes on the skeleton of the Australian Cassowary (*Casuarius australis*), in which the differences between the skull of that species and *C. galeatus* were pointed out. Mr. Flower's observations were based on the skeleton of this bird, transmitted to Mr. Sclater by the Messrs. Scott, of the Valley of the Lagoons, Queensland, and now in the Museum of the Royal College of Surgeons.—A communication was read from Mr. Andrew Murray, containing some notes on the structure of the young of the Sterlet (*Acipenser ruthenus*).—A communication was read from Mr. George French Angas, containing descriptions of thirty-four new species of shells from Australia.—A joint communication was read from Dr. G. Hartlaub and Dr. O. Finsch, on two collections of birds from the islands of Savai (Navigator group) and Rarotonga (Hervey group). Several new species were described in this paper, the most remarkable of which was a new form, allied to *Gallinula*, from Savai, proposed to be called *Pareudiastes pacificus*.

Geological Society, December 21.—Mr. Joseph Prestwich, F.R.S., President, in the chair. "On Lower Tertiary Deposits recently exposed at Portsmouth," by C. J. A. Meyer, F.G.S. The author described some exposures of Lower Tertiary deposits made during excavations for the "Dockyard Extension Works" in Portsmouth Harbour. The thickness exposed, exclusive of alluvial deposits, amounted in all to 127 feet. The beds dip S.S.W., or nearly south, 2½ to 3 degrees. The author grouped them under the following divisions, in ascending order:—

1. Clays and sands with pyrites, 36 feet.
2. Argillaceous sands with *Dentalium*, 25 feet.
3. Sands with *Lingula*, 8 feet.
4. Clays with *Cyprina* and sandy clays, 55 feet.

The author indicated the fossils contained in each of these divisions, remarking upon the range of some of the species, and upon the apparent mixture of London clay forms with others usually regarded as characteristic of higher or lower beds which occurs especially in the "*Lingula* sands." He suggested that, as the species found here present some slight differences from those occurring in other deposits, the difficulty might be got over on Darwinian principles. The author considered that the fossils did not furnish any satisfactory evidence of the true position of these beds; but, from stratigraphical evidence, he regarded them as being included in group 3 and part of group 4 of Mr. Prestwich's section of the Whitecliff strata in the Isle of

Wight. He concluded with some remarks on the superficial deposits consisting of gravel and old and recent mud overlying the Tertiary beds in the section described by him. Prof. Ramsay called attention to the value attaching to such observations as those of the author on the nature of the superficial deposits.—Mr. Etheridge observed that the presence of the *Lingula* determined the position of the Bognor beds in the series, though there appeared great difficulty in fixing it stratigraphically. The commingling of species exhibited in this instance of shells hitherto supposed to be peculiar to certain horizons, he regarded as very remarkable.—Prof. Morris observed that the section seemed to show, not only the order of the beds, but their manner of deposition, the whole having formed part of a tranquil sea-bottom. He remarked on the difficulty of separating the more recent mud deposits from the beds of more ancient date. He pointed out the method of formation of septaria apparently by segregation, as they sometimes included undisturbed parts of the beds. The number of bivalves bored by carnivorous mollusks was remarkable, as was also the absence of *Pectunculus*.—Mr. Gwyn Jeffreys observed on the habits of *Lingula*, which had been by some regarded as an annelid, and not as a mollusk. It afforded a curious instance of the persistence of species, as there was no distinction that could be established between those of the Crag and of Silurian times. It lived at the present time between high and low water mark, and the *Panopæa* at a slightly lower level, and probably had done so in Tertiary times. Mr. Evans inquired whether the upper gravel, like that on the shore of Southampton Water, contained any flint implements Mr. Meyer replied that he had not examined the gravels with that view.—"Note on some new Crustaceans from the Lower Eocene of Portsmouth, collected by Mr, C. J. A. Meyer, F.G.S." by Mr. H. Woodward, F.G.S. Mr. Woodward drew attention to the occurrence in the fossil state of pelagic forms of Crustacea armed with long spines on the latero-anterior angles of the carapace. Two Eocene forms had been described by Dr. Alphonse Milne-Edwards, namely, *Enoplonotus armatus* and *Psammocarcinus Hericartu*. Two new forms, differing generically from the above, but probably referable to the same family (the *Portunidæ*), were descibed, under the names of *Rhachisoma* (g n.), *R. echinata*, and *R. bispinosa*. A third form, belonging to the Corystidæ, was then noticed. This family, represented in the fossil state by the genus *Palæocorystes*, is well known in the Gault and Upper Greensand of Folkestone and Cambridge, one species ranging up as high as the Maestricht beds. The occurrence of *Palæocorystes* in the Lower Eocene is of great interest. Mr. Woodward named this new *Palæocorystes*, *P. glabra*. 3. "On the Chalk of the Cliffs from Seaford to Eastbourne, Sussex," by W. Whitaker, F.G.S.—The author compared the chalk of the Sussex coast with that of the Kentish coast, and stated that it consisted of the following divisions in descending order:—

1. Chalk with flints of great thickness.
2. Chalk with flints and nodular layers, weathering rough.
3. Chalk without flints, but with nodular layers, weathering rough.
4. Thick-bedded massive chalk without flints.
5. More thinly-bedded chalk without flints, but with marly beds.
6. Chalk-marl, 50 or 60 feet.

The highest of these divisions stretches as far eastwards as Beachy Head, and forms the whole of the cliffs to within a short distance of that point. 4. "On the Chalk of the southern part of Dorset and Devon," by W. Whitaker, F.G.S. The divisions of the chalk were traced by the author westward from cliffs on the north side of Swanage Bay to beyond Beer Head in Devonshire. At first the succession of the beds was shown to be as in the Isle of Wight, namely:—

1. Chalk with flints, very thick.
2. Chalk with few flints.
3. Chalk-rock, very thinly developed.
4. Chalk without flints.
5. Chalk-marl.

It was shown that the lower beds became thinner westward, until, at one part of the Beer Head section, the chalk with flints rested at once on the Upper Greensand; and the following general conclusions were drawn:—That the chalk-marl thins westward, and its bottom part becomes marked by the presence of quartz-grains, showing perhaps signs of a less deep-sea character than usual. That the chalk without flints thins westward (from about 200 feet in the Isle of Wight), until, in Devonshire, it is but 30 feet thick, or even less. The consequent nearness of the chalk with flints to the Greensand helps to explain the deposits of flints on some of the Devonshire hills. Mr. Etheridge pointed out the resemblance between the series described by the author and that of the chalk of Antrim. He thought it probable that the cretaceous beds had originally extended over the whole of Western England. He called attention to the Blackdown-beds, which had been regarded as Upper Greensand, but certainly were not so, though probably Cretaceous, as well worthy of examination. Mr. Hull hoped that some Fellows of the Geological Society would extend their examination of the chalk into Ireland, and visit the Antrim district. It was the case there that the Chalk with flints rested immediately on the Upper Greensand, though there was an intermediate band known as the Mulatto-bed, which might possibly represent the Chalk-rock. Prof. Morris thought the paper afforded evidence in favour of the Chalk having been deposited in a sinking area, and during the process various alterations in the conditions took place. Mr. D. Forbes inquired as to the character of the nodules mentioned, and whether they were siliceous or not? Mr. Meyer mentioned that near Branscombe there occurred a band within eight feet of the Red Marl, containing fossils apparently the same as those of Blackdown. Mr. Whitaker had purposely avoided characterising the greater part of the Greensand-beds as either Upper or Lower. He thought the Cherty-beds of the west were stratigraphically higher than those of the Isle of Wight. The nodules inquired about were not siliceous, though probably containing some silica, but were rather phosphatic.

Anthropological Society, January 3. Dr. Charnock, V.P., in the chair.—Captain C. C. Poole, of Myansung, Pegu, was elected a Fellow. Professor Cav. Luigi Calvri, of Bologna, was elected a corresponding member.—Mr. Joseph Wilkinson exhibited and described a collection of human remains, weapons, and other works of art, found in an Anglo-Saxon cemetery near Barrington, Cambridgeshire.—Dr. Richard King read a paper on "The Manx of the Isle of Man." The author treated of the physical and psychological characteristics of the people of the isle, who, he maintained, were a pure stock of the great "Keltic" division of mankind; of their history, superstitions, language, literature, and works of art, and the statistics of population. On the latter, however, further information was required, which Dr. King hoped the census of 1871 might supply.—A paper by Dr. Beddoe, president, was read "On the Anthropology of Lancashire." The pre-historic antiquities of Lancashire are rather scanty, and the early and mediæval history of the north-west of England is remarkably barren as compared with that of the north eastern district. The inhabitants of Salford were of Teutonic character, having been colonised during the Roman period by a cohort of Frisians, a few Danish, and other Scandinavian elements being present. The latter appear to be the strongest. The Saxon, or Angle, is in some force, as is also the Keltic, which, however, seems to have been partly Gaelic, and not wholly Kymric, as might, perhaps, have been expected. The effect of the Norman Conquest on the race elements in Lancashire would probably be inconsiderable, though there, as elsewhere, the Anglo-Danish, or Anglo-Norse, aristocracy, may have been somewhat more diminished, by slaughter and emigration, than the commonalty, whose blood may have had a larger admixture of the Keltic element.

Entomological Society.—Mr. Alfred R. Wallace, President, in the chair. Dr. Ross, of Toronto, was elected a member. The fourth part of the Transactions for 1870, published in December, was on the table. Exhibitions of British *Lepidoptera* were made by Mr. W. C. Boyd and Mr. Verrall; and of West African *Lepidoptera*, by Mr. Butler. A paper by Mr. Hewitson was read, entitled, "New Species of South American Diurnal *Lepidoptera*."

BRISTOL

The Observing Astronomical Society.—Report of observations made by the members during the period from August 6 to October 7, 1870, inclusive. (Continued from page 40.)

Aurora Borealis.—Mr. John Birmingham, of Millbrook, Tuam, writes:—"Though the night of the 24th September, when there was no moon and a densely cloudy sky, ought to have been extremely dark, it was, on the contrary, about as bright as if the moon were full, and the sky was similarly overcast. This extraordinary brightness began to decrease at eleven o'clock, and continued diminishing up till midnight, when the clouds cleared away, and revealed an Aurora Borealis of rare splendour, though it was doubtless inferior to what, unfortunately, the state of the sky prevented from being observed previous to eleven o'clock. The auroral arch was wanting, but long beams extended up from its usual position, and brilliant coruscations were flashing almost on every side to the zenith, even at times from the south. At precisely $0^{h}\ 40^{m}$ G.M.T. there was a well-defined corona, with its centre exactly at *Beta Andromedæ*. By means of a lamp held at some distance, with the aurora as a background, I was enabled to position three spectroscope lines with a small pocket instrument. These were the usual bright band in the green, a very faint one near it on the left, and one of medium brightness near F. On the night of October 14, during another remarkable display, when the intense red of broad areas of light did not seem enfeebled by the rays of a very bright moon, there was no indication of a red line in the spectroscope. In fact, there was here no line whatever to be detected, and the white light seen in some parts of the sky gave only the one principal line in the green." Mr. H. Michell Whitley, of Truro, reports that on September 21 he observed aurora-parallel streaks of a rosy hue reached to an altitude of about 30° fading away and reappearing in fresh position. On September 24 another aurora was observed by him. "At times the streaks almost reached Polaris. On the following night a repetition of the phenomenon occurred. He observed that the streamers, after fading and disappearing, would again appear in all their beauty in fresh positions, when the sky would glow like a furnace, lighting up the surrounding scenery, as if it were illuminated by the reflection from some distant fire. About $8^{h}\ 15^{m}$ a beautiful rosy beam enveloped Capella, whilst a fiery, glowing cloud lay a little to the east of it. At this time the display was at its maximum degree of splendour." Exhibitions of aurora were also observed by the Rev. S. J. Johnson, of Crediton, Mr. William F. Denning, of Bristol, and other members of the society.

Occultation of the Planet Saturn.—Mr. E. B. Knobel writes that this phenomenon was observed exceedingly well at Burton-on-Trent. "The disappearance occupied $1^{m}\ 10^{s}$. There was too much twilight for me to pick up Titan. The reappearance was observed to perfection, as far as atmospheric influences went, and, notwithstanding the low altitude, definition was very sharp as the planet emerged behind the bright limb of the moon. Ball's division, the shadow of the planet on the ring, and belt across the planet was quite distinct. The colour or rather the comparison between the colours of the moon and Saturn was decidedly different from that noticed at the April occultation. In consequence of low altitude, Saturn appeared of a light *liver* colour by the side of the yellow moon, whereas in April the colour of Saturn was more greenish. According to my rough observation, the time during which Saturn was *totally occulted* was $1^{h}\ 11^{m}\ 35^{s}$." The Rev. S. J. Johnson, of Crediton, reports that at $4^{h}\ 40^{m}$ he first observed the moon, but could not make out the planet before a quarter to six. "Saturn was then visible though somewhat faint, with a power of seventy on a 2¼in. aperture; with 150 it almost faded away in the field of view. The planet appeared very dull at its emersion." Mr. George J. Walker, of Teignmouth, witnessed the disappearance of the planet with a 2in. O.G., power 32. The Moon totally obscured Saturn at $5^{h}\ 45^{m}\ 45^{s}$, town mean time, which equals $5^{h}\ 59^{m}\ 44^{s}$ G.M.T. Mr. J. C. Lambert, of Sleaford, gives the following times of the disappearance of the planet and its ring:—First contact with ring, $6^{h}\ 4^{m}\ 2^{s}$; first contact with globe, $6^{h}\ 4^{m}\ 14^{s}$; disappearance of globe, $6^{h}\ 4^{m}\ 50^{s}$; disappearance of the ring, $6^{h}\ 5^{m}\ 6^{s}$.

Jupiter.—Mr. A. P. Holden, of London, writes: "The chief feature in the belt system of this planet has been the darkness of the permanent belt, which lies midway between the equator and the N. pole. Up till about the end of September it was so dark as to be readily visible with a very low power. Since then it appears to have brightened somewhat." Mr. H. Michell Whitley observed the planet on September 20, 11^{h} to $11^{h}\ 30^{m}$: "The equatorial zone is of the same copper colour as during the last opposition, and of quite as deep a shade. The streaks N. and S. of this zone pale yellow. The narrow sharply-defined belt N. of the N. yellow streak is of a finer tint than in the spring of this year, being of a fine purple grey, with a very perceptible tint of rose colour in it – N pole, grey. The narrow band S. of S. yellow streak not as fine a colour as the belt N. of N. yellow streak, being purple grey—S. Pole, grey."

The Nebula in the Pleiades.—Under very favourable atmospheric circumstances, Mr. Albert P. Holden, of London, has had a very careful search for this object with his 3in. refractor of very fine definition. He says: "Although favoured with good eyesight, I entirely failed to pick it up. Upon comparing the image of Merope, as seen in the telescope with that of the *lucida* of this group, a very perceptible difference was observable. The rings surrounding Merope were more numerous, and had a very misty, ill-defined appearance; they were also markedly extended in a direction N. of the star. With these exceptions not the slightest traces could be found of the nebula. It has been seen with only 2in. Webb saw it readily on October 6, 1863, but found it 'very feeble' on September 25, 1865. Any member of the society, working with a large aperture, would confer a favour by searching for this object."

Coggia's Comet.—This comet has been seen on several occasions by Mr. George J. Walker, of Teignmouth. On the 29th and 30th of September it was in the field with a large number of eight and nine mag. stars. "It looked like a tolerably bright globular nebula."

MANCHESTER

Literary and Philosophical Society, December 27, 1870—E. W. Binney, F.R.S., President, in the chair. "Observation of the Eclipse of the Sun, December 22, 1870," by J. B. Dancer, F.R.A.S. The eclipse of the sun on Thursday, the 22nd of December, was favourably observed at Ardwick. Although a slight fog prevailed, all the details of the phenomenon were distinct, and tolerably well defined. A number of spots were visible on the sun's surface, two of which were of some magnitude. The nuclei of these spots were linked together by maculæ, and surrounded by a penumbra which extended to a considerable distance. Faculæ also were very numerous and distinct. The approximate times of contact taken by a chronometer corrected by the standard clock at the Town Hall were as follows:—first contact of the moon's limb with the sun $11^{h}\ 5^{m}\ 49^{s}$; Contact of moon's limb with nucleus of the first large spot, $11^{h}\ 31^{m}\ 36^{s}$; with the nucleus of the second large spot, $11^{h}\ 37^{m}\ 20^{s}$; last contact of moon's limb with the sun, Greenwich mean time, $1^{h}\ 37^{m}\ 3^{s}$. The temperature during the progress of the eclipse was taken at intervals by a mercurial thermometer with a black bulb *in vacuo*, exposed to the sun at the height of 4 feet from the ground.

TIME. H.	M.	S.	TEMP. DEGREES.
11	10	0	31·5
—	35	0	30·25
—	45	0	29·75
—	50	0	29·25
12	22	0	27·2
—	35	0	28·5
1	37	0	29·0

I had an impression that the moon's edge could be traced a short distance from the edge of the sun at the upper and lower points of contact, but this might be imagination. The black surface of the moon appeared very uniform in colour. I tried with powers of 80 and 180 to distinguish the moon's disc, but did not succeed. Light clouds were passing over the sun's disc at this time. The diminution in light was quite perceptible at the time of the greatest phase. Mr. Baxendell said that he observed the commencement of the Eclipse at Cheetham Hill. The first contact took place at $11^{h}\ 5^{m}\ 46{\cdot}2^{s}$ G.M.T. or 24·2 seconds later than the time calculated by Mr. Dickinson and Mr. Hind. The definition of the limbs of the sun and moon, and of the spots on the solar disc, was remarkably good, and he did not think his observation of the time of first contact could be in error to the extent of one second. The limb of the moon on the sun's disc appeared to be more sharply defined than the sun's limb. No distortion of the cusps was noticed. Unfortunately he was obliged to leave the observatory before the end of the eclipse, and therefore did not observe the time of last contact.

EDINBURGH

Royal Society, December 19.—Dr. Christison, the President, in the chair. The following papers were read:—1. Additional Remarks on the Theory of Capillary Attraction, by E. Sang, Esq., C.E. 2. Laboratory Notes: On Thermo-Electricity, by Prof. Tait. An endeavour to prove, experimentally, that the electric connection of heat is proportional to the absolute temperature, and an application of this result to the measurement of high temperatures. (3 and 4.) Note on Linear Differential Equations in Quaternions, and Note on some Quaternion Integrals, by Prof. Tait. (5.) Note on an Ice Calorimeter, by Prof. Crum Brown. The author had, some years back, ordered the construction of an instrument on the same principle as that lately described by Bunsen. It is not yet completed, and he sent this note, not of course to claim priority, but to reserve to himself the right to use his own instrument.

Royal Physical Society, December 21.—Mr. R. F. Logan in the chair. The office-bearers for the session were elected as follows:—Presidents: R. F. Logan, C. W. Peach, Dr. Robert Brown. Council: James M'Bain, M.D., R.N.; Stevenson Macadam, Ph.D.; Andrew Wilson, Robert Scot Skirving, David Grieve, Professor Duns. Secretary: John Alexander Smith, M.D. Treasurer: Henry Budge, C.A. Assistant Secretary: James Boyd Davies. Honorary Librarian: Andrew Taylor. The Secretary exhibited the head of a roedeer, with the upper part of each horn bent backwards into a hook shape, or rather a complete loop; probably due to an injury when the horns were growing. He also exhibited a curious large specimen of a pigeon, with a bluish-grey head, mottled with white, and rest of plumage nearly white, the back and wings mottled with a few darker feathers; tail large, nearly white; the breast and abdomen show traces of the reddish colour of the cushat, and the sides of the neck also showed the bright white spot, slightly bordered with green and red reflections, as in the cushat. It was shot in company with a flock of wood pigeons at Aldrught, near Elgin, in December, 1869. The bird has been supposed to be probably a hybrid between the cushat, or wood pigeon, and a fancy domestic pigeon, as a pouter, being rather larger in size than the cushat. If this were so, it is perhaps the first instance of the kind that has been observed; but probably a much more simple explanation may be given by considering it simply an albino cushat, or variety showing the plumage much changed to white. Very little variety occurs in the plumage of the cushat, so that the specimen is a rare one. The secretary also exhibited a specimen of the *Labrus mixtus*, or cuckoo wrass, taken on a long or hadock line in the Firth of Forth, in September, 1870. It is common on some rocky coasts, and specimens have been taken acording to Mr. C. W. Peach, at Wick, Iverach, and Kirkwa, in Orkney, but has apparently not before been recorded as taken in the Firth. The *Labrus trimaculatus*, or three-spotted wrass has, however, been taken oncee or twice in the Firth of Fourth; and, according to Dr. Gunthr, in his valuable "Cataloge of Fishes," the latter is not a distinct species, but simply the female of the *Labrus mixtus*.—Mr. Robert Brown, Ph. D., M.A., submitted some recent observations regarding the Aretic marine currents. The author considered that there were three main currents, which traversed th Arctic, American and European seas, exclusive of those of Asiae and Behring Straits. These were:—1. The current sweeping out of the Kara Sea to the westward, getting deflected against the Greenland coast; sweeping down that coast at the averag rate of eight knots an hour, varying according to the season, doubles Cape Farewell, and then runs north along the western shores of Greenland, decreasing in rapidity and in breadth from about 100 miles, to which it stretches at Cape Farewell, until its force is exhausted at near Disco Island. This current jams up the eastern shores of Greenland, within which it is always on the move summer and winter, and as seen rounding Cape Farewell is known to Davis Strait navigators as the "Cape Ice." It brings into Davis Straits great quantities of driftwood and Polar bears. 2. A current down Davis Strait. About Rifkol it is deflected off to the westward, and flows down the western shores of Davis Strait, carrying down great quantities of icebergs, which strand and melt on the banks of Newfoundland, there depositing their loads, others helping to form these banks. Here it meets with the Gulf Stream—the meeting of the cold and warm currents giving rise to the fogs so characteristic of that locality. At the mouth of Davis Strait there is an indraught of the Gulf Stream, which joins the Cape ice on the Greenland coast. It is also to this indraught that the drift mahogany logs, now and then picked up on the Greenland coast, are due. 3. The Gulf Stream, with the exception of the indraught already mentioned, does not enter Davis Strait, but sweeps across the Atlantic, retaining some degree of warmth as far as Novai Zemlai, and landing tropical products on the shores of Iceland and Spitzbergen. It is to this current that is due the freedom of the harbours of Norway and South-Western Iceland from ice.

BOOKS RECEIVED

English.—The Heavens; A. Guillemin, 4th edition, edited by J. N. Lockyer (Bentley).—Travels in the Air: Jas. Glaisher, C. Flammarion, W. de Fonvielle, and G. Tissandier (Bentley).—A Treatise on Magnetism: G. B. Airy (Macmillan).—The Student's Elements of Geology: Sir C. Lyell (Murray).—The Marvels of the Heavens: C. Flammarion, translated by Mrs. Lockyer (Bentley).—Elementary Natural Philosophy: J. Clifton Ward (Trubner).—The Duke of Edinburgh in Ceylon (Provost and Co.)

Foreign.—Jahrbuch der k. k. geologischen Reichsanstalt zu Wien, 1870. (Through Williams and Norgate)—Die Mineralien: J. C. Weber.—Die Fische Deutschlands und der Schweiz: J. C. Weber.

DIARY

THURSDAY, January 12

Royal, at 8.30.—On Fluoride of Silver, Part II.: G. Gore, F.R.S.—Polarisation of Metallic Surfaces in Aqueous Solutions; Some Experiments on the Discharge of Electricity through Rarefied Media: C. F. Varley.
Society of Antiquaries, at 8.30.—Election of Fellows.
London Mathematical Society, at 8.—On Systems of Tangents to Plane Cubic and Quartic Curves: J. J. Walker.—On the Order and Singularities of the Parallel of an Algebraical Curve: S. Roberts.

FRIDAY, January 13.

Royal Astronomical Society, at 8.
Quekett Microscopical Society, at 8.

TUESDAY, January 17.

Zoological Society, at 9.—On a Skull of a Narwhal with two tusks, in the Cambridge University Museum: John W. Clark.—Descriptions of some new species of Australian Land Shells: Dr. James C. Cox.—Notes on some points in the Osteology of *Rhea Americana* and *Rhea Darwinii*: Dr. R. O. Cunningham.
Royal Institution, at 3.—Nutrition of Animals: Dr. M. Foster.
Statistical Society, at 7.45.—On the Comparative Taxation on Real Property, Personality, and Income: R. Dudley Baxter, M.A.
Anthropological Society, at 4.—Annual Meeting.

WEDNESDAY, January 18.

Meteorological Society, at 7.—Heights and Velocities of August Meteors in 1870: Prof. A. S. Herschel.—Lunar Influence upon Rainfall: John C. Bloxam.—On Prof. Poey's new Classification of Clouds: Dr. R. J. Mann.
Society of Arts, at 8.
Royal Society of Literature, at 8.30.—On Prospero's Clothes-line (by A. E. Brae): Dr. C. M. Ingleby.

THURSDAY, January 19

Royal Society, at 8.30.
Society of Antiquaries, at 8.30.
Linnean Society, at 8.—On the Vegetation of the Solomon Islands: Mr. Atkin.—Note on Byrsanthus (*Homalineæ*): Dr. M. T. Masters, F.R.S., F.L.S.—Historical Notes on the *Radix Galanga* of Pharmacy: Daniel Hanbury, F.R.S., F.L.S.
Chemical Society, at 8.
Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

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Erratum.—Page 182, first column, line 34, for "we are justified," read "we are not justified."

THURSDAY, JANUARY 19, 1871

THE MEDITERRANEAN ECLIPSE, 1870

CLOUD in Sicily, cloud in Spain, cloud in Africa. Such at first sight might seem to be the only result of all the observations made on the eclipsed sun of 1870; such the reception given by Nature to those who wooed her as she had never been wooed before, who approached her full of the rarest gifts which Science has placed at man's disposal.

But, after all, has the oracle been silent? I think not. Dare we, however, say that the great problem of the Corona, that one among the many still outstanding difficulties which the eclipse was invoked to settle, is settled? This, perhaps, would be saying too much, but still, I think, a step in advance has been made. The oracle has spoken darkly perhaps, but it *has* spoken.

Let me endeavour to put the question as it stood a few weeks ago as briefly as possible.

Beginning the story some few years back we find the corona, a halo of white light round the moon, with a height sometimes represented as equal to the moon's diameter, sometimes more, sometimes less, with a border *à discrétion*,—so much did the drawings vary—regarded as the solar atmosphere.

Some thought the red prominences to be mountains, other observers called them clouds.

The polariscope was brought up with a view of determining whether the corona shone by reflected light or not. The result of this new method of observation was doubtful.

In the Indian eclipse of 1868 M. Janssen, by means of the spectroscope, still another aid, determined that the prominences were masses of hydrogen gas, but there was no final word about the corona. Major Tennant observed that its spectrum was continuous. Later in the same year Dr. Frankland and myself approximately determined the pressure of the prominence gases by means of the new method and laboratory experiments, and at once stated our conviction that the extensive corona which had been depicted and represented by Kirchhoff and others to be the solar atmosphere must be something else. This was our idea. I cannot quote our words, for I am writing in Venice and have no copies of our paper with me.

In the American Eclipse of 1869 the problem was advanced considerably, perhaps even more considerably than we can yet form an idea of, writing as we must still do doubtfully. I do not refer to the drawings, for they varied considerably, but to the observation that the light of the outer corona, like that of the prominences, gave a bright-line spectrum. But as at least some of the observers gave positions doubtfully, "near C" and "near E," I thought that the explanation was still possible which regarded the corona as of terrestrial origin; that is, which assumed it to be an appearance due to the presence of light in our own atmosphere. The problem was one of such difficulty that there seemed a possibility that, by some unexplained cause, some of the solar light might be diffused and beat out of its course, and then, mixing up with the light of the chromosphere, give us a sort of continuous spectrum, with the hydrogen bright lines superposed upon it; in other words, that as the eye perceives a bright, irregular region or glare around the uneclipsed sun, an effect due to our atmosphere, so also the eye might perceive a bright, irregular region or glare round the *uneclipsed chromosphere during eclipses*, due also to our atmosphere.

One word here about the Chromosphere, the name I have given to the bright-line-giving region outside the photosphere. It has long been clear that the spectroscopic method of observing it when the sun is not eclipsed is not totally effective; that is to say, that we only see a percentage of it—perhaps only a relatively small percentage—but the glowing prominences, that is, those in which there is no evidence of the rapid motion of ejection from the sun, the ejection taking place at all angles from the line of sight, afford evidence that there is probably a layer of cooler hydrogen susceptible of being rendered visible above the ordinary level. Now as these prominences may be 5′ high, it is not unreasonable to suppose that the chromosphere may even extend to that distance, or even a little beyond it.*

Hence it was that in the Instructions to Observers, drawn up by Professor Stokes and myself, and approved by the Organising Committee for this 1870 Eclipse, it is stated that—

"The PRINCIPAL OBJECT to be obtained is to determine whether it is possible to differentiate the outer layers of irregular outline and the streamers (of the corona) from a stratum, say some 5′ or 6′ high, round the sun, which may possibly be the limit of the gaseous envelopes above the photosphere."

The spectroscopic observers, therefore, were enjoined—

a. "To determine the actual height of the chromosphere as seen with an eclipsed sun; that is, when the atmospheric illumination, the effect of which is doubtless only partially got rid of by the Janssen-Lockyer method, is removed. If the method were totally effective, the C line, the line of high temperature, should hardly increase in height; but there can be little doubt that the method is not totally effective, so the increase in height should be carefully noted."

b. "To determine if there exists cooler hydrogen above and around the vividly incandescent layers and prominences."

And the polarisers—

"To examine a detached and selected part of the corona about 6′ from the limb of the sun, and say about 8′ in diameter."

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* Here is what I wrote on this point a year ago:—"I next come to the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere. This is considerable; in fact, the evidences of it are very much stronger than one could have wished, but hardly more decided than I had anticipated. Professor Winlock's evidence on this point, in a letter to myself, is as follows:—'I examined the principal protuberance before, during, and after totality. I saw three lines (C, near D, and F) before and after totality, and eleven during totality; *eight were instantly extinguished on the first appearance of sunlight.*'" This effect was observed with two flint prisms and [illegible] inches aperture. Professor Young, with five prisms of 45° and 4 inches aperture, found the same result in the part of the spectrum he was examining at the end of the totality. He writes:—'I had just completed the measurements of 2602, when the totality ended. *This line disappeared instantly*, but 2796 [the hydrogen line near G] was nearly a minute in resuming its usual faintness.' These observations I consider among the most important ones made during the eclipse, for they show most unmistakeably that, as I have already reported to the Secretary of the Government Grant Committee, the new method to be employed under the best conditions must be used with large apertures and large dispersion." (Proc. R. S. 1870, p. 181.)

Having got so far, it may be here stated that of the three means of attack, namely, the spectroscope, the polariscope and telescope, and naked eye observations, the spectroscopic method, under certain circumstances, might have been by far the most doubtful, the polariscope method coming next.

With regard to the spectroscopic observations, if we assume that no light whatever is received by and from our own atmosphere, the observations would be easily translated. A pure continuous spectrum would reveal to us solid or liquid matter in the circumsolar regions; a spectrum continuous or not containing bright lines would give us gases or vapours; the ordinary solar spectrum, with its dark lines, would indicate matter incapable of radiation itself, and therefore cool, reflecting to us ordinary sunlight. It is clear that the problem would be complicated if circumsolar matter both reflected sun light and sent us its own; and still more so if we allow that the coronal light may be partly contributed from reflections and refractions in our own atmosphere. Then we have to consider whether the light thus contributed may possibly be due to the photosphere or to the prominences, and we are landed in a maze of difficulties which need not be discussed here.

The system of sketching introduced for this eclipse is at once so simple and final that the only wonder is it has not been introduced before. The corona must be either solar, atmospheric, or subjective, that is, more or less built up in the observer's eye, this more or less depending *cæteris paribus* upon the brilliancy of the undoubted solar portion. If at all stations, the stations being as wide apart as they have been this time, the drawings are similar, then the corona would be undoubtedly cosmical; if dissimilar, then it would either be terrestrial or subjective: and this point could and would have been settled this time, if the weather had permitted, by arranging the observers *in pairs*, that is, dealing with two observers in each place instead of a single one, and so obtaining the eye-variation.

This being premised, what is the result of the very few observations, comparatively speaking, which have been made? Before I attempt to give any idea of my answer to this question, it is only fair to myself to state that my only sources of information, up to the present time, have been conversations with some of the American members of the Sicilian expedition, a brief telegram from the members of the English party at Agosta, the Rev. S. J. Perry's communication to the *Daily News* of the 2nd instant, and an inspection of some drawings made by the officers of H.M. ships off Aci Reale. At Catania we saw a portion of the corona for 1½ seconds through a cloud, and that was all; and the day after the eclipse, before the more fortunate members of my party returned, it became my duty to proceed to Malta in H.M.S. *Lord Warden* to attend the court-martial on the officers and crew of the beautiful, but unfortunate, *Psyche*, in which we had been wrecked on the 15th ult.; and the weather in the Mediterranean has been so bad that it was impossible to leave Malta in time to rejoin the expedition before they left for England. Of detailed information, therefore, I have none.

In the first place, then, I submit that the fact that the corona is a compound phenomena comes out in an unmistakable way. We have first of all a ring some 5′ or 6′ high round the moon, which almost all observers have seen alike; and then we have light beyond which some observers have seen of one shape and some of another, now stellate with many rays, now stellate with few, now absolutely at rest, now revolving rapidly.

This I think to be the key-note of all the observations with which I have become acquainted. I need scarcely say that it is exactly what had been predicted.

First among the fortunate ones who observed the corona with the telescope was Prof. Watson, of Ann Arbor, who took up his station at Carlentini, and appears to have been the best favoured among the Sicilian observers. From his account I gather that there was an almost perfect *shell* around the sun about 5′ high, and that outside this shell were less definite rays. What he was particularly struck with was this, that, as seen in the telescope, the rayed portion was most developed over the prominences, and, as I gathered from him in one case, the rayed portion was absent as if a veil had been removed; so that he, at all events, is strongly impressed with the idea that the shell represented a true solar appendage, and that the rayed structure was due to our own atmosphere.

Next comes Mr. Brett, who, although he was not so fortunate, still was enabled to see and place on record some most interesting features, including the whole outline of the corona and even some of the protuberances. He also, as I am informed, saw the rayed portion of the corona most developed above the protuberances, the outline of the interior portion being visible, though not so strongly marked as in the case of Prof. Watson's drawing, in consequence of less favourable atmospheric conditions. I am thankful to say that the weather at Syracuse enabled Mr. Brothers to obtain some admirable photographs, which I have not yet seen. These are among the most important results of the Expedition.

Next I must mention Prof. Peirce, the head of one of the American parties, who observed two miles north of Catania, at a private casino of the Marchese Sangiuliano. I believe that he also saw the shell, but of this I am not absolutely certain; but he distinctly observed that the outer corona over the prominences was rosy red, although he did not see the prominences himself. A more beautiful proof of the terrestrial nature of this portion of the corona it would be difficult to imagine: for, of course, at the sun, the hydrogen, which thus tinged it, is incapable of colouring anything, as its own light is absorbed by the transcendent brilliancy of the photosphere; while nothing would be more natural than to suppose that the light, which, in its own atmosphere, should strongly tinge anything radially illuminated, should be that of the prominences.

But the strongest proof of the variability of the outer portion and of the constancy of the inner portion is afforded by the observations made on board the small fleet attempting to save the *Pysche* off Aci Reale, where the eclipse was observed in unclouded splendour. Here were the ironclads *Lord Warden*, *Caledonia*, and *Royal Oak*, and the tugs *Weasel* and *Hearty*, besides the Italian gunboat *Plebiscito*, all within a stone's throw of each other. In all the drawings, and many have been received, we have a ring 5′ or thereabouts, while the outer portion is as variable as may be. On the same deck, that namely of the flag-ship, *Lord Warden*, two drawings were made, one by Captain Brandreth, and the other by Dr. Macdonald, F.R.S., in which the variation is so strong that one would feel inclined to acquit the atmosphere of any participation in the matter, and to relegate the whole outer corona to subjectivity alone, did not Mr. Brothers's admirable photographs show both phenomena, as I am told they do. Dr. Macdonald saw eight rays arranged with perfect symmetry; Captain Brandreth saw only two elliptical hoops crossing each other at right angles.

Captain Cochran, of the *Caledonia*, besides the ring, saw a complicated stellate figure, the rays of nearly equal length, while Mr. Dexter, at sea between Catania and Syracuse, saw, besides the ring, *only one ray* of inordinate length.

So much for the drawings. I think that if the records of former eclipses be now examined, especially Mr. Carrington's drawing of the eclipse of 1851, and compared with the others taken at the same time, additional evidence will be gathered in favour of the compound nature of the corona, which, on the evidence now before me, I consider the great teaching of the present eclipse. Our experience in Sicily seems to be similar to that of the Spanish observers, for Mr. Perry writes that "some observed two curved rays," while the rapid degradation of light occurred at one-fifth of a solar diameter, but, so far as I know, no one in Sicily was favoured with a view of the dark intervals which were observed in Spain.

There is a strange and most interesting discordance between some of the spectroscopic observations made in Sicily and Spain. At Agosta, where the totality was well visible for ten seconds, Mr. Burton detected a green line near E, with a tangential slit (distance from moon not stated). This line, which was also seen by the Italian observers, is doubtless the one recorded last year by the American astronomers, but in Spain Mr. Perry states that bright lines at C near D, *b* (or E) and F were observed 8′ away from the sun. At Syracuse Prof. Harkness, whose telescope was moved into the various positions by Captain Tupman, R.M.A., found the green line in all parts of the corona, so far as about 10′ from the sun, and at one point thought he detected two green lines, less refrangible than it; but at several places he saw a complete hydrogen spectrum (including C), which he attributed to prominences, until he was informed by Captain Tupman that there was no prominence near the slit. More proofs of the terrestrial nature of this portion of the corona, I think, taken in connection with the fact that *the dark moon gave identically the same spectrum*. It would appear that there was so much atmospheric reflection in Spain, and here and there at Syracuse, that the true coronal spectrum with its line near E, the existence of which we must now accept as established beyond all question, was partially masked by the prominence spectrum with its usual well-known lines. There is one passage in Mr. Perry's interesting letter in which, if there be a misprint, as I suspect there is, an observation of great importance is recorded. It runs, "Mr. Abbay, observing at Xeres with a spectroscope of 2 prisms of 45° belonging to Professor Young, saw the bright lines C, D, F; and afterwards F and a line rather more bright than F on the less refrangible side of B, C not noticed then."

Now, if *b* (not B) was intended here we have sub-incandescent hydrogen mixed with the green-line-giving-substance, which may probably be a new element with a vapour density less than hydrogen.

So that roughly we might regard the chromosphere to be built up of the following layers, which are in the orders of vapour density in the case of known elements:—

X′ (new element)	. . . . . .	Green coronal line
Hydrogen	Sub-incandescent	F
	Incandescent	C, F, near G, h
X (new element)	. . . . . .	Near D
Magnesium	. . . . . .	*b* and lines in blue and violet
Sodium.	. . . .	D
Barium	. . . .	Several lines
Iron, &c.	. . . .	Several lines, including E

The foregoing table excludes naturally the substance or substances which give bright lines in the solar spectrum, which are at times visible in the spectrum of the chromosphere. I have ventured to suggest that the substance which gives the line in the green is a new element, because invariably I have found that in solar storms the chromospheric layers are thrown up in the order of vapour density, and because all the heavier vapours are at or below the level of the photosphere itself.

With regard to the question of polarisation, the parties in Sicily obtained evidence that the corona was radially polarised, though Professors Harkness and Eastman obtained a result which they explain differently. Mr. Ranyard, at Villamonda, and Mr. Peirce, jun., north of Catania, obtained identical results in favour of strong polarisation. Hence the solar corona, accepting these observations, not only radiates, but reflects solar light to us. A careful consideration of this fact, taken in connection with the possible addition of a, so to speak, terrestrial corona to its light, may enable us to account for some of the observations, both polariscopic and spectroscopic, which do not at first appear to harmonise with those to which I have referred, notably those which give a pure continuous spectrum to the corona, and which state that its light is only slightly polarised.

From what has preceded, then, we seem justified in suggesting as working hypotheses the following, which, however, more accurate information may alter, and which I offer as suggestions only, *bien entendu*.

1. The Solar Chromosphere extends some 5′ or 6′ from the sun (Watson and others), its last layers consisting of cool hydrogen (Mr. Abbay), and possibly a new element with a green line in its spectrum (Young, Burton, and others); which line, if it be identical with the auroral line as stated by Gould, may possibly be present in the higher regions of our own atmosphere.

2. Outside this stratum the rays, &c., are for the most part due partly to our own atmosphere, partly to our eyes, for their shape varies; they are seen by some at rest, by others in motion, and their spectrum is the same as that of the dark moon (Maclear).

3. The white light of the chromosphere above the prominences, as seen in an eclipse, is due to its strong reflection of solar light, as shown by the polariscopic observations (Ranyard, Peirce, jun., Ladd).

4. The rosy tinge of the corona proper, that is of the region more than 5′ or 6′ from the sun, is due to our atmosphere containing light which comes from both the higher and lower strata of the chromosphere (Peirce, sen., Maclear, Abbay).

Venice, Jan. 9 J. NORMAN LOCKYER

A HEARTH OF THE POLISHED STONE AGE

Note sur un Foyer de l'Age de la Pierre polie découvert au Camp de Chassey en Septembre, 1869. Par Ernest Perrault. 1870. 4to. Pp. 32, and 8 plates. (Chalon sur Saône, L. Landa. London: Williams and Norgate.)

ON the summit of a steep hill between the valley of the Bas Roches and that of the Dheune, overlooking the immense plain of the Saône and commanding a view of the Jura, the Alps, and the mountains of the Maconnais and the Morvan, and surrounded by numerous other camps, is the camp of Chassey, which occupies an area of about 800 yards in length by a breadth varying from about 100 to 200 yards. So commanding and important a spot was not only taken possession of by the Romans for a *castellum* and by the Gauls for an *oppidum*, but was also occupied in prehistoric times. Several collections of antiquities belonging to different periods have been formed upon the spot, but it was reserved for M. Perrault to make the interesting discovery which he has recorded in so simple yet so complete a manner in the pages now before us. A terrace, sheltered by rocks from the north and east winds and facing the morning sun, seemed to him well adapted for early habitations, while a depression in the ground in front proved, on examination, to contain the remains of a large hearth, or it might be termed kitchen, and here he instituted excavations.

Beneath a few inches of soil he found a bed rather more than two feet in thickness, made up of ashes, bones, and pottery, and containing numerous instruments of various kinds. The whole reposed on a platform of rough slabs of stone, blackened like the soil beneath them by the action of fire. Not a trace of metal was discovered, and in describing the objects found, M. Perrault divides them into (1) instruments of stone, (2) those of bone, and (3) pottery.

Exclusive of fragments some 150 stone instruments were found, consisting for the most part of hatchets, arrow-heads, flakes, borers, scrapers, hammers, mealing stones, and polishing stones. No less than eight perfect stone hatchets were found, as well as fourteen broken, and of those that were uninjured two were still mounted in stag's-horn sockets, similar to those with which the discoveries in the Swiss Lake dwellings have made us so well acquainted.

Only two are of flint, and one of fibrolite, the others being of chloromelanite, serpentine basalt, and diorite. They seem to have been formed from pebbles brought down by the Saône, and it is interesting to observe that the same process of manufacture was in use in this part of Burgundy as in Switzerland, the splitting of the pebbles into the required form having been partly effected by sawing. That some of the spare hours of those who frequented the hearth were employed in preparing their hatchets is proved by the large number of grinding or polishing stones, of which, counting fragments, upwards of sixty were present. M. Perrault regards one of the smallest of the cutting instruments, a little triangular celt, as a religious emblem, but it seems more probable that it was used as a hand-tool, like a chisel, of one of which the sharpened end was also found.

The arrow-heads of flint, twenty-three in number, present a variety of forms, leaf-shaped, triangular, lozenge-shaped, and tanged, the latter both with and without barbs. Their general aspect is such as might have been expected from the locality, most of the forms occurring also in Switzerland. There are, however, one or two shaped-like small hatchets, with a broad sharp base, formed by the original edge of the flake from which they were made, and rounded or truncated at the other end. It is stated that this sharp edge was intended for insertion in the wood, but more probably it was the other end that was thus secured, and the arrows were, so to speak, chisel-pointed, like the flint-tipped arrows which survived in use, probably for fowling purposes, after metals became known to the ancient Egyptians. Similar arrow-heads, if such they be, have been found in considerable numbers in Sweden, and a few in Denmark, as well as in some other parts of France. It seems by no means impossible that some of the sharp-based instruments from the Yorkshire Wolds may have served a similar purpose.

The mealing stones consist of a large block, usually of hard sandstone or porphyry, and a smaller stone as muller, and are of the same character as those still in use in Central Africa. They must have been gradually eaten together with the flour they produced, and no doubt tended to promote that wearing away of the crown of the teeth so common in ancient times. None of the grain has been found, but probably most of the cereals known to the old Swiss Lake dwellers were also known at Chassey.

The objects in bone and horn are almost identical with those from the earlier Swiss Lake dwellings, and consist of the sockets already mentioned, awls, chisels, &c. The pottery, which is extremely fragmentary, is much of the same character as the Swiss. It has been ornamented both by punctured dots and by a sort of pillar moulding as well as by incised lines. In one instance there seems to have been an attempt to represent the outline of a boar by lines scratched in the clay when still moist. In another, the ornament consists of bands of triangles alternately cross-hatched and plain, a style more in accordance with the bronze age than with that of stone. Most of the pottery seems to have been adapted for suspension. The number of small ears or handles found exceeded 200. A few spindle-whorls and beads were also found, but the most curious objects are the spoons, exactly similar in form to those of wood in common use in our kitchens at the present day, but formed of clay. It is true that several wooden ladles and at least one earthenware spoon were found in the settlement of Robenhausen, but one can hardly repress a feeling of surprise at finding the spoon so fully and completely developed among a people apparently unacquainted with the use of metal, though it is true that they appear to have had the materials for porridge at their command.

In concluding this short notice of a valuable contribution to prehistoric archæology, a regret must be expressed that the animal remains discovered in the refuse heap have not, apparently, as yet been submitted to proper scientific examination, so as to determine the species and which of them were domesticated, though some human remains from neighbouring tumuli and interments are reported on by Dr. Pruner-Bey. The animals whose bones occurred are described as ox (possibly domesticated), pig, stag, sheep, goat, and horse, which is rare. The bones are not always broken, and the vertebræ occasionally occurred in juxtaposition, as if meat at times had been extremely abundant. There is no mention of any remains or traces of dogs, and this condition of the bones seems to afford an argument in favour of their absence, which, if established, would be a remarkable fact. Some teeth of reindeer are mentioned as having been found on the plateau, and it would be of great interest to ascertain their relation to the other remains. Let us trust that ere long there may again be a season in France when a thought may fairly be bestowed on other camps and other earthworks than those on which attention is now so unfortunately concentrated.

J. E.

SPONTANEOUS GENERATION

I HAVE repeatedly subjected various solutions for Dr. Bastian to a temperature of 150° to 156° C. in sealed vacuous tubes, in order that he might afterwards submit them to a microscopical search for living organisms. The result of this search led him to conclude that living organisms had been generated from non-organised matter, whilst Professor Huxley, who examined the contents of one of the tubes, considered that no such conclusion could be drawn from his own observations. I therefore determined to repeat these experiments, operating in exactly the same manner as before in the preparation of the solutions, the sealing them up in vacuous tubes, and exposing them to a high temperature, but taking additional and much more stringent precautions against the subsequent admission of atmospheric germs into the tubes.

For this purpose four tubes of hard Bohemian glass were prepared, and about half-filled with a liquid consisting of

Carbonate of Ammonia . . .	15 grains.
Phosphate of Soda	5 grains.
Distilled Water	1 oz.

No care was taken to exclude living germs from these ingredients, reliance being placed, for the destruction of their vitality, upon the high temperature to which they were afterwards subjected.

These tubes were carefully exhausted by means of the Sprengel pump, and hermetically sealed; they were then, on July 18, 1870, exposed for four hours to a temperature varying from 155° to 160° C. in a Papin's digester. After being allowed to cool, the digester was opened, and the tubes immediately plunged, two of them into colourless concentrated oil of vitriol, and the remaining two into a nearly colourless saturated solution of carbolic acid in water. These precautions were taken in order to avoid the possible admission of atmospheric germs through invisible cracks in the glass; such cracks, entirely invisible to the eye, are known sometimes to exist, and to be in some cases so excessively minute as to require several days for the admission of enough air to perceptibly impair a torricellian vacuum within. By keeping the tubes entirely immersed in liquids which are immediately fatal to vitality, I hoped to meet any objections that might be raised, in the event of living organisms being subsequently found in the tubes, that the germs of such organisms had gained access to the enclosed liquids through invisible fissures in the glass. On examining them when they came out of the digester, it was evident that the interior walls of the glass tubes had been corroded by the enclosed liquid, and as the tubes had stood upright in the digester, it was easy to see, by the sharp limits of the erosion, the extent to which the liquid had expanded under the influence of the high temperature to which it had been exposed.

The cylinders containing the immersed tubes were now maintained at a temperature from 60° to 75° F., and were exposed to bright diffused daylight, and sometimes to sunlight, for a period of more than five months.

The liquid in all the tubes became more or less turbid, and in some cases a small quantity of a light flocculent precipitate subsided to the bottom. On the 24th of December last two of the tubes, which exhibited the greatest turbidity, were selected for examination (one of them had been immersed in concentrated sulphuric acid, the other in the solution of carbolic acid). The vacuum was unimpaired, and the liquid in the interior formed a good water hammer. These tubes were opened in the presence of Prof. Huxley and Mr. Busk, and we submitted their contents to a searching microscopical examination with powers varying from $\frac{1}{8}$th to $\frac{1}{16}$th. Especially was the flocculent sediment in the tubes subjected to careful inspection. So far as the optical appearances presented by the sediment go, they may be appropriately described in the terms which Dr. Bastian applied to the matter found by him in a solution of like composition and similarly treated (see NATURE, July 7, 1870, p. 200). "A number of little figure-of-eight particles, each of which was $\frac{1}{20000}$" in diameter, were seen in active movement, even in situations where they could not have been influenced by currents. The portions of the pellicle were made up of large, irregular, and highly refractive protein-looking particles imbedded in a transparent jelly-like material. The particles were most varied in size and shape, being often variously branched and knobbed. There were also seen several very delicate, perfectly hyaline vesicles, about $\frac{1}{20000}$" in diameter, these being altogether free from solid contents" But the movement of the particles which we observed was obviously mere Brownian motion; and many of the particles were evidently minute splinters of glass. There was not the slightest evidence of life in any of the particles. The water on the slide containing these solid matters was evaporated off, and they were treated with hot concentrated sulphuric acid, the temperature of the slide being raised to about 100° C. There was no blackening, and the rounded and dendritic bodies remained as entirely unaltered as the glass splinters. Indeed, some of the larger spheroidal bodies were evidently rounded particles of glass which had become detached from the inner walls of the tube by the corrosive action of the enclosed liquid at the high temperature to which it had been exposed in the digester.

London, January 16 E. FRANKLAND

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Continuity of the Chalk

SIR CHARLES LYELL devotes a paragraph of his valuable "Students' Elements of Geology," just published, to the consideration of what he regards as a "popular error as to the geological continuity of the Cretaceous period." I feel the utmost diffidence in venturing to controvert any opinion of an authority so unrivalled in such questions, but as I believe the first definite suggestion of this view occurs in the report of the *Lightning* Expedition of 1868 by my friend and colleague, Dr. Carpenter, where it is specially associated with my name, I feel bound to defend so far as I can, or at all events to explain, an opinion which I then held on perhaps somewhat slender grounds, but which further investigation and reflection have since ripened into a firm belief. Sir Charles Lyell says (p. 263) that "certain points of resemblance which the deep-sea investigations have placed in a strong light, have been supposed by some naturalists to warrant a conclusion expressed in these words: 'We are still living in the Cretaceous epoch;' a doctrine which has led to much popular delusion as to the bearing of the new facts on geological reasoning and classification." I do not say that the phrase "we are still living in the Cretaceous epoch" is defensible in a strictly scientific sense, chiefly because the terms "geological epoch" and "geological period" are thoroughly indefinite. We speak indifferently of the "Silurian period" and of the "Glacial period," without consideration of their totally unequal value, and of the "Tertiary period," and of the "Miocene period," although the one includes the other. It is intended rather, I believe, in a popular sense, to meet what seems to be the general popular impression, that a geological period has, in the region where it has been studied and defined, something in the shape of a beginning and an end, that it is bounded by periods of change,—elevation, denudation, or some other evidences of the lapse of unrecorded time; and that it would be inadmissible to speak of two portions of the same continuous deposit, however distant the times of their deposition might be, and however distinct their imbedded faunæ, as belonging to two geological periods.

It was certainly under this idea that in an address to a popular audience in April 1869, I stated my belief that it is not only chalk which is being formed at present in the bed of the Atlantic, "but *the* chalk, the chalk of the Cretaceous period." Sir Charles Lyell says, in summing up his objections at the end of the paragraph, "the reader will at once perceive that the present Atlantic, Pacific, and Indian Oceans are geographical terms, which must be wholly without meaning when applied to the Eocene, and still more to the Cretaceous period; so that to talk of the chalk having been uninterruptedly forming in the Atlantic from the Cretaceous period to our own, is as inadmissible in a geographical as in a geological sense." I confess I do not understand the geographical difficulty; the "Atlantic Ocean" is doubtless a geographical term, but the depression under discussion occupies the area at present expressed by that term, and to use it seems to be the simplest way of indicating its position. That it is highly probable that the chalk has been so uninterruptedly forming over some parts of that area, is, however, exactly what I wish to show. I will therefore set aside the question of expression, and address myself simply to the consideration of the fact. And first with reference to the physical aspects of the case.

All the principal axes of elevation in the north of Europe and in North America have a date long anterior to the deposition of the Tertiary and even of the newer Secondary beds; and these strata were, consequently, all deposited with a certain relation in position to certain main features of contour, which are maintained to the present day. Many oscillations have, undoubtedly, taken place since, and every spot on the European plateau has probably many times alternated between sea and land, but it is difficult to show that these oscillations have occurred in the latitude of Britain to a greater extent than 1,500 feet up and down; a subsidence to that extent would, however, be sufficient to produce over most of the northern land a sea 100 fathoms deep, the average depth of the German Ocean.

From a glance at a geological map of Europe and North America, it would seem that the sum of these elevations and subsidences has produced a gradual elevation of the edges, and a general contraction, of a basin the long axis of which coincides roughly with the axis of the Atlantic. The Jurassic beds crop out along the outer edge of this basin, the Cretaceous beds form a middle band, while the Tertiaries occupy the troughs and valleys. All of these, however, maintain a certain parallelism, determined by the contour of the older mountain ridges, to one another and to the shores of the present sea.

From the parallel of 55° N. lat., at all events to the equator, we have on either side of the Atlantic a depression 600 to 700 miles in width, averaging 15,000 feet in depth.

These two valleys are separated by the modern volcanic plateau of the Azores. I cannot think it at all probable that any general oscillations have taken place in the northern hemisphere since the commencement of the Tertiary period sufficient to form that immense abyss, or, if formed, to convert it into dry land; but on this point I am able to quote the highest authority:—"If at any former period the climate of the globe was much warmer or colder than it is now, it would have a tendency to retain that higher or lower temperature for a succession of geological epochs. . . . The slowness of climatal change here alluded to, would arise from the great depth of the sea as compared to the height of the land, and the consequent lapse of time required to alter the position of continents and great oceanic basins. . . . The mean height of the land is only 1,000 feet, the depth of the sea 15,000 feet. The effect, therefore, of vertical movements equally 1,000 feet in both directions, upward and downward, is to cause a vast transposition of land and sea in those areas which are now continental, and adjoining to which there is much sea not exceeding 1,000 feet in depth. But movements of equal amount would have no tendency to produce a sensible alteration in the Atlantic or Pacific Oceans, or to cause the oceanic and continental areas to change places. Depressions of 1,000 feet would submerge large areas of the existing land, but fifteen times as much movement would be required to convert such land into an ocean of average depth, or to cause an ocean three miles deep to replace any one of the existing continents." (Lyell, "Principles of Geology," 1867, pp. 265-6.) The wide extent of Tertiaries in Europe and the north of Africa sufficiently proves that much land has been gained in Tertiary and post-Tertiary times, and the great mountain masses of Southern Europe give evidence of great local disturbance. But although the Alps and the Pyrenees are of sufficient magnitude to make a deep impression upon the senses of men, taking them together, their materials would, if spread out, only cover the surface of the North Atlantic to the depth of about six feet, and it would take at least 2,500 times as much to fill up its bed. It would seem by no means improbable that while the edges of what we may call the great Atlantic depression have been gradually raised, the central portions may have acquired an equivalent slight increase in depth; but it is most unlikely that while the main features of the contour of the northern hemisphere remained the same, an area of so vast extent should have been depressed by more than the height of Mont Blanc. On these physical grounds alone I should be inclined to believe that a considerable portion of this area has been continuously under water, and that consequently a deposit has been forming uninterruptedly from the period of the chalk to our own.

I would now refer to the palæontological bearings of the question. Sir Charles Lyell says (p. 263), "The reader should be reminded that in geology we have been in the habit of founding our great chronological divisions, not on foraminifera and sponges, nor even on echinoderms and corals, but on the remains of the most highly organised beings available to us, such as mollusca. . . . In dealing with the mollusca, it is those of the highest or most specialised organisation which afford us the best characters in proportion as their vertical range is the most limited. Thus the cephalopoda are the most valuable, as having a more restricted range in time than the gasteropoda, and these again are more characteristic of the particular stratigraphical sub-divisions than are the lamellibranchiate bivalves, while these last again are more serviceable in classification than the brachiopoda, a still lower class of shell-fish, which are the most enduring of all." With great deference to Sir Charles Lyell, I cannot regard the most highly specialised animal groups as those most fitted to gauge the limits of great chronological divisions, though I admit their infinite value in determining the minor sub-divisions.

The culmination of such animal groups, such as we find in the marvellous abundance and variety of both orders of cephalopods at the end of the Jurassic and the commencement of the Cretaceous period, undoubtedly brings into high relief, and admirably illustrates to the student, the broad distinctive characters of the Mesozoic fauna; but speaking very generally, the more highly a mollusc is specialised, the shallower is the water which it inhabits. The cephalopods are chiefly pelagic and surface things, and their remains are consequently found in deposits from all depths. The gasteropods, with comparatively few exceptions, range from the shore to 1 to 200 fathoms, and lamellibranchs become scarce at a slightly greater depth; while some orders of crustacea, brachiopods, echinoderms, sponges and foraminifera, descend in scarcely diminished numbers to a depth of 10,000 feet. In fact, the bathymetrical range of the various groups in modern seas corresponds remarkably with their vertical range in ancient strata.

A change in the distribution of sea and land involving a mere change in the course of an ocean current, might modify the conditions of an area for all cephalopods, pteropods, heteropods, and other surface living animals of high type, even to their extinction. By oscillations of 500 feet up and down, the great mass of gasteropods and all reef-building corals, would be forced to emigrate, become modified, or destroyed, and another hundred fathoms would exterminate the greater number of bivalves; while elevations and depressions to ten times that amount might only slightly affect the region of brachiopods, echinoderms, and sponges.

In the late deep-sea dredgings by M. de Pourtales, off the American coast, and by H.M.S. *Lightning* and *Porcupine*, and

Dr. Marshall Hall's yacht *Norna* off the west coast of Europe, no animal forms have been discovered, so far as I am aware, identical with chalk fossils. Additional evidence has, however, been procured that over a large part of the bottom of the Atlantic a deposit is being formed mainly of disintegrated globigerinæ and other foraminifera and coccoliths, which appears to be undistinguishable from the ancient chalk.

Not fewer than twenty genera of vitreous sponges have been dredged belonging to two groups, the Hexactinellidæ and the Lithistidæ of Oscar Schmidt, both of which groups are highly characteristic of the chalk and greensand. These sponges are in vast numbers, like the ventriculites and their allies in older Cretaceous beds; and they, with other silicious organisms equally abundant in the modern chalk area, seem to be capable of supplying that amount of silica in a fine state of division which might explain the production of chalk flints. A large series of echinoderms were found, recalling to a remarkable degree, from the profusion of Cidarids and of star-fishes of such genera as Archaster, Astrogonium, and Stellaster, the general facies of the chalk echinoderm fauna; and besides this general resemblence, members of several families have been recovered which were supposed to be extinct. *Salenocidaris varispina* A. Ag., dredged by De Pourtales in the Strait of Florida, is a living Salenia; *Echinolampas caratomoides* A. Ag. perpetuates some of the most marked characters of the Galeritidæ. *Pourtalesia*, a genus first found by Count Pourtales and afterwards in the *Porcupine* expedition, is a true Dysaster. *Porocidaris purpuratus*, a fine species dredged off the Butt of the Lews, represents a genus hitherto known only by some isolated plates and radioles. Two very remarkable generic forms, dredged from the *Porcupine* off the coasts of Scotland and Portugal, only known from some fragments in the English white chalk, found a new family near the Diademidæ. Some new ophiurids approach their fossil ancestors; and off the coast of Portugal the dredge brought up at one rich haul twenty or thirty examples of a fine *Pentacrinus*; while over the whole area *Rhizocrinus loffotensis*, a degenerate little Bourgueticrinus, seemingly one of the last of the pear-encrinites, is abundant.

I am not in a position to say much about those groups which I have not personally examined, except that Mr. Gwyn Jeffreys and Prof. Martin Duncan report that among the mollusca and corals many species occur which have been hitherto known only as fossils, principally, as might have been expected, in comparatively shallow water forms in the Tertiaries.

I do not see that there is any object in attempting to explain this singular resemblance between these deep-sea deposits in the Atlantic and the old chalk in composition and structure and in embedded fauna on any other assumption than that of a continuity of conditions over some part at all events of the area. During the lapse of time, while the fauna of shallower water has again and again undergone almost total change by changes in the distribution of temperature and in the distribution of sea and land, the fauna of the deep water has been also affected. To a depth of 5,000 feet it is at present heated over a large portion of the North Atlantic many degrees above its normal temperature. Accepting, as I believe we are now bound to do, some form of the doctrine of the gradual alteration of species through natural causes, one is quite prepared to expect a total absence of the identical forms found in the old chalk. The utmost which might be anticipated is such a resemblance between the two faunæ as might justify the opinion that the later fauna bears to the earlier the relation of descent with extreme modification. Sir Charles Lyell asks if we have dredged belemnites, ammonites, baculites, hamites, turrilites, &c.; that question is, I think, best answered by the record of the old Cretaceous beds themselves, which are scarcely more remarkable for the presence of these singular and beautiful forms than for their rapid extinction. According to the view which I have felt myself compelled to adopt, the various groups of fossils characterising the Tertiary beds of Europe and North America represent the constantly altering fauna of the shallower portions of an ocean whose depths are still occupied by a deposit which has been accumulating continuously from the period of the pre-Tertiary chalk, and which perpetuates with much modification the pre-Tertiary chalk fauna. I do not see how this view militates in the least against the "reasoning and classification" of that geology which we have learned from Sir Charles Lyell. Our dredgings only show that these abysses of the ocean which Sir Charles Lyell admits in the passage quoted above to have outlasted on account of their depth a succession of geological epochs, are inhabited by a special deep-sea fauna possibly as persistent in its general features as are the abysses themselves.

WYVILLE THOMSON

Ocean Currents

ATTENTION has been much drawn of late to the subject of Ocean Currents and their causes, and it has occurred to me that there is a directing if not an originating cause of these streams, which has, so far as I am aware, been overlooked by physicists. It is known* that at some parts of the earth's surface there exists an atmospheric pressure capable of sustaining a column of mercury in the barometer of upwards of 30 inches in height; at the same time there are certain areas over which this pressure is only such as to raise the barometric column to a little over 29 inches. Now if we compare the difference of absolute weight sustained by two such areas, we shall see that in the space over which the higher atmospheric pressure exists, there is an excess of weight of air, amounting in round numbers to 1,000,000 of tons on each square mile. Applying this fact to the region of the ocean in which the surface currents are best known, the North Atlantic, we find from the isobaric chart that there is throughout the year over a large portion of the eastern side of this sea, next the coast of North Africa, a pressure (to use the convenient mode of expressing it) of upwards of 30·2 inches. To westward of this space, towards the Gulf of Mexico and the coast of the United States, the average pressure decreases; between Newfoundland and the British Isles the pressure is still diminished, till in the wide channel between Iceland, Norway, and Spitzbergen, we arrive at a yearly pressure of less than 29·6 inches. It is reasonable to believe that the waters which lie under the high pressure area have a tendency to escape from under the excessive weight, towards the space over which the pressure is less. But the high pressure area next the African coast is precisely that upon which the north-east trade winds descend, and the waters, aided in their choice of an exit, will naturally flow off to south-westward before the wind. Their continuance in this direction is barred, however; for across the whole of the southward passage between Africa and South America, there exists another belt of high pressure, out of which the south-easterly trade winds blow. The only course left for the escaping waters (allowing for the moment that the excess of pressure *is* a cause of their movement) is to westward, where the pressure is lessened, towards the Gulf of Mexico, and the east coast of America, and thence towards the low-pressure space between Iceland and Norway. But this is exactly the course that the Gulf Stream, or rather the North Atlantic warm stream of which the Gulf Stream is the most prominent feature, is seen to take. Are we not then warranted in concluding that the difference of atmospheric pressure has some power both in originating and in directing the course of this ocean current?

In suggesting the unequal distribution of atmospheric pressure as a supplementary cause to difference of temperature and of density, to evaporation, rain, and winds, and to whatever further agents there may be in the production of ocean currents, I would venture to express a hope that some one in authority, by carefully comparing and valuing the power of each one of these motive forces, and their application to the known streams, will give to the world a system of the causes of ocean currents which will be vastly more relevant to the phenomena these streams are known to present, than any one of the theories which have as yet been put forth.

When we know that Sir John Herschel gives to the winds the entire right of setting the ocean streams in motion; that Captain Maury holds the universal circulation of the sea to be caused by nothing else than the differences in its specific gravity, and that Dr. Carpenter (or rather Professor Buff) would bring about a general interchange of polar and equatorial water by the aid of sunshine and frost alone; is it not time to ask which of these three causes we should accept as the true one, or if all three are partially concerned, what part is to be taken from each to let the others have their fair share in the work?

KEITH JOHNSTON, Jun.

The Measurement of Mass

I AM happy to meet with an opponent who comes so directly to the point as my Reviewer, W. M. W.

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* I would refer those who desire to look more particularly into this matter to the monthly isobaric charts prepared by Mr. Buchan, to illustrate his admirable paper on the mean pressure of the atmosphere. Trans. Royal Soc. Edin., vol. xxv.

The gist of his argument lies in his assertion, that "if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound."

Now, since the determination of a pound is actually effected by making it a copy of a standard so that they shall counterpoise each other *in vacuo*, it is strictly independent of locality.

The standards actually employed for this purpose, for example, the authorised copies of the principal standard pound which have been sent to various countries, have been made as nearly identical in mass as skilled workmen could make them; and have been sent at random to different latitudes.

Similar remarks apply to the weights in a chemist's box. Surely it is not seriously proposed that the chemist should file them down to accommodate them to the increase of gravity, when he takes them from London to Edinburgh.

Where should we find ourselves, if the makers of chemists' weights endeavoured to make them of different masses, according to the places where they were to be used?

Fortunately, this has never been attempted; and since all parts of the world are in possession of practically identical standards of mass, under the name of standard pound or standard kilogramme, and tolerably accurate copies of these and their fractional parts are in everybody's hands, why not acknowledge them as standards of mass, which they are in point of fact, although, according to the theory which I am combating, they ought not to be?

Those who hold that theory must choose between two evils:—they must either make the pound a unit of force, in which case they must file or load their weights as they go from place to place (this seems to be the alternative which W M. W. chooses); or they must accept the gravitating forces of equal pieces of metal as nominal units of force at the different localities where these pieces of metal may happen to be, although these forces are really not equal. This latter alternative, which gives a variable unit of force, has been commonly adopted till recently, and a variable unit of mass has been conjured up to suit it.

If our spring-balances were as accurate as our standard weights, W. M. W.'s idea would be practicable. The equality of two forces at different places could then be very directly tested; but, in fact, the most accurate means we possess of making such a comparison consists in a double process, a weighing with the ordinary balance, combined with a difficult and less accurate pendulum-comparison. Inasmuch then as ease and accuracy of comparison is the first essential of a scientific standard, I submit that the world is right in employing standard pounds of equal mass and not standard pounds of equal gravitating force.

Much confusion arises from using the word *weight* in a connection which leaves it doubtful whether *mass* or *gravitating force* is meant. I trust you will keep your columns open for the further discussion of this question, as it much needs ventilation.

Belfast, January 11 J. D. EVERETT

The Tails of Comets, the Solar Corona, and the Aurora considered as Electric Phenomena

IN reference to a letter from Mr. Bedford, Ph.D., published in your last number, allow me to state—

1. That I had never seen or heard of Dr. Bedford's theories.
2. That, judging from the extract given in his letter, Dr. Bedford has not published anything analogous to the electrical hypothesis which I have put forward. In his letter he has misquoted and made omissions to the extent of one-half the heading, in order, I suppose, to avoid the very mention of the word electricity, which, on the other hand, is the very substance of my postulate. OSBORNE REYNOLDS

Owens College, Jan. 17

Apparent Size of the Moon

FROM almost any place in the balcony of St. James's Hall, at an evening performance, one may find in the coronets of gas-jets, forming one or more of the arches across the roof, corroboration of what I wrote in NATURE of May 12, 1870 (vol. ii. p. 27). The nearest or first coronet and the two next, observe a gradation of increase in apparent magnitude, as they should do. But the rest, which (unlike those three) descend towards the horizon, and should, nevertheless, observe a gradation of increase, are apparently all of a size. I call attention to this "unconsidered trifle" as having the full force of a very elaborate experiment. C. M. INGLEBY

Ilford, Jan. 7.

Atmospheric Currents

THE following is part of a letter signed "G." in NATURE of 6th October, 1870:—

"It is very important to obtain correct and copious data regarding the atmospheric currents between say 5,000 feet and five miles above the level of the sea, and especially at various points on and near the Equator, and at about 30° to 32° North and South latitudes. Within these limits the rain-bearing currents of the atmosphere move. If self-registering meteorological instruments were placed permanently upon several of the leading mountain ranges of the world, and their records copied at stated intervals, we should obtain valuable data for determining the direction, velocity, and magnitude of the controlling atmospheric currents of the globe."

This suggestion is most valuable, but it would involve a source of error that would be difficult to allow for. Most mountain ranges occupy so extensive an area that they have their own local climates, and indications on such ranges would consequently not give accurate information about the currents where they are not modified by such influences. The most valuable information will be obtained from the most isolated mountains. The most isolated mountains of sufficient height, within sufficiently easy reach of us, are Etna and Teneriffe. Etna is in the variables, and Teneriffe in the trade-winds.

I do not, however, agree with the following suggestion, that more valuable observations still might be obtained by means of captive balloons. Balloons cannot be used in stormy weather, and they are also subject to the great disadvantage, that they tell nothing about barometric fluctuations: because the height of a balloon above the earth can be known only from the barometer, and consequently there is no way of ascertaining the indications of the barometer at a known height. It is not at all certain that the barometric curves at the summit and at the base of Etna or Teneriffe would present any close correspondence. Sets of barometrical and other meteorological observations taken for sufficiently long periods at the summit and at the base of such mountains would be probably the most valuable of all data for meteorological science in its present state.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Jan. 14

THE AMERICAN ECLIPSE EXPEDITION

THAT portion of the American Eclipse Expedition which was stationed at Xeres was favoured by weather which enabled the observers to examine the Corona during the whole period of totality, and with some results of interest; an informal account of a part of which the kindness of Professor Winlock enables me to lay before your readers.

The party was in charge of Professor J. Winlock, and stationed in an olive grove about a mile from the city, the property of Mr. Davies, of Xeres, to whom all the members of the expedition are indebted for continual aid and kindness. The observations for the determination of longitude, latitude, and times of contact were conducted by Assistant G. W. Dean, of the Coast Survey, aided by Captain Ernst, of the U.S. Engineers, and by Mr. Gannett, of the Harvard College Observatory.

Their labours were facilitated by the courtesy of the Director of the neighbouring Observatory of San Fernando, with which telegraph connection was established, and the results attained will be found in full in their official report.

Besides the instruments of precision, the party was provided with four equatorial telescopes of from six to eight inches aperture, driven by clockwork, and many smaller ones. One of the largest telescopes carried an objective specially corrected for actinic rays, with which, and a companion instrument, photographs were taken during the totality and previous stages by Mr. Willard and his assistant.

In addition, a photographic telescope of upwards of thirty feet focus, placed horizontally, and receiving the solar rays from a heliostat, was used, in charge of Mr. Gannett.

The accessories of the instruments were such as the previous experience of the observers (nearly all of whom had witnessed the eclipse of August 1869) suggested, and were too numerous for description here, though a means of rapidly recording the position of the lines noted in the Spectroscope, devised by Professor Winlock, proved of too essential service to be left unnoticed. Each spectroscope was provided before leaving America with this attachment, which enabled the observer to record the position of the lines as fast as he could point on them, and with a precision that compared favourably with micrometrical measurement.

I pass to the work done by each observer.

The Spectroscope used by Prof. Young was the one recently described in your pages; and through this he was enabled to watch the occultation of the protuberances of the photosphere, and announce the approach of the moon, some fifteen seconds before "first contact." At this time the sky about the sun was nearly clear, and numerous successive photographs were taken, with the accompanying chronographic record of the instant of exposure. Heavy clouds then obscured the sun, at intervals, till totality was very near, when a happy fortune gave us a few minutes of almost clear sky while it lasted.

It is important to explain that, though the sky was so little obscured that the Corona was visible during the whole of totality, there was during most of this time a very slight haze. I recall a short view of sky, distinctly blue in spite of the darkness; and the structure of the protuberances was continuously visible through the telescope; nevertheless, the presence of this partial haze should be remembered, as having a possible bearing on some of the conclusions to be drawn from what follows.

I give as nearly as I briefly can the principal results of each observer, as regards the corona only.

Professor J. Winlock, using a spectroscope of two prisms on a five-and-a-half-inch achromatic (directed by Mr. A. Clark as the finder) found a faint continuous spectrum, without dark lines.

Of the bright lines, the most conspicuous was 1474 Kirchhoff, which was followed all around the sun to at least 20′ from the disc. It may be here remarked that all the spectroscopes showed this as much the most conspicuous coronal line. A number of other lines were also noted, and their position recorded by the apparatus just referred to.

Professor Young's observations of this line were similar; he estimates its least extension at one half the solar diameter. With the slit of his spectroscope placed tangentially at the moment of obscuration, and for one or two seconds later, the field of the instrument was *filled with bright lines.* As far as could be judged, during this brief interval every non-atmospheric line of the visible spectrum showed bright; an interesting observation, confirmed by Mr. Pye, a young gentleman whose voluntary aid proved of much service. The observation of Mr. Pye was made with a spectroscope of one prism, before which was placed a small telescope arranged at Prof. Young's suggestion, not to give, as usual, a definite image on the slit, but to supply light from all portions of the corona and neighbouring sky with more intensity than would be furnished by directing the instrument toward the sun without it. From the concurrence of these quite independent observations, we seem to be justified in assuming the probable existence of an envelope surrounding the Photosphere, and beneath the Chromosphere usually so called, whose thickness must be limited to two or three seconds of arc, and which gives a discontinuous spectrum consisting of all, or nearly all, the ordinary lines, showing them, that is to say, *bright* on a dark field.

Mr. Abbay, of Wadham College, Oxford, assisted with a Spectroscope prepared by Prof. Young for collateral observations to his own, and obtained results which have already been made public.

The Polariscope was used by Prof. Pickering at a station half a mile distant from the rest of the party. Using successively an Arago Polariscope, one of the form employed by Prazmowski, and a Savart, he is understood to have obtained with all three, results pointing to a radial polarisation of the Corona. The light covering the moon's disc he observed to be polarised throughout in the same plane, and the observations showed that the Arago and other Polariscopes dependent on colour were sufficiently delicate to determine this plane with accuracy.

At the same time Mr. Ross, his assistant, using the instrument employed by Prof. Pickering in the last eclipse, obtained the same results as were then found. Mr. Ross used a modification of the Bunsen photometer, and obtained several concordant measurements, showing that the light was equal to that of a standard candle at two feet.

The writer used a Savart's polariscope attached to a small telescope of 1½ inches aperture, and having a field of about 2°.

The observations with the Savart's polariscope being subject to ready misconception, the preparation for observation and the appearance during it are here given with some minuteness.

Before the eclipse the Savart was so adjusted that the bands were most distinct when vertical, viewing the meridian sun reflected from water. None were visible when the sun was directly scrutinised before or after totality. During totality the appearance which presented itself was unexpected.

The bands were distinctly seen on the corona, and were brightest where normal to the limb and where tangential to it. As the polariscope was slowly rotated, no marked diminution of their brightness was seen, and when it had been turned through 45° they were as bright as before; distinctly visible even in colour, and they so remained, the rotation being continued for greater security through a whole revolution, during the whole time they presented the appearance described, and characteristic of radial polarisation. They were not noticed on the disc of the moon, but this may well have been from the observer's attention being so exclusively directed to verifying their persistency on the corona.

The writer also employed a good achromatic, of four inches aperture, with a power of about 150, in the direct study of the coronal structure with negative results.

On the closest scrutiny of the part nearest the sun, nothing was seen but a nearly uniform diffuse light, except that one "dark ray" in the field was noticed to be absolutely straight and nearly radial. The outline of the Corona was roughly quadrangular, and a heavy field bar provided for the purpose being carefully set during totality in the direction of the longer diagonal, was found on subsequent estimation to make an angle of as nearly as possible 45° with the vertical.

The "red flames" were beautiful objects, as incidentally noticed during the telescopic scrutiny of the Corona, but not, I may add, more distinctly or more in detail than I had viewed them the day before through the Spectroscope of Professor Young.

The coronal structure is well shown on a photograph obtained by Mr. Willard during totality; a very interesting drawing of it was also taken by Mr. Gordon, a gentleman resident in the vicinity; and all the observers have descriptions to give of its appearance to the naked eye, which differ in some degree from each other. If I compare my impressions with those of others, or even with my own of last year, I find difference enough to suggest the probability of considerable "personality" in all such statements. In some well-marked features all agree, in other minor ones such difference exists that one might almost say each saw a different Corona. The observations with the Spectroscope and Polariscope are happily more removed from uncertainty.

The conclusions reached with both are not perhaps as absolute as they would have been with the cloudless sky of last year's Eclipse, they are still such as to fully justify, we may hope, the cost of labour and time in obtaining them, and which none of those present can regret.

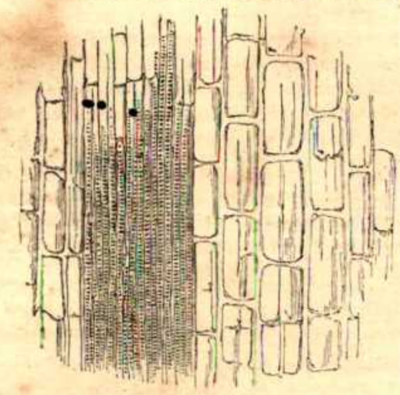
S. P. LANGLEY

SUGAR

II.

THE Beet-root (*Beta vulgaris*) is a native of the south of Europe, and is said to have been introduced into England in 1656. It is a hardy biennial plant with reddish purple leaves and large fleshy roots, which in some varieties are long or spindle-shaped like a carrot, in others short and thick, almost like a turnip. The colour also varies, some forms being of a deep purple, while others are of a dirty white with a purple tinge. Internally the beet-root is of a blood-red colour. It is well known with us both as a pickle and as a salad plant, but it is much more extensively used even for these purposes in Germany and France than it is in England.

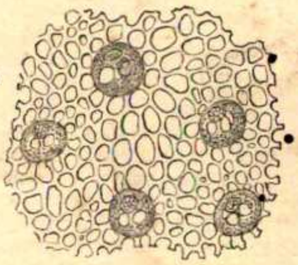
Numerous varieties of the beet are in cultivation, but one known on the Continent by the name of Betterave à Sucre is extensively grown as a sugar-producing plant; and the trade in beet-root sugar is a very important one throughout France, Belgium, Germany, and Russia. The bulk of the sugar consumed in these countries is furnished by this plant. Its cultivation for the produce of sugar has been more than once attempted in this country, but hitherto on too small a scale to be successful. Latterly, however, more attention has been paid to it, and more spirit and energy shown by those who have taken up the question towards overcoming obstacles that were at one time considered insurmountable; but whether the results will continue to prove remunerative remains to be seen. So long ago as 1837 a refinery solely for the manufacture of beet-root sugar was established at Chelsea; and many acres of land in the neighbourhood of London were devoted to the cultivation of beet. The discovery of the presence of sugar in beet was made in 1747 by a Prussian chemist named Margraaf, but his discovery was at first considered rather in the light of a scientific than of a practically useful character. It formed the subject of a communication to one of the learned societies in Berlin; and for several years afterwards the sugar so produced was considered an article of curiosity, and was consequently sold at fancy prices. Forty or fifty years elapsed before any experiments were made for the purpose of putting the discovery into practice, with the idea of extracting the sugar for actual use; these experiments however did not succeed, owing to the imperfect manner in which they were carried out; and the beet sugar manufacture would possibly even then have resulted in utter failure, had not Napoleon I., by excluding British Colonial produce from France, rendered it necessary for some new method to be devised for supplying France with sugar. Prizes were offered and many plans submitted. The Government, however, gave its support to the beet as the most likely source of success. Experiments were again renewed, and the result proved satisfactory, so that by 1812 this branch of manufacture was, for a time, placed on a firm footing in France. In 1814, however, the French markets again being opened, large quantities of cane sugar at once appeared from the West Indies, and beet sugar again fell into the background. A system of heavy and increasing duties continued to be levied upon colonial sugar till the year 1822, when the duty became so high as actually to amount to a prohibition to its entrance into French ports, and again the beet-root factories began to flourish. At the present time, or rather before the War broke out, hundreds of millions of pounds were made and consumed. Germany and France produce the largest quantity, Russia following close behind. It is, however, feared that the 300,000 tons of beet-root sugar, which was the estimated produce of France for 1870, will have been for the most part lost, owing to the interruption of the harvesting of the roots, and the consequent stoppage of the operations of the sugar factories.



LONGITUDINAL SECTION OF SUGAR-CANE, MAGNIFIED

Like the sugar-cane, the beet varies in quality and in the quantity of saccharine juice, according to the climate, soil, and mode of culture.

After the roots are dug up they are cleaned, usually by scraping them with a knife; they are then either sliced or rasped, and reduced to a pulp, which is placed in canvas bags and submitted to high pressure, by which means the juice is expressed. The pulp undergoes a second and sometimes a third squeezing, so as to obtain all the saccharine matter. This juice or liquor is then heated in a copper, and filtered and boiled with lime and water. A scum rises to the surface, which is taken off, and the juice is again boiled till it becomes of a proper consistency, when it is crystallised in a similar manner to cane sugar. Beet-root juice as expressed is very clammy to the touch, is nearly colourless, has a strong disagreeable smell, and contains a larger quantity of nitrogenous matter than cane juice. It is capable of being clarified and refined, so that it is made almost, if not quite, equal in appearance to the most superior descriptions of lump or moist sugar prepared from the sugar-cane.



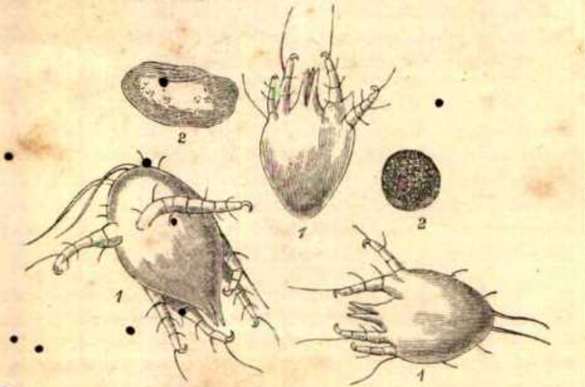
TRANSVERSE SECTION OF SUGAR-CANE, MAGNIFIED

"Beet-root sugar is not only identical with cane sugar, but much of the Dutch lump sugar is actually the produce of beet-root. The circumstance cannot be too much insisted upon, that the seeming distinction between yellow beet sugar and yellow cane sugar depends on the extraneous coloured matters present. These, when eliminated by refining, leave white materials in all respects identical. There is positively no difference between these two, whether of colour or of grain. Grain or crystals can be developed from either to the size of the largest candy if desired; in fact, large white crystals produced from beet-root are sent in quantities from France into this country to compete with London, Bristol, Scotch, and other crystal manufactories."

As beet sugar has become of late in more general use in this country, so has cane sugar found its way in large quantities on the Continent. The mixing of these sugars can scarcely be considered in the light of adulteration, except when an inferior kind of one is mixed with a superior quality of another. The perfect system of filtering adopted in the manufacture of beet-root sugar causes it to be much freer from extraneous matters than cane sugar, small pieces of the cane itself frequently occur in some descriptions of the latter. Of course it is impossible for them to be contained in loaf sugar, sugar-candy, or the finer kinds of sugar which undergo a careful process of purification.

A great deal has been said about the adulteration of sugar with sand, powdered marble, bone-dust, &c., and these tales are readily believed in by many people; but it is very unlikely that such insoluble substances would be used, as they would be sure to be detected. We know that inorganic substances are present in sugar, but we are inclined to believe that they are traceable to the imperfect cleaning of the cane or to the accidents, if we may so call them, attendant upon the process of manufacture.

Amongst the organic impurities often to be found in sugar, besides fragments of the cane before alluded to, are glucose or grape sugar, vegetable albumen, starch, minute fungi, and very frequently a number of small insects known as the sugar beetle (*Acarus sacchari*). The first of these, grape sugar, we have before described as being distinct from cane sugar, though its composition is nearly similar; as it is found most abundantly in grapes, whence its name; but it is also contained in various other fruits, as well in the dried state as in the fresh; and, moreover, it can be produced by the action of dilute sulphuric or other mineral acids, on starch or woody fibre, and so, in short, can be manufactured, which cane sugar cannot. It does not so readily crystallise as cane sugar, and the crystals are irregular, or have no definite form. It is much more clammy in an ordinary way, and dissolves or runs to juice at the least possible moisture of the atmosphere to which it is exposed. It is of less sweetening power than cane sugar, and is much more liable to ferment, therefore its presence with cane sugar tends to lessen the value of the latter.



MICROSCOPICAL APPEARANCE OF THE SUGAR INSECT (*Acarus sacchari*).—1. PERFECT INSECT.—2. OVA

The purest form of cane sugar to be obtained is that known as "lump," and for this reason it is always used in preserving fruit, as the chances of fermentation are thus diminished. Sugar-candy and all the large crystal or dry sugars are pretty sure to be free from glucose, because its presence would, according to the proportions, cause the sugar to be more or less clammy or moist. The presence of vegetable albumen and starch in cane-sugar is one step towards its conversion into grape sugar or glucose, and consequently increases its inclination to ferment. The removal of this vegetable albumen is one of the objects, as well as one of the difficulties, of perfect sugar refining.

The presence of minute fungi in sugar can only be detected by the aid of a microscope, but the sugar-beetle may be seen with an ordinary lens.

We know of no better way of detecting it along with other impurities than one which has been before recommended, namely, to place a teaspoonful or two of sugar in a glass of tepid water, let it stand till the sugar has completely dissolved, when the insects will be found, some perhaps at the top of the water and others at the bottom amongst the sediment. It is supposed that this insect is the cause of the irritation of the skin, more especially of the hands, from which grocers and those who have the handling of sugar are said to suffer.

Many other plants besides the sugar-cane and beet are used in their respective countries as sugar-producers. Thus, while we and our European neighbours receive our supplies from the two plants just alluded to, the chief sugar-producer in North America is a species of maple (*Acer saccharinum*), a large forest-tree; and in the East Indies several species of palms yield sugar.

With regard to the effects of sugar upon the system, it is abundantly proved that it is most valuable. Nature has so provided that it shall be diffused not only in the sap of young and growing plants, but also in milk, the food of the young of all Mammalia. Though sugar may be considered as a force producer, it is much more adapted to the young than to the adult, for it is not capable by itself of producing muscle.

JOHN R. JACKSON

NOTES

IN addition to the details given this week of the Eclipse Expedition by Mr. Lockyer and Mr. Langley, we have received from Prof. W. G. Adams a report of the Agosta section of the Expedition, publication of which we are compelled to defer till next week.

IT is with great pleasure we hear of the appointment of the Rev. F. W. Farrar to the Headmastership of Marlborough College. This institution has already set an example to our other public schools in the cultivation of Natural Science, which will, we trust, receive a fresh impulse under the new headmastership.

WE rejoice to hear that there is at last a chance of a large amount of scientific knowledge being required at the hands of naval and military officers. This is as it should be, and we earnestly hope that the long-needed reform will not stop here.

THE Austrian astronomers, MM. Weiss and Oppolzer, who went to Africa to observe the Eclipse, were, we regret to learn, as unfortunate as the other observers on that continent.

AT a meeting of the Master and Senior Fellows of Caius College, held December 14, 1870, the following College order was passed:—"That until as many as forty shares of the College income are applied to the promotion of the Natural Sciences and Experimental Physics, a sum equal to the deficit (estimated at 20*l.* a share) be taken from the balance for distribution, and paid to a fund called the 'Natural Science Fund.' That the shares applied to any Fellowship or Scholarship awarded for proficiency in the Natural Sciences and Experimental Physics be counted as so applied. That the Natural Science Fund be applied solely to the promotion of the study of Natural Sciences and Experimental Physics in any way whatever which, in the judgment of the College, may seem proper."

THE Birmingham and Midland Institute has done equal honour to Professor Huxley and to itself, by electing him to the office of President, in succession to the late Charles Dickens.

DR. WOOD has been appointed Lecturer on Chemistry at the St. Mary's Hospital Medical School, in succession to Dr. Russell, transferred to St. Bartholomew's.

WE understand that Dr. Michael Foster and Mr. Watts are engaged in translating Kühne's "Physiological Chemistry."

WE hope in succeeding numbers to give in full Dr. Carpenter's and Mr. Gwyn Jeffreys's Report on Deep-sea Researches carried on during the months of July, August, and September 1870, in the *Porcupine*, recently read before the Royal Society.

WE quote the following pleasant "international" from the *New York Technologist*:—In view of recent remarks concerning the action of the British Government in regard to the expedition proposed by British scientific men, for the purpose of observing the approaching eclipse, it is but justice to say that on proper representation being made to Mr. Lowe, the Chancellor of the Exchequer at once expressed his opinion that such an expedition was one eminently worthy of government aid, and perfect arrangements as regards ships, funds, &c., have been made. One good result has, however, attended this delay on the part of the British Government—it has afforded an opportunity for the exchange of very pleasant courtesies between the scientific men of the United States and those of Great Britain. As soon as Prof. Peirce heard that their government had refused the use of the necessary ships, he at once tendered to the English scientists all the facilities at his command; and they were ample. No one doubts that, under similar but opposite circumstances, the same offer would have come from the other side; for, however stubbornly *Alabama* controversies may hold out, science takes no cognizance of them."

WE are informed that the series of penny lectures on Natural Science, delivered at the Hulme Town Hall, are to be obtained in London of Mr. Pitman, Paternoster Row.

A DEPUTATION from University College, London, consisting of Mr. George Grote, the president, the Hon. George Denman, M.P., Mr. Julian Goldsmid, M.P., and Dr. Storrar, members of the Council; Prof. Fuller, C.E., Prof. Williamson, F.R.S., and Mr. J. Robson, the secretary, has waited upon the Duke of Argyll, at the India Office, to present a memorial from the Council and the Senate of the College, on the subject of the proposed institution of a new Engineering College for the Indian Service. The interview with his Grace lasted upwards of an hour. The memorialists consider that the deficiency in the present system has arisen, not from any defect in the existing places of education, but from the injudicious system of examinations hitherto pursued, and from a want of sufficient inducements to well-qualified men to enter on the career proposed to them. The memorialists assume that the latter cause is recognised by the Government, for it is understood that it is in contemplation to augment considerably the salary upon which a civil engineer in the service of the Indian Government will hereafter commence his work. The memorialists believe the proposed college will be prejudicial to the public service by narrowing the field for the selection of candidates, and by limiting their means of obtaining the requisite instruction. They submit that the working of Government colleges has not been such as to recommend the creation of a new Government college, having practically the monopoly of appointments, and protected from competition. They suggest that such a step is at variance with the plan for throwing open to all Her Majesty's subjects the opportunity of gaining Government appointments.

AT a meeting of the Royal Geographical Society, held on the 9th inst., Dr. Carpenter read a paper on Oceanic Circulation, especially in reference to the inset current into the Mediterranean through the Straits of Gibraltar. After detailing the theories which have hitherto been propounded to account for this current, all of which he pronounced to be unsatisfactory, Dr. Carpenter propounded the following explanation:—The water of the Mediterranean has a uniform but limited excess of weight over that of the Atlantic—so limited as to do away altogether with the idea that there is an accumulation of salt in the Mediterranean. The excess of salt was found to be greater in the lower than in the upper stratum of water, and thus the excess of evaporation was produced. If there be two columns of water of equal density—one that of the Atlantic, the other that of the Mediterranean—an excess of evaporation lowers the height of the Mediterranean column. If the Atlantic column were of fresh water, just enough would flow in from it to restore the evaporated water. But it is salt water which actually flows in from the Atlantic, and therefore it produces an increase of pressure, which presses the Mediterranean water outward till the equilibrium is restored. This being restored, there is once more a reduction of the Mediterranean level by evaporation, and so on; and, therefore, a circulation is always going on between the waters of the Mediterranean and Atlantic. The only difficulty in receiving this explanation is that the water of the outward-flowing current must run uphill; but other examples, he stated, are known. The fact that such a circulation exists, he said, is now indisputable, recent experiments having satisfactorily determined the evidence of a westerly current underlying the surface easterly current. These currents are, therefore, only a portion of the general oceanic circulation, which causes a perpetual surface motion of warm water from the equator to the pole, and a counter under-current of cold water from the pole to the equator. In replying to objections raised in the course of the discussion, Dr. Carpenter said that he had only broken ground in the enunciation of his theory, which it would require much labour yet to work out. Sir Henry Rawlinson, who occupied the chair, stated that in consequence of the continued illness of Sir R. Murchison, he had been appointed president of the Society *ad interim*.

THE Wind-Chart, to which we referred last week, furnished daily by the Meteorological Office to the *Shipping and Mercantile Gazette*, is the invention of Captain Charles Chapman.

FROM the annual report, it appears that the gardens of the Dublin Zoological Society have been very successful, the number of visitors being 136,000, and a considerable balance lies in favour of the society. The donations of axolotls from Prof. Wyville Thomson, and of Mammoth Cave fishes from Prof. Mapother, are acknowledged, but it is not stated if they live or have undergone any developmental changes.

THE following regulations have been issued by the Science and Art Department with regard to science teachers who wish to attend the special (six weeks') courses for training in teaching at South Kensington in June, July, and August, in accordance with the circular of September 1870. These applications are so numerous that it is impossible for the Department to make the selection of teachers to attend, as was originally proposed. The Department has therefore decided to make the selection by competition at the next May examinations. The competition for those who wish to attend the instruction in Biology will be in Animal Physiology, Zoology, Vegetable Anatomy and Physiology, and Systematic and Economic Botany. The candidate may take up all the four subjects, but a fair proficiency in Animal Physiology and in Vegetable Anatomy and Physiology will be essential. If this proficiency is shown, the marks obtained in the other subjects will be counted. By fair proficiency is understood the amount of knowledge required to pass in the advanced stage. For those who wish to attend in Experimental Physics, the competition will be in Acoustics, Light and Heat, and in Magnetism and Electricity. A candidate will be required to take up the honours' papers in those subjects in which he teaches, or in which he is qualified to earn payment, but in any of the other subjects of the group in which he is competing he may take the advanced or the elementary paper. The marks obtained in these subjects will count as in the competition for Royal Exhibitions. The papers of the teachers who are competing will be specially and separately looked over by the professional examiners—the examination being competitive only—and the teachers who answer best, probably to the number of about forty-five in each group, will be allowed to come to London for the six weeks' course of training. They will receive their travelling expenses, namely, second-class railway fare, and 30*s.* a week while in London. The results of the teachers' examination will not be published. Each candidate will be informed of the position in which he would have been placed had he been examined as an ordinary candidate for honours. If he wish his success recorded, it will be done in the ordinary way. The Biology course will commence on the 14th June, and the course on Experimental Physics on the 5th July. It must be understood that no teachers, except such as come up under the foregoing rules, will have travelling allowances under § lxviii. of the Science Directory. No persons are eligible to receive the allowances granted in this minute except those who have been engaged in science teaching under the Science and Art Department during the session 1870-71.

It appears from Mr. Glaisher's Meteorological Tables, that while December 25 was the severest day of the recent frost in the neighbourhood of London, it was still more intense in other localities on January 1, falling as low as 4·9° at Wolverhampton, 5·0° at Hull, 6·7° at Bradford, 7·0° at Leeds, and 9·7° at Leicester, the minimum at Blackheath on that day being as high as 19·2°, although at Twickenham, as reported by Mr. Hall last week, the thermometer fell as low as 6·3°. As on Christmas Day the severest cold was again in the Midland and North Midland counties, and we have once more to record the phenomenon to which we have had occasion to allude more than once recently, that the frost was considerably more intense in England than in Scotland. For the week ending January 7, no lower temperature was recorded at any of the Scotch stations than 21·0°, at Dundee; the mean temperature for all the English stations was 29·6°, while for all the Scotch stations it was 35·5°, or more than 6° higher. From the 20th of December to the 13th of January, when the frost finally broke up, the temperature in London was only even slightly above the mean of the last fifty years on three days.

The Rev. Mr. Gribble, F.R.S., reports from Constantinople that after a fine morning the disc of the sun in the late eclipse was obscured for some time, so that it was not well seen until $3^h 26^m$ mean time, when there was only about one-eighth of the disc clear. A large solar spot then became uncovered; clouds coming on again, this was all that was seen clearly. At $3^h 26^m$ the light was considerable.

The *Scottish Naturalist*, to which we referred some weeks since as a new periodical proposed to be issued under the auspices of the Perthshire Society of Natural Science, has made a good beginning. Opening with a short programme of future operations, in which the editor, Dr. Buchanan White, wisely states his intention of restricting the contributions to papers and notes especially bearing on Scottish Natural History; we then have an article by Dr. Lauder Lindsay "On Natural Science Chairs in our Universities," to which we may possibly take occasion again to refer. The writer deals with a bold hand with the evils which are said to prevail more especially in the election of Professors in our northern Universities. He refers to the manner in which these appointments are stated often to be regulated by political influence or religious belief rather than by possession of qualifications to teach Natural Science, and is especially severe on the system of personal canvass and the "testimonial nuisance." Mr. J. A. Harker contributes a paper "On the Work and Influence of Local Natural History Societies;" Mr. George Norman a List of the Noctuæ occurring in Morayshire; the editor some hints on Sugaring, how, when, and where to do it; and an anonymous contributor, a paper "On the Mollusca of the North-east of Scotland," illustrated with a map. Some shorter articles and notes with reports of proceedings of Scottish Natural History Societies, make up the number, which we commend to the notice of all interested in natural history across the Border.

HENDERSON'S PATENT PROCESS FOR REFINING CAST-IRON

An article was furnished a short time ago, and published in Nature (No. 57, p. 94), respecting a new process for the production of steel by the partial decarburisation of cast-iron, invented by the writer. It was then stated that a description of a new process for refining cast-iron would soon follow.

This process supplies a more effective and economical process than the English finery or the German reverberatory furnace processes for refining cast-iron. It is very simple, and does not require any fuel, labour, or expensive apparatus; and there is no loss of weight of iron, as the impurities go off in vapour. The cost of refining is less than one-twentieth of that of the English and German processes; and its effect is more thorough than is possible by those systems.

The agents used are fluorine and oxygen combined. The fluorine is derived from any fluoride, and the oxygen from any substance containing or capable of evolving oxygen, which is adapted to use in the manufacture of wrought-iron and steel. Fluorspar and pure rich iron ores are the most available and economical substances for producing these agents, and are applied finely powdered and mixed, and placed in receptacles, preferably, so as to act from the under side upwards upon cast-iron in its molten state.

The most economical mode of application of this process is to treat the cast-iron in the condition in which it flows from the blast furnaces, with fluorspar and iron ore, applied in the "chills" or pig moulds used at blast furnaces, by being spread over the bottom of the moulds.

The iron, when tapped from the blast furnace, flows into the mould thus prepared; the heat of the iron causes fluorine and oxygen to be liberated, and, by reason of the affinities of these substances for silicon and phosphorus, these impurities are removed in the form of *vapour*. The reactions in the "chills" are similar to those of the boiling puddling process, and last about five minutes. The metal during this period is covered with jets of flame and smoke. The resulting metal, with respect to silicon and phosphorus, is as pure as wrought-iron.

It is preferable to use iron ores containing the largest amount of oxygen and the least of silica and phosphorus. These conditions exist in "washed iron sands," and the red hematites of Cumberland and Lancashire. When using the hematite ores, varieties that are the easiest to reduce to powder are preferred; and the ordinary edge running apparatus, with cast-iron rollers revolving in a pan, is an excellent one for the purpose.

The fluorspar and fine ore are passed through a sieve of not less than four hundred meshes to the square inch, and afterwards mixed so thoroughly as to appear to be one substance, in the proportion of one part of fluorspar to two parts by weight of iron ore, and are spread one-fourth to three-eighths of an inch deep over the "chills," then the iron is run upon them so as to form slabs one inch thick.

The former article gave analyses of refined cast-iron produced in the reverberatory furnace. It will be seen by comparing the analysis of the refined metal of this process with the analysis of that of the reverberatory furnace process, that they are analogous, except that the latter contains no silicates or graphite. The refined metal of the pig-mould process saves the fuel and time taken for refining by the reverberatory furnace process, and shortens the time of producing steel or wrought-iron by that process fully forty minutes. When using the refined metal of this process two workmen can take charge of five ordinary boiling puddling furnaces making steel, and six furnaces making wrought-iron, as the only labour necessary is "balling" and removing it from the furnace. When high carburised steel is made, "balling" is not required, as the metal will be fluid enough to run from the furnace into ingot-moulds. The economy of the process consists in the saving of the labour of eight workmen making steel, and two workmen making wrought-iron; and in the superior quality of the result as compared with puddling the refined metal. The time occupied by the conversion of the refined metal into steel or wrought-iron by the process without puddling is about the same as that usually taken in puddling pig-iron.

Ordinary coke pig-iron, smelted near Pittsburgh from a mixture of hematite ore and mill cinder, has been treated by this process. The refined metal was afterwards puddled and rolled into "muck" bars, and once heated and rolled into merchant bar-iron. The pig, refined cast, and wrought-iron, have been analysed by an analytical chemist in this city. These analyses are annexed, also analyses of refined cast-iron by the English finery and German reverberatory furnace process, and of the highest standard qualities of English, French, Swedish, and Russian wrought-iron, are given for comparison, taken from Percy's "Iron Metallurgy" and Ure's Dictionary:

	Pittsburgh Coke Pig-iron.	Patent Refined Cast-iron.	Bar-iron
Carbon (combined)	0·2040	0·3613	not deter.
do (graphite)	2·7685	2·5066	—
Silicon	2·3096	none	none
Slags (silicates)	0·3623	0·2983	not deter.
Phosphorus	0·4196	0·1029	0·0087
Sulphur	0·1298	0·1269	0·0438

Analyses of Foreign Iron.

	English Finery Process.	German Reverberatory Furnace Process.	
	Refined Cast-iron.	Pig-iron.	Refined Cast iron.
Carbon	3·07	—	—
Silicon	0·63	4·66	0·62
Phosphorus	0·73	0·56	0·50
Sulphur	0·10	0·04	0·03
Silica Alumina } Silicates	0·44		

	English Wrought-iron. Low-Moor Stamp.	French. Petin, Gaudet, & Co.	Swedish. Hoop L.	Russian. CCND
Carbon	0·016	0·143	0·087	0·272
Silicon	0·122	—	0·115	0·062
Sulphur	0·104	0·058	0·220	0·234
Phosphorus	0·106	0·030	0·034	—

On comparing the analysis of the refined cast-iron of the patent process with those of the English and German processes, it will be seen that while the refined iron of the new process contains *no silicon*, that of the English and German processes contains 0·63 and 0·62 per cent. respectively; and compared as regards phosphorus, the German process reduces it from 0·56 to 0·50 per cent., or about 0·06 per cent., and the new process reduces it 0·32 per cent., or over five times as much phosphorus is removed by the new process as by the other processes. The slags or silicates are 0·15 per cent. less than in the refined iron of the English finery process. The analysis of the patent refined cast-iron, compared with that of the above English wrought-iron, shows that whilst the wrought-iron contains 0·122 per cent. of silicon, the refined metal contains none, and compared as regards phosphorus they are about the same.

The analysis of the puddled wrought-iron, made from the refined cast-iron by once heating and rolling the puddled or "muck" bar, shows a purer quality of iron than the most celebrated makers of Europe produced from the purest ores with charcoal.

The economy of using the refined metal in saving cost of labour, fuel, &c., for the puddling process, has been fully demonstrated by numerous trials. When all the advantages of the process are realised, about one-half of the cost of converting cast-iron into wrought-iron can be saved; and there is an improvement in the quality equal to the difference between ordinary forge pig-iron and charcoal iron.

These advantages are:—

1. Better quality, which is due to the purity of the refined metal; as good qualities of wrought-iron are produced from it as from pig-iron made from the best ores smelted with charcoal.

2. The refined metal being as pure as wrought-iron, with respect to silicon and phosphorus, requires merely decarbonising, with less skill to work it, and greater certainty of the quality of the product.

3. Large saving in cost of production, owing to the shortening of the time of puddling, which is caused by the removal of a large part of the impurities by the refining process. White refined iron is decarbonised in twelve to fifteen minutes, and a "heat" or charge of five hundred pounds is puddled in fifty-five minutes, including time of charging, melting the iron, and stirring or puddling or "balling" and removing it from the furnace; grey forge iron requires sixty-five minutes, and foundry iron about seventy minutes. Seven "heats" or charges to a "turn" or a day's labour, are of easier accomplishment than five charges are from the pig-iron from which it was produced; the five charges now require ten hours to convert pig-iron into wrought-iron. It is possible to obtain, with the patent refined metal, by employing three sets of workmen in twenty-four hours, instead of two sets as is now customary, twenty-one charges in twenty-four hours instead of ten charges; and allowing sufficient time for repairs, the production of any ironwork may be doubled, without additional investment of capital and without additional cost of repairs.

4. Saving of fuel per ton of iron produced, amounting to one-half, caused by increased production.

5. Reduction of general business expenses per ton of iron, amounting to one-half, caused by increased production.

6. Reduction of wages, by reason of the diminished labour, of 40 per cent. per ton of iron.

7. The puddling furnace cinders of the refined metal contain but about one-fourth of the phosphorus, as compared with the cinders resulting from the use of pig-iron; and when smelted produce better qualities of pig-iron.

The cost of refining in the pig-moulds is very little. Fluorspar is a cheap material, and but about seventy pounds are required to refine a ton of iron. The cost is nearly compensated by the saving of the fuel and lime which would be required to reduce the puddle cinders of the refined cast-iron to pig-iron, as they contain but small portions of silica, and will require less fuel and limestone. The residue of the fluorspar and oxide is agglutinated, and remains in the pig moulds, and is pure lime and deoxidised iron ore, and is available as so much lime and ore in the blast furnace.

JAMES HENDERSON

SOME EXPERIMENTS ON COLOUR

THE theory of colour perception, although in England it has not yet made its way into the text-books, still less into the popular works on science, is fully established with regard to many important points. It is known that our perception of colour is threefold, that is, that any colour may be regarded as made up of definite quantities of three primary colours, the exact nature of which is, however, still uncertain. More strictly stated, the fundamental fact in the doctrine of colour is that, between any four colours whatever given, as well in quantity as in quality, there exists what mathematicians call a linear relation, that is, that either a mixture of two of them (in proper proportions) can be found identical, so far as the eye is able to judge, with a mixture of the other two, or else that one of them can be matched by a mixture of the other three. There are various optical contrivances by which the mixture spoken of may be effected. In the year 1857, Mr. Maxwell published an account of some experiments with the colour top undertaken to test the theory. From six coloured papers, black, white, red, green, yellow, and blue, discs of two sizes were prepared, which were then slit along a radius so as to admit of being slipped one over the other. Any five out of the six being taken, a match or colour equation between them is possible. For instance, if yellow be excluded, the other five must be arranged so that a mixture of red, green, and blue is matched with a mixture of black and white. The large discs of the three colours are taken and slipped on to each other, and similarly the small discs of black and white. When the small discs are placed over the others, and the whole made to rotate rapidly on any kind of spinning machine, the colours are blended, those of the large discs and those of the small, each into a uniform tint.

By adjustment of the discs an arrangement may be found after repeated trials, such that the colour of the inner circle is exactly the same both in tint and luminosity with that of the outer rim. The quantities of each colour exposed may then be read off on a graduated circle, and the result recorded. For instance (the circle being divided into 192 parts), eighty-two parts red mixed with fifty-six green and fifty-four blue, match thirty-seven parts white mixed with 155 black. In this way Maxwell observed the colour equations between each set of five, in all six sets formed by leaving out in turn each of the six colours. Moreover, for greater accuracy each set was observed six times, and the mean taken. But according to the theory these six final equations are not all independent of each other, but if any two of them are supposed known, the others can be found by a simple calculation. Accordingly, the comparison of the calculated and observed equations furnishes a test of the theory; but in practice, in order to ensure greater accuracy, instead of founding the calculations on two of the actually observed equations chosen arbitrarily, it is preferable to combine all the observations into two equations, which may then be made the basis of calculation. In this way, a system of equations is found necessarily consistent with itself, and agreeing as nearly as possible with the actually observed equations. A comparison of the two sets gives evidence as to the truth of the theory according to which the calculations are made, or if this be considered beyond doubt, tests the accuracy of the observations. In Maxwell's experiments the average difference between the calculated and observed systems amounted to ·77 divisions of which the circle carried 100. So good an agreement is regarded by him as a confirmation of the whole theory; but it seems to me, I confess, that only a very limited part of it is concerned. The axioms, in virtue of which it is permitted to combine the colour equations in the manner required for the calculations, are only such as the following:—If colours which match are mixed with colours which match, the results will match. It is difficult to imagine any theory of colour which will not include them. What proves the threefold character of colour—the most important part of the doctrine—is simply the fact that with any five-coloured papers *whatever* a match can be made, while, with less than five it cannot (except in certain particular cases). In regard to this point the value of the quantitative experiments is rather that they show of what sort of accuracy the eye is capable in this kind of observation. Those to whom the subject is new may think at first that if colour be threefold a match ought to be possible between any *four* colours. And so it is possible if there is no other limitation; but in experiments with the revolving discs, we are subject to a limitation, being obliged to fill up the whole circumference somehow. The difficulty will clear itself up, when it is remembered that one of the five colours may be black, so that with any *four* colours and *black* a match can be made with revolving discs.

It was rather for my own satisfaction than with the hope of adding anything new to a subject already so fully and ably treated by Maxwell, that I commenced a repetition of his experiments. The colours used were, roughly speaking, the same as his, as was also the general plan of the observations. The agreement of the calculated and directly observed equations was very good, the average error being only ·24 divisions of which the complete circle contained ninety-six, or one-third of the corresponding average error in Mr. Maxwell's Table. A second set of observations and calculations made after a year's interval with a different set of colours gave about the same result. I am inclined to attribute the considerably greater accuracy of my observations rather to an excellent perception of minute differences of colour (to which I have always found my eyes very sensitive) than to greater care in conducting the experiments. One precaution, however, I have found so important as to be worth mentioning. Unless the small discs are very accurately cut and centred, a coloured rim appears on rotation between the two uniform tints to be compared and adjusted to identity, which is exceedingly distracting to the eye, and interferes much with the accuracy of the comparison. One set of observations made with the same care, and apparently as satisfactory as any of the others, puzzled me for some time on account of the great discrepancies with the others which it exhibited. I have no doubt that the cause lay in the different character of the light on the day in question, which came from the unusually blue sky which sometimes accompanies a high wind. On the other days the light came principally from clouds. I have had no opportunity of confirming this opinion by a repetition of the experiment with a sky of the same degree of blueness, but that the disagreement was not the result of unusually large errors of observation, is, I think, to be inferred from the fact that the observations under the blue sky were as consistent among themselves as any of the other sets. As the point is of some interest, I give the figures in full.

July 23, blue sky.

Black.	White.	Red.	Green.	Yellow.	Blue.	
0	+ 30	+ 122	+ 40	— 77	— 115	obs.
0	+ 32·2	+ 120·8	+ 39·1	— 78·8	— 113·2	calcd.
+ 94	0	— 132	— 60	+ 55	+ 43	obs.
+ 91·6	0	— 133·5	— 58·5	+ 54·4	+ 45·9	
— 138	— 54	0	+ 24	+ 50	+ 118	obs.
— 138·3	— 53·7	0	+ 23·1	+ 49·5	+ 119·5	calcd.
+ 92	+ 50	+ 50	0	— 66	— 126	obs.
+ 94·1	+ 49·5	+ 48·5	0	— 65·2	— 126·7	calcd.
— 154	— 38	+ 86	+ 52	0	+ 54	obs.
— 154·6	— 37·5	+ 84·6	+ 53·1	0	+ 54·3	calcd.
+ 139	+ 18	— 128	— 64	+ 35	0	obs.
+ 138·5	+ 19·7	— 127·5	— 64·5	+ 33·9	0	calcd.

The numbers read off for the big discs are written with the sign + prefixed, and those corresponding to the little discs with —. Thus the first line may be read:—30 parts white together with 122 red and 40 green, match 77 yellow and 115 blue. The upper line of each pair represents the actual observation, and the second is the theoretical equation calculated from two in the manner described. The average difference between the two sets of numbers which may be taken as a measure of the inaccuracy of the observations amounts to 1·1. A similar table, formed from the observations of July 20 (cloudy), and which agreed very well with the results of other days, is as follows*:—

Black.	White.	Red.	Green	Yellow.	Blue
0	+ 30	+ 117	+ 45	— 79	— 113
0	31·1	116·2	44·8	79·9	112·2
+ 90	0	— 128	— 64	+ 56	+ 46
85·9	0	128·4	63·5	57·0	49·0
— 136	— 56	0	+ 22	+ 52	+ 118
137	55	0	22·3	50	119·6
+ 100	+ 50	+ 42	0	— 64	— 128
99·2	51	41·9	0	65	127·1
+ 135	+ 21	— 123	— 69	+ 36	0
135·7	21·5	122·7	69·3	34·8	0
— 152	— 40	+ 80	+ 56	0	+ 56
152·6	39·5	81	56	0	55

The average error is here ·95, showing only a trifling better agreement than the former set, so that the blue sky observations are nearly as self-consistent as those made with cloud-light. Moreover, the agreement is itself very good, being decidedly better than Maxwell's, though his calculations refer to a *mean* of six sets of observations.

While therefore there is no reason to distrust the results of July 23 any more than of July 20, the differences between them are much greater than can be ascribed to errors of observation. It will be found that they relate principally to the quantities of red, the numbers under that head being considerably greater for the case of the blue light from the sky. I am not aware whether the difference of sky and cloud light has ever been made the subject of direct investigation, but it would seem a fair inference that it must consist mainly in a relative deficiency of the red rays. If this be so, as I have other grounds for suspecting, the light of the sky would be similar in composition to that of dilute solutions of copper, which acquire their light blue tint by a partial suppression of the extreme red.[*] There is no doubt that the colour equations are dependent on the character of the light, as may easily be proved by taking an observation looking all the time through a layer of coloured liquid. It is not, however, the most brilliantly coloured solutions that cause the most disturbance, for anything like a complete stoppage of all the rays which are capable of exciting one of the primary colour sensations would affect both the mixtures to be compared in nearly the same manner, putting the observer in fact very much into the positions of a colour-blind person. Those liquids will be most efficient which have a different action on parts of the spectrum allied in colour. For instance, an aqueous infusion of litmus has a strongly marked action on the yellow ray, stopping it with great energy, even in rather dilute solutions. It is easy to trace the effect of looking through this on most of the colour equations. Consider, for example, the fifth equation of July 20 (that from which the blue is absent) wherein red and green are matched against black, white, and yellow. The red and green will for the most part escape absorption, but the white and yellow will be shorn of a part of their yellow rays. The match supposed to have been adjusted without the litmus must evidently be spoiled; the red and green mixture becoming strongly yellow in comparison with the other. In order to restore equivalence the yellow must be considerably increased. On trial I found, 124 black + 19 white + 49 yellow matched 121 red + 71 green.

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* These calculations were made by means of Prof. Everett's Proportion table, which seems admirably adapted to work of this sort.

It is only the impurity of the colours on the discs that prevents the effect being still more strongly marked, for with the pure colours of the spectrum the most violent alterations are possible. When a match is made between the simple yellow and that compounded of pure red and green, almost any coloured liquid acts unequally on the two parts and destroys the balance. The simple yellow, of course, retains its colour under any absorbing influence, and can only be changed in luminosity. Chloride of copper extinguishes the red component of the compound yellow, which accordingly becomes green. Litmus would leave the compound colour nearly unchanged, while it extinguishes the simple yellow. It is needless to multiply instances.

Before leaving the compound yellow, of whose very existence many are incredulous, I will mention an easy way of obtaining it, which is the more desirable as the use of the pure spectral colours is not very convenient. In order to isolate the red and green rays of the spectrum by means of absorption, the first thing is to find a liquid capable of removing the intermediate yellow and orange. With this object we may fall back on the alkaline solution of litmus, whose opacity to the yellow, and particularly to the orange rays is so marked. The next step is to remove the blue and bluish green, for which nothing is more convenient than the chromate of potash. A mixture of these two liquids in proper proportions, easily found by trial, isolates the green and extreme red rays with considerable perfection, and exhibits in a high degree the phenomenon of Dichromatism. According to the thickness traversed by the light the red or the green predominates, and there is no difficulty with a given thickness in arranging the strength of the solution so as to give a full compound yellow. It is worth notice in confirmation of the opinion expressed as to the character of the sky-blue, that when a cloud seen through the liquid appears a full yellow, or even orange, the former, if at all intense, acquires a decided green colour. A window backed by well-lighted clouds, when looked at across a room through the liquid and a prism, has a very splendid appearance, the red being isolated on one side, and the green on the other; while the intermediate space, where the two overlap, shows the compound yellow in great perfection. Another liquid, in some respects preferable, which answers the same purpose, may be made by mixing chloride of chromium and bichromate of potash. Through either of them the sodium flame is invisible, though they may easily be made to correspond with it in colour very closely. I tried to obtain a liquid capable of isolating the pure yellow ray, but only with partial success. The best was a mixture of bichromate and permanganate of potash with a salt of copper (sulphate or chloride). The first removes the blue and violet, the second the green, and the third the red, and thus the yellow is isolated in considerable purity. This liquid is very unstable. The comparison of the simple and compound yellow (which nearly matched) was interesting. One was transparent to the sodium flame, the other completely opaque to it. When the two are brought together so that the light has to traverse both, almost complete darkness results, even when the brightest clouds are used. I should mention that it is only when the light is strong that any of these liquids give yellow in full perfection; otherwise the colour is more nearly described as brown, which is, in fact, identical with a dark yellow or orange. The best natural yellows, such as chrome, are partly simple and partly compound, returning all the light which falls upon them except the blue and violet. It is clear that neither a purely simple nor a purely compound yellow can rival them in brilliancy.

Impartial observers, unprejudiced by the results of mixing pigments, or, on the other hand, by experiments on the spectrum, see, so far as I can make out, no connection between the four principal colours—red, yellow, green; and blue. It seems to them quite as absurd that yellow should be compounded of red and green, as it most unquestionably is, as that green should be a compound of blue and yellow, though many have accepted the latter alternative on the authority of painters, and some have even worked themselves into the belief that it is only necessary to look at the colours in order to recognise the compound nature of green. My own prejudice would be on the other side, the result of experiments on the compound yellow, which is seen so easily to pass into green on the one side or red on the other. The most impartial opinion that I can form is that there is no real *resemblance* between any of the four, and if this be so it is certainly a most remarkable, if not unaccountable, fact. The difficulty is not so much that we are unable to analyse the compound sensation, as to explain why our inability is limited to yellow (and white). For everyone, I imagine, sees in purple a resemblance to its components red and blue, and can trace the primary colours in a mixture of green and blue. Sir John Herschel even thinks that our inability to resolve yellow leaves it doubtful whether our vision is trichromic or tetrachromic, but this seems to me to be going much too far. Surely the fact that the most saturated yellow can be compounded of red and green, deprives it of any right to stand in the same rank with them as primary colours, however little resemblance it may bear to them and blue. Besides, if yellow is to be considered primary, why not also white, which is quite as distinct a sensation as any of the others? Undoubtedly there is much that is still obscure in the mutual relations of the colours—why, for instance, as mentioned by Sir John Herschel, a dark yellow or orange suggests its character so little as to be called by a new name (brown), while a dark blue is blue still. But difficulties such as these should make us all the more determined to build our theories of colour on the solid ground that normal vision is threefold, and that the three primary elements of colour correspond nearly with red, green, and blue.[*]

J. W. STRUTT

SCIENTIFIC SERIALS

THE *Quarterly Journal of Science* for January commences a new *régime* under the sole editorship of Mr. W. Crookes. As will be seen from the following summary of its contents, all the papers, with only one exception, refer to some department of Physical Science to the exclusion of Natural History. The articles are as follows:—1. "Double Spectra," by W. M. Watts. A *résumé* of the facts known to the present time to modify the conclusions drawn from the earlier spectrum researches of Bunsen and Kirchhoff, from which it was concluded that the spectrum of each element was one and invariable. A plain and a coloured lithographic illustration show the spectrum of copper chloride when volatilised undecomposed, as contrasted with that of the metal; the different spectra of barium, strontium, and calcium obtained at different temperatures; the three spectra of hydrogen, $H\alpha$, $H\beta$, and $H\gamma$, probably due to differences in temperature; the two spectra of aluminium; the two of nitrogen; and the four spectra which are all probably due to incandescent carbon vapour. 2. "The Great Pyramid of Egypt, from a modern scientific point of view," by C. Piazzi-Smyth, Part I. 3. "On the Theory of

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[*] Direct observations, made since the above was written, show that there is no *peculiar* deficiency at the red end of the spectrum, but a general falling off as the refrangibility diminishes from one end to the other. If lights from sky and cloud are of equal intensity at the line C in the red the first will be somewhere about twice as bright as the other at B in the green. This is for a well-developed blue light taken from the zenith, but, even with a large allowance, enough difference remains to account for the discrepancies in the two sets of colour disc observations. I have lately found from theory that the power of very small particles to scatter the rays belonging to different parts of the spectrum varies as the inverse fourth power of the wave length

[*] This paper was read before Section A of the Liverpool Meeting of the British Association.

Irrigation," by F. C. Danvers. An account of the advantages that would result to agriculture from a system of irrigation, rendered necessary by the destruction of forests, irrespective of the question of the utilisation of sewage. 4. "War Science," by H. Baden Pritchard. A statement of the instruction in War Science given in the Royal Artillery and Royal Engineers, and of recent improvements in gunnery, illustrated especially by the Scott and Moncreiff gun-carriages; the improved modes of manufacturing gunpowder; and the invention of the electric torpedo in conjunction with the Abel fuse. 5. "Spectra of Metallic Compounds," from the Journal of the Franklin Institute. 6. "On the various Tints of Autumnal Foliage," by H. C. Sorby. A very interesting paper, in which the writer details the results of his experiments on the various colouring matters of leaves, &c., which he classifies as follows:—(1) *chlorophyll*, or the green colouring matter; very rarely found pure, even in fresh leaves; insoluble in water, but soluble in alcohol or bisulphide of carbon; the spectra have all a very well-marked absorption band in the red, but the green is more or less completely transmitted, so that the prevailing tint is a more or less modified green; (2) the *xanthophyll*, or yellow group; insoluble in water, but soluble in alcohol and in bisulphide of carbon; the spectra show absorption at the blue end, often with more or less well-marked narrow bands; but the red, yellow, and yellow-green are freely transmitted, so that the general colour is clear yellow or orange; (3) *erythrophyll*, or the red colouring matter; soluble in water and aqueous alcohol, but not in bisulphide of carbon; show strong absorption in the green part of the spectrum; (4) *chrysophyll*, or the golden-yellow group; soluble in water and aqueous alcohol, but insoluble in bisulphide of carbon, with variable spectra; (5) the *phaiophyll* group, comprising various browns; soluble in water, but not in bisulphide of carbon; do not give well-defined absorption-bands. Mr. Sorby gives the following scheme of the relative abundance of these various groups of colours as the leaves advance towards decay.

Complete vitality and growth . . .	Chrysophyll . .	More or less bright green.
	Chlorophyll . .	More or less green-brown.
Low vitality and change . . .	Erythrophyll . .	More or less red-scarlet.
	Xanthophyll . .	More or less bright orange-brown.
Death and decomposition . . .	Phaiophyll . .	
	Humus	Less or more dull brown.

7. "On the Relations between Chemical Change, Heat, and Force, with a special view to the economy of electro-dynamic engines," by the Rev. H. Highton. Contests the theory generally accepted that a certain amount of chemical change corresponds and is interchangeable with a certain amount of heat and electric force; and that this heat again corresponds and is interchangeable with a certain amount of work or mechanical energy. The author considers that the whole subject requires a fresh, strict, and full experimental investigation. He then refers to the many different answers that have been given to the question, What is the mechanical equivalent of heat? and holds that it has never been proved that there is any such equivalent. He details in support of his view the conflicting results obtained from the elaborate experiments with a galvanic battery by such experimenters as M. Favre, M. Sorel, M. Weber, M. Kohlrausch, and Mr. Gore. 8. "Our Patent Laws."—The pages devoted to "Progress of the Sciences" again embrace Physical Science only, Physics, Chemistry, and Mechanics—an inequality which will doubtless be rectified in future numbers.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 12.—"Some Experiments on the Discharge of Electricity through Rarefied Media and the Atmosphere." By C. F. Varley.—After the labours of Mr. Gassiot, one approaches this subject with diffidence, lest he should appear to be attempting to appropriate the glory which so justly belongs to that gentleman and to Professor Grove. The nature of the action inside the tube is at present involved in considerably mystery, but some light is thrown upon the subject by the following experiments. Before describing them, however, the author wishes to observe that he has seen Mr. Gassiot's last papers,* and finds that, so far as regulating the strength of the current is concerned, he has been proceeding in a similar manner to the author. The tube principally used in these experiments contains two aluminium wire rings, the one $\frac{4}{10}$ inch in diameter, the other $\frac{9}{10}$, and separated $\frac{8}{10}$ inch, the tube 1¼ inch in diameter, 3¼ inches in length; it was one of Geissler's manufacture, was very well exhausted, and professed to contain hydrogen. A U-shaped glass tube containing glycerine and water was placed in circuit. Two aluminium wires inserted in this tube gave a ready means of reducing or augmenting the resistance at pleasure. Glycerine affords an easy means of producing very great resistances. The battery used in this experiment was a Daniell's battery, each cell having a resistance of from 50 to 100 Ohms. The resistance of the glycerine-and-water-tube was between 2 and 3 megohms; this latter resistance was made large, in order that the resistance of the tube and battery might be neglected without entailing error. The following law was found to govern the passage of the current:—1st. Each tube requires a certain potential to leap across. 2nd. That having once established a passage for the current, a lower potential is sufficient to continue the current. 3rd. If the minimum potential, which will maintain a current through the tube, be P, and the power be varied to P + 1, P + 2, &c. to P + *n*, the current will vary in strength, as 1, 2, &c. *n*. The following Tables (I. and II.) illustrate this; there is a little irregularity in the figures due to the irregularity of the battery, although it was recharged for the occasion.

TABLE I.

1.	2				3.	4.
Cells of Daniell's Battery, P + *n*.	Observed Deflections of Reflecting Galvanometer.				Mean.	3rd Col. divided by *n*.
307 = P + 3	0	0	0	0	0	0
308 = P + 4	5	5½	5	5½	5¼	1·3
309 = P + 5	9	9	9	9	9	1·8
310 = P + 6	12	12½	12	12½	12¼	2·04
311 = P + 7	14	14	..	..	14	2
312 = P + 8	16	.	..	..	16	2
313 = P + 9	17½	18	18	..	17¾	1·97
314 = P + 10	19½	15½	.	..	19½	1·95
315 = P + 11	21½	21	22	21½	21½	1·95
316 = P + 12	23½	23½	23½	..	23½	1·96
317 = P + 13	25½	..	..	..	25½	1·96
318 = P + 14	27½	..	..	..	27½	1·97
319 = P + 15	29½	..	..	.	29½	1·97
320 = P + 16	31½	31½	31	31	31¼	1·95
*323 = P + 19	37½	38	37	37½	37½	1·97
325 = P + 21	40¾	41	40½	..	41¼	1·94
330 = P + 26	51	51	..	..	51	1·96
335 = P + 31	60½	60½	..	..	60½	1·95
340 = P + 36	70	70	..	..	70	1·94
345 = P + 41	79½	79½	..	..	79½	1·94
350 = P + 46	89	89	..	..	89	1·94
355 = P + 51	98½	98½	..	..	98½	1·93
360 = P + 56	108	108	..	..	108	1·93
365 = P + 61	118	118	..	..	118	1·94
370 = P + 66	128	128	..	..	128	1·94
375 = P + 71	139	140	..	..	139½	1·96
380 = P + 76	150	150	..	..	150	1·97

TABLE II.

1.	2				3.	4.
304 = P + 0	0	..	..	..	0	0
305 = P + 1	2	2	..	..	2	2
306 = P + 2	4	4	..	..	4	2
307 = P + 3	6	6	..	..	6	2
308 = P + 4	8	8	..	..	8	2
309 = P + 5	10	10	..	..	10	2
310 = P + 6	12	12	..	..	12	2
320 = P + 16	31½	32	..	..	31¾	1·97
330 = P + 26	51	51	..	..	51	1·96
340 = P + 36	71	71½	..	..	71	1·97

It thus appears that a certain amount of power is necessary to spring across the vacuum; after that it behaves as an ordinary conductor, excluding that portion of the battery whose potential is P, and which is used to balance the opposition of the tube. In these experiments P was 304 cells. The tube in question could not be persuaded to allow a current of less than 323 cells to pass; but when once the current had established a channel, on lowering the potential by *short circuiting* portions of the battery, so as not to break the circuit, the current would flow when the battery was reduced to 308 cells. By, however, passing a current from 600 cells through the second tube W, filled with pure glycerine, and offering several thousand megohms resistance, an extremely feeble current, too weak to affect the galvanometer, kept a channel open by its passage; with this arrangement the figures in Table II. were obtained, which are more regular at the commencement, and a power of P + 1 would pass across the tube. The positive pole alone was observed to be luminous when the current was very minute, and the negative only was luminous when the current was strong. The following experiments were tried, and the results, which have been photographed, accompany the paper. A current was passed through the tube U and the vacuum; the U tube contained pure glycerine, and had a very large resistance, which was gradually reduced. At the commencement it was more than 10,000 megohms; the upper or small ring was positive, the lower ring was negative. The power was so reduced that the faintest possible light only was visible; in this case the positive wire alone was luminous, whether it were the large or small ring that was positive. The light was so feeble that, though the experiment was conducted in a perfectly dark room, we were sometimes unaware whether the current was passing or not. An exposure of thirty minutes' duration left, as will be seen, a very good photographic record of what was taking place; this means of viewing light too feeble for the eye may receive other applications. The resistance was then reduced, when the light became much more brilliant,—a tongue of light projected from the positive pole towards the negative, the latter being still almost completely obscure. The light around the positive pole was to all of our eyes white, while the projecting flame was a bright brick-red. This bright brick-red, however, possessed great photographic power. The negative wire at this stage began to show signs of luminosity. As the power was increased, the flame became detached from the positive pole. On still further increasing the power, the positive pole ceased to be luminous; and on still further increasing the power, by removing the U tube altogether, the light surrounded the negative wire, the photograph shows a white flattened hour-glass, apparently detached from the wire; to the eye, however, the wire appeared to be surrounded by a bright blue envelope $\frac{1}{4}$ inch in diameter, which did not possess sufficient photographic power to leave a record of itself, while the red portion did so. A large condenser was now attached to the battery, and discharged through the tube (the condenser had a capacity of 27 microfarads); this was equivalent to a momentary contact with a battery of little or no resistance. The flash was exceedingly brilliant to the eye; it could be heard outside the tube with a sharp click; the eye, however, was so dazzled as not to be able to see its shape. The light was confined entirely to the *positive pole*; thus, then, as the power is increased from nothing upwards, the first pole to become luminous is the positive; secondly, the two poles become luminous; thirdly, the negative pole alone is luminous; and fourthly, with an instantaneous discharge, the positive pole only is luminous. When the resistance in the U tube was greatly reduced, and a galvanometer (not very sensitive) was inserted, so that the chief resistance in circuit was that of the exhausted tube, as the potential was augmented cell by cell, the changes took place abruptly and suddenly. For example, when the power was so low that the positive pole only was visible, the current was feeble, and kept augmenting in power as cell after cell was added on. Suddenly the luminous red flame made its appearance, and the galvanometer showed that the current had suddenly augmented three or four times in power. As the power was again further increased, cell by cell, the current again steadily augmented in proportion, until the luminous tongue suddenly disappeared, the galvanometer showing a still further sudden increase in the current.

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* Proc. Roy. Soc., vol. xi. p. 329, & vol. xii. p. 329.

* This power, 323, was the lowest at which the current would jump.

Nature of the luminous cloud.—Plücker has shown that when such an exhausted tube, with a current through it, is placed between the poles of an electro-magnet, a luminous arch is produced, which arch follows the course of the magnetic rays. As the electro-magnet is magnetized, the tube, which before was full of a luminous cloud, is seen gradually to change; the magnet gathers up this diffused cloud, and builds up an arch. Inasmuch as the electricity was passing in a continuous current from the battery, from wire to wire, it is evident the light is projected right and left into those parts of the tube where there is no electric current flowing. To endeavour to ascertain the nature of this arch, a tube was constructed. A piece of talc, bent into the form U, had a fibre of silk stretched across it; on this fibre of silk was cemented a thin strip of talc, 1 inch in length, $\frac{1}{10}$ inch broad, weighing about $\frac{1}{10}$ of a grain. The tube was sealed up and exhausted; carbonic acid and potash were used to get a high vacuum. When the magnet was not magnetized, the passage of the current from wire to wire did not affect the piece of talc. When the magnet was charged, and the luminous arch was made to play upon the lower portion of the talc, it repelled it, no matter which way the electric current was passing. When the tube was shifted over the poles of the magnet so as to project the luminous arch against the upper part of the talc, the upper end of the talc was repelled in all instances; the arch, when projected against the lower part of the talc, being near the magnet, was more concentrated, and the angle of deviation of the talc was as much as 20°. When the upper part of the arch, which was much more diffused, was thrown upon the upper part of the talc, it was repelled about 5°. This experiment, I think, indicates that this arch is composed of *attenuated particles of matter projected* from the negative pole by electricity in all directions, but that the magnet controls their course, and these particles seem to be thrown by momentum on each side of the negative pole, beyond the limit of the electric current. This arch requires time for its formation, for when a charged condenser is discharged through the tube no arch is produced. The arch from the negative pole is a hollow cylinder; the little talc tell-tale against which the arch was projected cut out the light, and a corresponding dark space existed throughout the remainder of the course of the arch. There was on the talc, at the spot where the arch struck it, a little bright luminous cloud, as though the attenuated luminous vapour was condensed by this material obstruction. Great care had been taken not to let the arch strike the single filament of silk which suspended the talc. Having demonstrated that the talc was repelled as described, the arch was allowed to play against the silk fibre, which the author expected would have been instantly burnt; such, however, was not the case. Even when a powerful induction-coil replaced the battery, the fibre remained unhurt.

Comparison of the above phenomena with discharges between the poles of a Holtz's Machine in air.—In the first part of this paper four different kinds of discharges were described *in vacuo.* With a "Holtz's" machine, which will give 11-inch sparks in the air, four well-marked different kinds of discharge have been obtained *in the air*; one of which, the author thinks, will explain the curious and rare phenomenon known as "ball lightning." In the experiments hereafter referred to, the condensers were in all cases attached to the "Holtz's" machine. The first discharge is the long 11-inch zigzag spark or lightning-flash; the second is the well-known "brush," which is best obtained by connecting the negative pole of the "Holtz's" machine to the earth; the third kind of discharge is a hissing red flame, $\frac{1}{2}$ inch in length, playing about the negative pole, the positive pole being scarcely luminous at all, and if luminous, at one or two points only; the fourth or most remarkable phenomenon is best obtained in the following manner (I should here remark that the brass balls on each of the poles are about an inch in diameter):—Tie to the negative pole a small thin strip or filament of wood three inches in length, and bent so as to project on each side of the negative pole, and a little beyond it towards the positive. On rotating the machine, two bright spots are seen upon the positive pole. If the positive pole be made to rotate upon its axis, the luminous spots do not rotate with it; if, however, the *negative pole*, with its filament of wood, be rotated, the spots on the *positive pole obey it*, and rotate also. The insertion of a non-conductor, such as a strip of glass, in front of the projecting wooden end, obliterates the luminous spot on the positive pole. When the author first discovered this, he, seeing apparently pieces of dirt on the positive pole, wiped it clean with a silk handkerchief, but there they remained in spite of all wiping; he then examined the negative pole, and discovered a minute speck of dirt corresponding to the luminous spots on the positive pole. When the filament of wood is removed from the negative pole there is sometimes a luminosity or glow over a large portion of the surface of the positive ball. If in this state three or four little pieces of wax, or even a drop or two of water, be placed upon the negative pole, corresponding non-luminous spots will be found upon the positive pole, which rotate with the former, but do not with the latter. It is therefore evident that there are lines of force existing between the two poles, and by these means one is able to telegraph *from* the negative *to* the positive pole to a distance of 8 inches through the air, without any other conductor than that which the electrical machine has constructed for itself across the non-conducting gas. The foregoing seems to the author to give a possible explanation of "ball-lightning;" if it be possible for there to be a negatively electrified cloud sufficiently charged to produce a flash from the earth to the cloud, a point in the cloud would correspond to the wood projection on the negative conductor; if such a cloud exist, a luminous spot would be seen moving about the surface of the earth, corresponding to the moving point of cloud over it, and thus present phenomena similar to those described by the privileged few who have witnessed this extraordinary natural phenomenon. The following experiment shows that, prior to the passage of the electric spark, a channel is prepared for this spark to pass. The positive and negative balls of the machine were separated to a distance of 6 or 7 inches, and a common candle-flame was placed midway between them. On rotating the machine, the flame was drawn out on each side just prior to the passage of the spark. Sometimes it extended to a width of 5 or 6 inches; this took place every time the spark passed. It is well known that the duration of this spark is less than the $\frac{1}{1000000}$ part of a second; the flame occupied the $\frac{1}{8}$ or $\frac{1}{10}$ part of a second in flying out to make the conducting channel through which the discharge went. The author has been informed more than once by captains of vessels, that when men have been struck by lightning a burn has been left upon the skin of the same shape as the object from which the discharge flew. In one instance he was informed that some brass numbers, attached to the rigging from which the discharge passed to the sailor, were imprinted upon his skin. It is now seen that this is perfectly possible if the discharge be a negative one, that is, if the man be + to the brass number.

Ethnological Society, January 10.—Prof. Huxley, President, in the chair.—Mr. Francis Hewitt was announced as a new member.—A collection of stone implements from Queen Charlotte and Vancouver Islands was exhibited by Dr. Hooker, C.B.; and some artificially distorted skulls from Vancouver Island were exhibited by Col. A. Lane Fox.—A paper was read, "On the Prehistoric Monuments in Brittany," by Lieut. S. P. Oliver, R.A., illustrated by a large collection of drawings and plans. The author first described the alignments at Carnac, which are arranged in three distinct groups—those of Menec, Kervario, and Kerlescant. At Menec there are eleven lines of stones with a circle at the south-west; at Kervario there are ten convergent lines; and at Kerlescant thirteen lines, with an enclosure of horse-shoe form. The stone avenues of Erdeven, St. Barbe, St. Pierre, and Plouhinec, were also noticed among the antiquities of the Morbihan; and attention was then directed to the alignment of Crozon, in the neighbouring Department of Finisterre. From the rude character of the stones in all these avenues and circles as compared with those in the neighbouring dolmens and menhirs, the author believes that the megalithic remains are of two distinct classes, differing considerably in date, the alignments and circles of amorpholiths being referable to a much earlier race than the dolmen-builders. Many of the tumuli of Brittany were then described, and attention directed to the archaic sculpturing upon some of the stones, and to the celts which have been found within the tumuli. Of these celts a large proportion are formed of the rare mineral *fibrolite*. The discussion upon this paper was sustained by Sir H. Dryden, Capt. Godwin-Austen, the Rev. H. Winwood, and Messrs. A. W. Franks, Hyde Clark, and Edgar Layard.—Some notes were read on a Cairn near Cefn, St. Asaph, by the Rev. D. R. Thomas, M.A., and T. McK. Hughes, M.A. The authors described two chambered tombs within the area of a large cairn which had long been removed. One of these cists was brought to light by excavations, which were undertaken in consequence of a visit to the spot by a party from the British Association after the Liverpool meeting.

NORWICH

Norfolk and Norwich Naturalists' Society, December 20.—A box of very beautiful and rare fen moths, presented by the Hon. T. de Grey to the Museum, was exhibited.—Mr. Stevenson read a paper on "The Abundance of Little Gulls on the Norfolk Coast in the Winter of 1869-70." This bird has hitherto been considered an occasional straggler on the Norfolk coast, but during the month of February last an unprecedented number appeared. It is probable that over sixty specimens were killed in this county; of these the great majority were adult males. These birds have recently been discovered to breed in considerable numbers in Lake Ladoga, further to the north and west of which it is not at present known to breed. Mr. Stevenson presumes that the stragglers, mostly young birds, which in autumn and winter appear on the coast of Great Britain, form part of that colony, which, migrating in a westerly rather than in a southerly direction, pass from the Baltic into the North Sea. The adult birds rarely approach the shore, but, from the sudden and irresistible force of the gale which visited us in February last, they were, doubtless, compelled to seek the shelter of our bays and estuaries.—Mr. Barrett stated that, since he read his paper on "The Coast Insects found near Brandon," at the last meeting, he had received some valuable evidence confirmatory of the views he then expressed. The Hon. T. de Grey informed him that he had taken *Agrestes cinerea* and *Gelechia vitella*, both of them rare sandhill insects, at Brandon, and *Gelechia marmorea* as far away as Tottington, on the Merton estate. Other species had been taken by the Rev. H. S. Marriott, of Wickham Market, and the Rev. H. Williams, of Croxton.—Mr. Geldart stated that *Phleum arenarium*, an essentially sea-side grass, was to be found growing on the sandy warrens about Brandon.

DUBLIN

Royal Geological Society, January 11.—"On the Geological age of the Ballycastle Coal-field, and its relation to the carboniferous rocks of the West of Scotland, by Mr. E. Hull, F.R.S." The object of the paper was to prove that the coal-field of Ballycastle, Co. Antrim, was referable to the type of the lower coal-field of Scotland, and consequently of the age of the Lower Carboniferous series; in other words, of the mountain limestone. The Carboniferous series of Ballycastle which had been described in 1829 by Sir R. Griffith, F.R.S.,[*] was shown to consist of three divisions in descending order. 1. *The Upper*, consisting of massive sandstones, and shales with beds of coal, black band and clay band ironstones, &c. (*Lingula squamiformis*). 2. *The Middle*, consisting of a thin bed of limestone lying between shales with carboniferous limestone genera and species of shells, crinoids, and corals. 3. *The Lower*, consisting of massive reddish grits, and conglomerate with thin beds of shale. The author showed that the carboniferous limestone of Ireland undergoes, in its extension northward, changes similar to those of the same formation in Britain, when traced from Derbyshire into Northumberland and Scotland. The calcareous element thins away, and is replaced by sedimentary strata of sandstone, shale, &c., with approximately terrestrial conditions, productive of coal and ironstone. It was thus that in the case of the Glasgow coal-field, the limestone of Derbyshire, several thousand feet in thickness, was represented by only thin bands of earthy limestone, interstratified with a thick series of grits, shales, &c., with ironstone and coal. In a similar manner the Ballycastle coal-field, with only a few feet of limestone, shown in the cliffs of the bay, was the representative of the carboniferous limestone of the centre of Ireland, nearly 3,000 feet in thickness. Mr. Hull regarded the lower division (No. 3) of the Ballycastle beds (as above described) as undoubtedly the representative of the "calciferous sandstone series" of the Geological Survey, which lies at the base of the carboniferous rock of the West of Scotland, and that the middle and upper division (Nos. 2 and 1) corresponds to the carboniferous limestone series, or lower coal-field of that country. As regards the palæontological evidence, it was in favour of this view, as far as it had been studied. Out of thirty-three species observed in the limestone band of Ballycastle Bay, 50 per cent. had been described in the Lower Carboniferous rocks of the West of Scotland,[†] and one of the uppermost seams of coal, lying above the limestone, had yielded *Lingula squamiformis*, a form characteristic of the limestone series in the North of England, Scotland, and Ireland. Mr. W. H. Baily, F.G.S., concurred in the view of the age of these beds on palæontological grounds. The author concluded by pointing out several features of similarity between the Ballycastle beds and the lower coal series of the West of Scotland, such as the occurrence of several beds of "black band" ironstone; the hydraulic and earthy character of the limestone of Ballycastle Bay, exactly resembling the "Arden" and "Cowglen" bands of Glasgow. Some uncertainty still remained whether there were any beds in the Ballycastle district as high in the Geological Series as the millstone grit, a true coal measure, but until more light could be brought to bear on this question by further exploration, and a complete investigation by Government surveyors, the author meanwhile regarded the whole series as Lower Carboniferous.

MONTREAL

Natural History Society, Nov. 28.—Dr. Dawson, president, in the chair. A paper was read upon the Canadian phosphates of lime in their application to agriculture, by Mr. Gordon Broome, F.G.S., calling special attention to the valuable apatite deposits of the Rideau district. The author gave a large amount of valuable information upon the manufacture of "superphosphates," noticing the good work being done at Brockville in this particular branch of industry, and proving that large areas of land now wholly or partially exhausted, in various portions of Lower Canada, might still have been yielding large returns of wheat and other cereals, had they been farmed upon a rational system of agriculture, coupled with the judicious employment of phosphatic and other fertilisers. We quote from the concluding words of this comprehensive essay: "He made especial reference to the comparative dormant state of this, and other equally obvious sources of industry in Canada, arising from a great deficiency in a most important division of our national education, and that nothing save a liberal augmentation of the ordinary courses of instruction in modern subjects can ever dispel the immense existing clouds of ignorance and prejudice. It is, therefore, sincerely to be hoped that the very able remarks recently made by Principal Dawson upon this question may have their desired effect, and that Canada may speedily obtain a share in the improvements that have of late almost revolutionised the systems of educations in the universities of the mother country." Dr. Hunt, Vice-President of the Association, then referred in a feeling manner to the loss sustained by science in Canada, in the death of Mr. Hartley, of the Geological Survey, who, though only twenty-three years of age, was one of the most promising young men in the country.

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[*] Report on the Coal Districts of Tyrone and Antrim, addressed to the Royal Dublin Society, 1829

[†] Trans. Geol. Soc., Glasgow.

VIENNA

I. R. Geological Institute, December 20.—M. Fr. v. Hauer stated that the Institute had happily come into possession of the only known specimen of Psephophorus polygonus from Neudörfl in Lower Austria, which had been described after a drawing more than twenty years old by Herm. von Meyer, and had been designated by him one of the most interesting fossil remains of a vertebrate animal ever found in Austria. It consists of a plate of epidermal scales nearly two hundred in number, and perfectly irregular in size and form. H. v. Meyer thought it to belong to an animal of the family of the Dasypodidæ, whilst Prof. Muller compared an analogous plate found in the Zeuglodon beds of North America with the plate of Dermatochelys.—M. F. v. Vivenot read a memoir on the microscopical examination of the syenite of Blansko in Moravia, in which, besides the minerals which are distinguishable with the naked eye, as oligoclase, quartz, amphibole, biotite, titanite, and magnetite, he discovered also orthoclase, epidote, and apatite.—M. E. Tietze exhibited specimens of cornubianite from Servia, which perfectly resembled the cornubianite of Cornwall, and like it were found in a stratum interposed between granite and argillaceous schist.—M. Posepny communicated his observations on the beds of rock-salt in Transylvania.

BOOKS RECEIVED

English.—The Transformations (or Metamorphoses) of Insects: P. M. Duncan (Cassell and Co)—Wonders of the Human Body: A. Le Pileur (Blackie and Son)—Dogs and their Doings: Rev. F. O Morris (Partridge and Co).—The Genesis of Species: St. George Mivart (Macmillan and Co.).—Matheran Hill: its People, Plants, and Animals: Dr. J. Y. Smith (Maclachlan and Stewart)

Foreign.—Vorschule der Experimentale Physik: H. F. Weinhold (D. Nutt).

PAMPHLETS RECEIVED

Prospectus of Sir Joseph Whitworth's Scholarships for Mechanical Science, 5th ed—A Letter to the Duke of Argyll on the Proposed Scheme for an Engineering College: Dr. F. T. Bond—On Equal-surface Projection and its Anthropological Applications: C Piazzi Smyth—On the Cellular Structure of the Red Blood-corpuscle Dr. J. G. Richardson (Philadelphia).—Report on the Canadian Phosphates: G Broome, pt. 1.—Eloge de Jean-Theodore Lacordaire: par E Morren.—Das Differentialphotometer u. eine neue Thermosaule: Prof K W. Zenger—Chemische Notizen. Prof. F. Stolba—Ueber Das Kieselfluorammonium u. Kieselfluornickel: Prof F. Stolba.—Ueber Erzeugung algebraischer Curven durch mehrdeutige Elementargebilde: Dr E. Weyr.—Studien zur Theorie der Covarienten u. Invarienten der binaren Formen: Dr. J Dienger.—Ueber den Zusammensetzen verschiedener Transformationsformeln für elliptische Integrale: Prof J Lieblein.—Ueber fortgesetztes Tangentenziehen au Curven dritter Ordnung: Dr. H. Durége—Beobachtungen ueber einige fossile Pflanzen aus dem Steinkohlenbecken von Radnic: C. Feistmantel.—Beobachtungen uber die Entstehung einiger spharoidischer Gebilde im Mineralreiche: C Feistmantel.—Die Porphyre im Silurgebirge von Mittelböhmen: C. Feistmantel.—Die Steinkohlengebilde in der Umgebung von Radnitz in Bohmen: C. Feistmantel—Beitrage zur Lehre der universellen Summirung von Strecken: Dr. W. Martzka.—Die Satze von Bürmann u Lagrange. Dr J. Dienger.—Ueber die Normalenflache zum dreiaxigen Ellipsoide: J M Solin.—Ueber die Callianassen der bohmischen Kreideformation: Dr. A. Fritsch.—Ueber die physikalischen Constanten des Wasserdampfes: Prof G. Schmidt.—Ueber die neueren physiologisch-psychologischen Forschungen im Gebiete der menschlichen Sinne: Dr J. Dastich—Auflosung der Gleichungen des 2^{ten}, 3^{ten}, u 4^{ten} Grades: J Machovetz—Beitrage zur Weddle's Methode der Auflosung numerischer Gleichungen: J. Popper.—Beiträge zur Auflösung hoherer Gleichungen: Dr. J. P. Kulik.—Ueber das Bourdonsche Metallbarometer: Dr V. Pierre.—Ueber das Binocularsehen: Dr. J. Ritter von Hasner—Die Methode der orthonogalen Projekzion auf zwei Ebenen: Prof. R. Skuhersky.—Parallèle entre les dépôts Siluriens de Bohème et de Scandinavie—Resultate mehrjähriger Beobachtungen uber jene Pflanzen deren Blumenkronen sich taglich periodisch offnen u. schliessen: K Fritsch.—Grundzuge einer Meteorologie für den Horizont von Prag: K. Fritsch.

DIARY

THURSDAY, January 19.

Royal Society, at 8 30—On the Structure and Development of the Skull of the Common Frog: W. K. Parker, F.R.S—Method of Measuring the Resistance of a Conductor: H. Mance—Modification of Wheatstone's Bridge to determine the Resistance of a Galvanometer Coil from a single Deflection of its own Needle: Sir W. Thomson, F.R S

Society of Antiquaries, at 8.30.—The Pre-Christian Cross; H. M Westropp.

Linnean Society, at 8.—On the Vegetation of the Solomon Islands: Mr. Atkin.—Note on Byrsanthus (*Homalineæ*): Dr. M T. Masters, F.R S, F L S.—Historical Notes on the *Radix Galanga* of Pharmacy: Daniel Hanbury, F.R S, F.L.S.

Chemical Society, at 8.

Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

FRIDAY, January 20.

Royal Institution, at 9—Dr. Tyndall.

SATURDAY, January 21.

Royal Institution, at 3—Laws of Life revealed in History: Rev. W. H. Channing.

Association of Officers of Health, at 8—On Parasites, in relation to the Sewage Irrigation Question: Dr. Cobbold.

SUNDAY, January 22.

Sunday Lecture Society, at 3.30—How Coal and the Strata in which it is found have formed: A. H. Green.

MONDAY, January 23.

London Institution, at 4.—On the First Principles of Biology: Prof. Huxley (Educational Course).

Royal Geographical Society, at 8.30

Entomological Society, at 7.—Anniversary Meeting.

TUESDAY, January 24.

Royal Institution, at 3.—Nutrition of Animals: Dr. Foster.

Ethnological Society, at 8.—On the Languages and Tribes of East Africa: Rev Dr. Steere.—On African Weapons and Implements: Dr. Eyschmacher.—A Zulu Law Case: Sir John Lubbock, Bart, M.P.

WEDNESDAY, January 25.

Society of Arts, at 8.—On New Paper-making Materials, and the Progress of the Paper Manufacture: P. L Simmonds.

London Institution, at 7.—Dust and Disease: Prof. Tyndall. (Conversazione)

Geological Society, at 8.

THURSDAY, January 26.

Royal, at 8 30.

Society of Antiquaries, at 8 30

London Institution, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff.

Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

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THURSDAY, JANUARY 26, 1871

PHYSICAL LABORATORIES

It is well known that chemistry can be taught far better by a laboratory in which the student performs the various experiments, than by any system of lectures. Now, although for many years physicists have been in the habit of instructing their special students and assistants in this way, yet it is only recently that the same plan has been tried with large classes in physics. One of the first institutions to attempt this method, in America at least, was the Massachusetts Institute of Technology in Boston; and as I find many colleges here establishing physical laboratories, I trust that our experience may prove of some interest. The great difficulty is to enable twenty or thirty students to perform the same experiment without duplicating the apparatus, and to avoid the danger of injury to delicate instruments. Our plan is this:—Two large rooms (one nearly a hundred feet in length) are fitted up with tables, supplied with gas and water, somewhat like a chemical laboratory. On each is placed the apparatus prepared for a single experiment, which always remains in this place, thus avoiding the danger of breaking it in moving. A full written description is also given of each experiment, pointing out the proper precautions to avoid error or breakage. Near the door is an indicator or board containing the names of the experiments, and opposite each is placed a card bearing the name of the student. When the class enters the laboratory, they go to the indicator, and each member notices what experiment is opposite his name; he then goes to the proper table, reads the description, and performs it. He next reports his results to the instructor in charge, and if they are correct, his card is moved to some unoccupied place, and he proceeds as before. Care is taken that the number of experiments shall exceed that of students, and there is therefore no delay. The instructor in the mean time is enabled to pass from student to student and to see that no errors are committed. As quantitative work is far more valuable than qualitative, most of our experiments are of the former kind, and the student learns to measure physical constants and to verify laws numerically. For example, in one experiment a steel bar is supported on knife edges, and a weight is applied at the centre. The flexure is then measured by a micrometer screw, the exact point of contact being determined by including the screw and bar in the circuit of a battery and galvanometer. After making a number of experiments with various weights, the student constructs a curve in which ordinates represent deflection, and abscissæ weights applied. The law of elasticity shows that this curve should be a straight line, and the close agreement is convincing proof to the student of its correctness. In the same way the law of the conjugate foci of lenses is tested, and the observed curve compared with that deduced from theory. Some experiments are introduced to accustom the student to general methods of research, such as the computation of probable error by least squares, various forms of interpolation, &c. The graphical method is largely used, as it at the same time enables the student to take in all his observations at one glance, while the instructor can constantly tell how carefully the work has been done. For the microscope a few objects are selected to show certain general methods of using this instrument, as one requiring a diaphragm, a second oblique illumination, and so on. Again, the student views by polarised light such objects as unannealed glass, crystals, designs in selenite, and studies the effects produced by various agencies. By thus handling the instruments he acquires a facility in using them and comprehension of their construction which he could never obtain from lectures. The excellence of the work done by many of the students led to the hope that valuable results might be attained by assigning to different students the experiments in a research, taking care that each should be repeated several times by different individuals. These results, if concordant, would be much more conclusive than those obtained by a single experimenter, since they would be free from all personal bias. In this way some interesting results have been attained on the foci of lenses placed obliquely, the flow of air through straight and curved tubes, and other similar subjects. Photometry and electrical measurement seem especially suited to this purpose, and the application of the latter subject to submarine cables would be both interesting and instructive to the student. During the winter time of 1869 and 1870 about sixty students worked in one laboratory, so that the experiment was tried on a sufficiently large scale to enable us to speak with confidence of its success. We found the system described above worked well, the students were interested in the subject, and obtained results of considerable accuracy. The loss by breakage was exceedingly small, and the current expenses insignificant compared with a chemical laboratory, since there is but little consumption of the materials employed.

There are now in America at least four similar laboratories either in operation or preparation, and the chances are that in a few years this number will be greatly increased. The value of a knowledge of physical manipulation is becoming daily better appreciated, and it is evident that instruction of this kind can be properly given only in a physical laboratory.

EDWARD C. PICKERING

SCIENCE TEACHING IN PRIVATE SCHOOLS

A WRITER of the early part of last century defined a philosopher as one "whose trade was to do nothing, and to speculate upon everything." While philosophers were so lightly esteemed, it is no matter of surprise that philosophy was little cared for as a part of education. But such a definition as the above would not now be generally accepted even by the unscientific public. All are beginning to see that it is to Science they are indebted for so many of the comforts and advantages of civilisation, yet to the many is Science a mystery and closed book. And one great cause of this we believe to be, because it is not taught in our schools.

We purpose, in the present article, to speak only of private schools. If a visitation were to be made of such schools in England, we venture to say that very few, comparatively, would be found, in which Science, in any of its branches, is made a subject of regular education. The boys of most schools would be classed by the masters under three heads, classical boys, mathematical boys, and good-for-nothing boys. This last class exists mainly because the proper food for them has not been provided, they are allowed to starve for lack of it, and grow up as men with stunted and impoverished intellects; they have not been educated, the powers of their minds have not been drawn out by the fit means, and they pass through the world as animated failures.

Let Science work side by side with Classics and Mathematics—not usurp their places—in the work of education, and the good-for-nothing class will be very sensibly diminished, if indeed it be not entirely done away with. But how is this to be done? In the first place, gradually; in the second place, zealously; in the third place, thoroughly. Gradually, because it is a new thing, and a large proportion of our private schoolmasters have had no regular training in science themselves. Zealously, because if a teacher be not himself interested in what he teaches, he can have no assurance of success, and no encouragement from his pupils. Thoroughly, because a thing worth doing at all is worth doing well.

This is the spirit in which the work must be done. but what are the special means? Are boys to read about Science merely, or are they to touch and handle Science for themselves? It is doubtless a good thing to read about the truths of Science and their experimental illustration; it is a better thing to see those truths illustrated and proved by another; but it is by far the best thing to experiment upon and prove the truths by one's self. There is nothing that comes home so much to a boy's mind as an experimental proof. He may read of the dual character of electricity, and may get some vague ideas on the subject; as soon, however, as he takes two sticks of sealing-wax, suspends one, rubs both, and brings one near to the other, he, as it were, discovers for himself that the same bodies electrified by the same means repel one another, and on experimenting with glass and sealing-wax sticks he learns something of attraction, and is naturally led on from experiment to experiment until the powers of his mind become quite drawn out, or educated in the pleasurable pursuit of the subject. In the Science-teaching of boys, then, practical demonstrations must play an important part. Let reading, hearing lectures, and attending classes, and individual experimentation, be the working tools. A lecture of itself is but a poor tool, it produces an effect for the time, but in many cases no very permanent good. A lecturer must also be a teacher out of lecture hours.

As a commencement of Science-teaching in schools we commend the following plan to the notice of Science-teachers. Let one or two evenings in the week be set apart for lectures on some branch of Science, Experimental Physics, Botany, or Geology. Each lecture to last *not more* than an hour, and to be experimentally illustrated in the way best suited to the subject, always bearing in mind that the simplest experiments, or those most easily imitated by the pupils, are the best. Let the pupils be encouraged to take notes, and let the lecturer sum up in a concise form at the end of the lecture the main points established, which may be written in the form of memoranda on a black board and copied by the pupils. A day or two after the lecture let him hold a conversational class, the attendance optional. He will then briefly run over the matter of the last lecture, find out by questioning what points are not thoroughly understood, re-explain or even re-experiment, and endeavour to leave each mind with a perfect understanding of fundamentals. On a third evening let him give a series of simple—not needlessly puzzling—examination questions, and look over each boy's answers with himself alone, if possible, in order to give an opportunity for a yet more thorough explanation of any difficult point suited to the individual capacity of each. Instead of a string of questions, a subject for an essay in connection with the lectures might occasionally be given. Private reading of text-books should always be encouraged.

Now, as to the results of a system of this kind, taking such a subject as Experimental Physics, it will be found that the lectures are always looked forward to with no ordinary pleasure, and are listened to with no ordinary attention. At the conversational classes the way in which such subjects teach boys to think is often clearly seen; they ask most puzzling questions, yet natural ones, and in many cases seek to go far deeper into the subject than the lecturer had at first any idea of leading them. This general interest incites them to read and perform such simple experiments for themselves as are within their power.

Generally speaking it is not the high classical or even the mathematical boys that have excelled in Science learning, but precisely those who before occupied no prominent place in the school, had no special gift for classics or mathematics, and were considered, more or less, good-for-nothings. And here it is important to remember that a person may have a mathematical mind without being a mathematician.

While such subjects as Chemistry and Physics claim, perhaps, the highest position as a means of scientific education, it is important to vary the programme as much as possible without treating any superficially. Thus Astronomy, Geology, Physiology, and Botany have strong claims. It is certainly most deplorable to think that even now in many of our private schools the pupils are being tacitly taught that the world was made in six days, and that man is but some 6,000 years old. They might as well learn that there are but four elements—earth, air, fire, and water. We look with confidence for better things in the future.

COHN'S CONTRIBUTIONS TO THE BIOLOGY OF PLANTS

Beiträge zur Biologie der Pflanzen. Herausgegeben von Dr. Ferdinand Cohn. Erster Heft. Mit sechs zum theil farbigen Tafeln. (Breslau, 1870. London: Williams and Norgate.)

THIS is the first part of a new periodical established primarily for the publication of the results of the observations made at the Botanico-Physiological Institute at Breslau. The part contains five papers on different microscopic algæ and fungi, and their pathological effects. In subsequent numbers it is intended to give priority to botanical observations which relate to biological questions, or which are more or less connected with practical natural science, medicine, agriculture, &c. It is hoped that the publication may fill the place formerly occupied by Karsten's "Botanische Untersuchungen."

We propose here to give a short account of the contents of the present number. The first paper (by Dr. J. Schroeter) is on the parasites of the genus *Synchytrium*. The *Synchytria* form a small group of parasitic cryptogams, remarkable for their peculiar mode of development and the absence of any mycelium. They produce small swellings, or "galls," on the plants in which they occur, but in comparison with other parasites cause little injury or distortion. The genus was established by De Bary and Woronin in 1863 for the reception of the plant now known as *Synchytrium Taraxaci*. Several other species have since been discovered, the number of those described by Dr. Schroeter in the present paper amounting to eleven. The plants were divided by Woronin into two groups. In the first the protoplasm of the cells is white, and the zoospores pass at once into the condition of resting spores. In the second group the protoplasm is reddish, and the zoospores, attaching themselves to living plants, produce spherical masses of zoosporangia until the close of the vegetative period, when they produce resting spores. Dr. Schroeter proposes three groups, which he calls *Eusynchytrium*, *Chrysochytrium*, and *Leucochytrium*. The first corresponds to Woronin's second group, the second to Woronin's first group, and the third comprehends certain *Synchytria* which agree with those of Woronin's first group except in having white protoplasm. Dr. Schrœter describes the species with great care, and in considerable detail, and the descriptions are for the most part illustrated by excellent plates. He considers the genus to be one of wide distribution. With regard to its systematic position, there has never been any doubt that *Synchytrium* belongs to the *Chytridiaceæ*, but whether the latter are fungi or algæ is not so well settled. The conclusion at which the author has arrived is that they should be placed as a distinct family amongst those *Palmellaceæ* which produce zoospores, being nearly related to *Hydrocytium*, *Codiolum*, and their allied genera.

In the second paper ("Ueber die Faule der Cactusstamme") MM. Lebert and Cohn describe a new species of *Peronospora*, which was most destructive to a collection of cactuses, affecting particularly *Cereus giganteus* and *Melocactus nigrotomentosus*. The epidermis of the cactus was not materially altered, but the cellular tissue beneath became entirely rotten, the decay beginning with the inter-cellular substance: so that the parenchymatous cells were easily separable from one another. The contents of these cells became brown, and the cell-membrane almost entirely dissolved, the cactus thus exhibiting an internal state of decay similar to that seen in diseased potatoes. The fungus is closely allied to the potato blight, *Peronospora infestans*, which it resembles in having the hyphæ only slightly ramified, not dichotomous and therefore producing but few conidia, as well as in its delicate mycelium (which has no suckers) and in the swellings underneath the large, beaked conidia. The authors suspect that the fungus must have been imported with the cactuses from America.

The third paper, by Dr. Cohn, is entitled "Ueber eine neue Pilzkrankheit der Erdraupen," and contains a detailed account of a fungoid disease affecting the caterpillars of *Agrotis segetum*. The skin turns black, a coal-black pigment appears in the blood, and the caterpillar becomes a wrinkled and brittle mummy. Upon examining the interior the whole cavity of the body, except the intestinal canal, is full of a black tinder-like substance, consisting of very large, dark brown spores, which are globular, but sometimes wrinkled at the surface, so as to present a crenate outline under the microscope. Contemporaneously with the colouring of the blood, cylindrical or curved tubes (*Schläuche*) are seen, which become divided by septa, and form rows of cells which separate from one another and appear in the blood as free globular or oval cells. These Dr. Cohn calls *gonidia*. These gonidia germinate shortly before the death of the grub, and produce a mycelium which displaces the inner organs, except the intestinal canal and the tracheæ, and fills the hollow of the body. This mycelium produces partly new gonidia and partly the dark spores already mentioned. The latter are probably resting-spores, as it seems from Dr. Cohn's observations that they do not germinate until the spring. The fungus (to which the author gives the name *Tarichium*) appears to be generically the same as Fresenius's *Entomophthora*, and to be closely allied to *Empusa*. Dr. Cohn thinks it not improbable that *Empusa* and *Tarichium* are stages of development of the same fungus; that *Empusa* may be the conidial form of a fungus of which *Tarichium* represents the teleutospores. If so, *Empusa* bears the same relation to *Tarichium* as *Oidium* does to *Erysiphe*, *Uredo* to *Puccinia*, and (perhaps) as the *epi*phytal conidia of *Peronospora* to the *endo*phytal oospores.

We have but little space to notice the two remaining papers. One is by Dr. Schroeter, and relates to a disease affecting *Pandanus*, which was observed many years ago by Sinnig, the Inspector of the Botanic Garden at Poppelsdorf, and which has since been investigated by Bouché, at Berlin. In the spring of the present year it attacked a splendid specimen of *Pandanus odoratissimus*, Jacq., in the gardens at Breslau; the branches decayed, and it was found necessary to cut off the crowns, afterwards the branches themselves, and eventually the greater part of the entire plant, leaving only a portion of the stem and a single branch. A strict investigation afforded no ground for supposing that the disease had been produced by cold, drip, or other causes which would suggest themselves to cultivators, but a fungus was discovered identical with one described by Leveillé as long ago as 1845, which occurred in the Botanic Garden at Paris upon *Pandanus*, and which he called *Melanconium Pandani* (Ann. d. Sciences Nat. ser. iii. t. 3, p. 66). The *Melanconium* in the present case was accompanied by a *Nectria*, which Dr. Schroeter considers to be the *Nectria Pandani*, Tul., and conidia were also observed, the mycelia of which exhibited the forms of *Tubercularia*, *Stilbum*, and *Verticillium* or *Penicillium*. The author considers it extremely probable that the *Melanconium*, the *Nectria*, and the conidia are produced from the same mycelium, the fungus thus exhibiting the following *forms of* fruit:—1. Grey-green conidia (*i.e.* the *Melanconium* fructification). 2. Colourless conidia, the supports of which assume three different forms, viz. (*a*) *Tubercularia*, (*b*) *Stilbum*, (*c*) the form of such moulds as *Verticillium* or *Penicillium*. 3. Spores in asci formed in orange-red perithecia seated on a *Stroma*.

Dr. Schroeter concludes that there can hardly be a doubt that the fungus is the direct cause of the progress and of the destructive operation of the malady, and that it is probable, though not so certain, that the spores germinate in the healthy stem, and actually produce the disease.

The last paper contains a description by Dr. Cohn of a plant which he has discovered in well-water at Breslau, and to which he has given the name of *Crenothrix polyspora*. The genus is new and is closely allied to *Chamæsiphon*, being intermediate between that and *Lyngyba*. It would take too much space to describe the plant at length, for the particulars of which we must refer to the paper, in which will be found, moreover, some interesting observations upon the microscopical analysis of well-water in general. The *Crenothrix* was first noticed in water from a well at Breslau, in a part of the town notorious for the prevalence of typhus. It has been found also in other wells of bad reputation, but whether it has any injurious effect upon the health of the dwellers in the neighbourhood of the wells in question Dr. Cohn cannot venture to say.

It will be seen, from what has been said, that the periodical under notice is well deserving the attention of botanists and physiologists, and from the reputation of its editor there is every reason to hope that the scientific interest of future numbers may equal that of the present one.

F. CURREY

OUR BOOK SHELF

Matheran Hill, its People, Plants, and Animals. By J. Y. Smith, M.D. (Edinburgh: Maclachlan and Stewart.)

THIS is perhaps the first attempt that has been made to give a comprehensive account of the natural history of any particular spot in our East Indian possessions, and we welcome this little book as a sample of what may be accomplished by residents there in the midst of their official occupations. The undertaking is worthy of all praise, and, as far as it goes, it is a valuable contribution to the ethnology and natural history of India, and will no doubt be the groundwork for further research, and lead to other similar works.

The hill of Matheran is within twenty miles of the coast of Bombay, it is basaltic, and rises in the centre of a vast plain to the height of about 2,600 feet, it is precipitous on all sides, flat on the summit, which has an area of about five square miles, and is clothed with luxuriant vegetation; it is, in short, one of the most charming spots in India. In less than three hours one can be transported from the heat, dust, and noisy traffic of Bombay to what appears to be another world, where the body is refreshed and invigorated by the pure mountain air, and the spirit soothed by the beauty of verdant foliage, the cheerful music of feathered songsters, scenery grand and picturesque, and the general repose of nature To the naturalist Matheran opens a grand volume to him, the little plateau has an air of enchantment, and he has spread out before him in the most attractive form objects which will supply him for even years with delightful and instructive occupation without the toil and exposure of long journeys. To such Dr. Smith's account of the more prominent objects to be met with will therefore be a valuable boon, and, indeed, to all lovers of nature who may visit the place. Let the home botanist imagine himself in a pretty little cottage on Matheran, with no less than seventy-five flowering trees and shrubs within a mile of his residence; not to mention climbing plants, creepers, herbs, parasites, and ferns in abundance. As the author is now returning to the East, we hope he will have opportunities of extending his interesting observations, especially on the birds and insects. To the geologist, Matheran is an object of interesting study; he will there see that curious rock called laterite or iron stone clay, the nature of which has been so much disputed, capping the great basaltic formation (to use a homely phrase) like the sugar on a Christmas cake, and if he extends his observations to the north and south, he will find the same capping on other hills, thirty to sixty miles distant, while no trace of it exists in the interval between. To what bold speculations does this fact give rise? Did this laterite once cover the whole country as with a mantle, and are present appearances due to a vast denudation of hard trappean rock 2,000 to 3,000 feet deep?

We are glad to find natural history included among the subjects for Indian Civil Service examinations, for hitherto this class of Europeans in India have contributed comparatively little to our knowledge of that wonderful land "where all, save the spirit of man, is divine."

A Voyage Round the World. By the Marquis de Beauvoir. 2 vols. (Murray, 1870.)

THIS is the gossiping journal of a young companion of the Duc de Penthièvre, son of the Prince de Joinville, often amusing and spirited, but of little permanent value. We have the usual exaggerations of a novice in the tropics. At Batavia he speaks of "this torrid temperature of 104° in the shade," a degree of heat never experienced by the present writer during many years' residence in those regions. The author's scientific attainments may be estimated by his account of his visit to the Melbourne Museum, when he makes Prof. MacCoy speak in this fashion:—"The stratum of alluvial soil covering the crust of primitive rocks, which formed round the earth while it was still in a liquid and incandescent state, possesses the same specific type of animal life that characterises the ancient strata of Wales, Sweden, and North America. Then come soils identical with those of these countries, schist and fossil rocks; thus Canada, Scotland, and the province of Victoria have all passed through the same form of existence at this remote period." The countries described are Australia, Java, Siam, and Canton, and the whole journey occupied about six months.

A. R. W.

The Student and Intellectual Observer. A Quarterly Journal of Science, Literature, and Art. Vol. 5. (London: Groombridge and Sons.)

THE volume now before us is in every way worthy of the reputation of its predecessors. Four papers on poison are contributed by Mr. F. S. Barff, and Dr. Carpenter contributes two interesting papers on the "Deep Sea;" the first on its physical, and the second on its biological condition. The author's experience in these matters, owing to his connection with recent explorations, make the papers the more interesting, because they are the words of an actual and accurate observer. Dr. Collingwood also gives a very readable paper on a kindred subject, "The Sargasso Sea and its Inhabitants," in which the Sargassum or Gulf-weed comes in for a good share of attention, being, as it is, the home of multitudes of Polyzoa, Polyps, Crustacea, Molluscs, and similar creatures. Mr. Shirley Hibberd talks about Cycads under the very misleading title of "Sago Palms." At one time the Sago of commerce was supposed to be the produce of the Cycads, but now we know that the bulk of this useful article is yielded by two or more species of *Sagus*, true palms; it is, to say the least, advisable that an old term proved to have been wrongly given, should not be perpetuated. The author, however, does attempt to qualify its use in the following sentence:—"By 'Sago Palms' is to be understood the great group of gymnospermous plants, of which the Cycads and their allies are representatives, a group possessing powerful morphological relations, and, of course, a correspondence within certain limits in all their biological characteristics." The volume contains many other interesting papers in various branches of science, and we conclude this short notice by wishing well to an old-established monthly in its new quarterly form.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Isolation of St. Michael's Mount

MR. PENGELLY, in a letter addressed to you, and printed in your journal of Jan. 12, 1871, complains of my having ascribed to him a belief in the extreme antiquity of the Cornish language which he does not hold now, and which he did not hold at the time when he delivered his first lecture "On the Isolation of St. Michael's Mount," at the meeting of the British Association at Birmingham in 1865. He declines to be responsible for any notices or report of his lecture that I may have seen in some newspaper or journal.

All I can say in my defence is that even while the meeting at Birmingham lasted, I received not only newspaper reports, but letters from friends who had heard the lecture, and who asked me in great dismay whether it was possible that a Cornish name, such as *Cara clowse in cowse*, meaning "the hoar rock in the wood," could have existed in so called prehistoric times.

The question discussed at the meeting, so far as I could understand it from letters and the official short report in the Transactions of the British Association, was this, whether St. Michael's Mount was severed by encroachment or subsidence. Those who held the former view required 20,000 years, those who held the latter were satisfied with a smaller number of years, though I could not find out exactly what that number was. Both parties maintained that Cornish must have been spoken in Cornwall before the severance of the Mount took place, because only before that severance could the Mount have been called *Cara clowse in cowse*, "the hoar rock in the wood."

What I wanted to show was simply this, that neither party could properly avail itself of the linguistic argument, whether for positive or negative purposes. If the Mount was severed 20,000 years ago, it would not follow from the name *Cara clowse in cowse*, "the hoar rock in the wood," that Cornish was spoken at that distant time; nor would it in the least follow from that name that the severance could not have taken place until Cornwall was occupied by Celtic speaking tribes. The linguistic and geological evidence can in no wise be brought to bear upon each other.

If I said that "Mr. Pengelly has somewhat modified his former opinion," all I meant was that in his second paper he himself seems much less inclined to trust to the linguistic and legendary evidence. But if in his letter to you he says that the point of his argument was that the hypothesis of insulation by encroachment without subsidence could not be admitted because it led to an untenable philological conclusion, this shows that the old leaven is still at work. If the facts which I collected in my essay on the Insulation of St. Michael's Mount are right, that hypothesis would lead to no untenable philological conclusion whatever, for the simple reason that the name *Cara clowse in cowse*, or "hoar rock in the wood," referred originally to Mont St. Michel, in Normandy, if not to Mons Garganur in Apulia, and does not occur in Cornwall before the 16th, or possibly the 15th century of our era.

If I have in any way misrepresented the exact geological reasoning of Mr. Pengelly, all I can do is to plead the ignorance of a layman, and to ask his forgiveness.

Oxford, Jan. 23 MAX MÜLLER

Earth-Currents

IN Mr. W. H. Preece's communication concerning the earth-currents which occurred on the 24th and 25th of last October in England, published in your issue of the 3rd of November, just come to hand, he says: "This is only a sample of what occurred simultaneously all over England, and probably the globe."

The following few extracts from the Log of the Madras-Bombay Lines show what was taking place out here:—

"Oct. 24, 22 hours, working Bellary with great difficulty. Severe lightning taps on instrument every now and then." "Oct. 25, 8 hours, strong earth-currents at times.—17 hours to 17·45 hours, very strong earth-currents.—22 hours, working Bombay with frequent stoppages, owing to strong earth-currents and failure of signals." Such is the character of the log throughout the 24th and 25th of October.

The direction of the Madras-Bombay Lines (nearly east and west) would account for the fact of the earth-currents being so strongly pronounced in them.

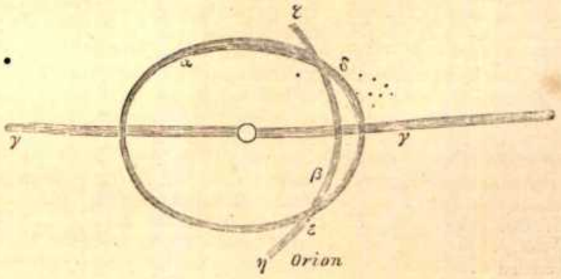
R. S. BROUGH,
Assistant Superintendent of Government
Telegraphs in India

Lunar Bows

A REMARKABLE phenomenon was visible at Liverpool from 7.30 to 7.45 P.M., on Wednesday, 4th inst.

The moon was nearly full at an altitude of 45° or 50°, just above Orion, the sky was covered with a slight mist sufficiently dense to obscure all stars except those of the first magnitude, though here and there some of the lesser were visible.

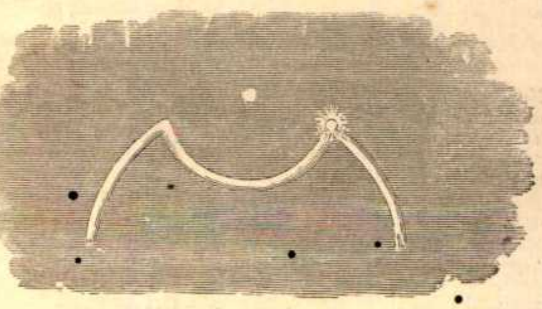
There appeared three lunar bows or halos—α β γ in sketch. α was nearly but not quite a perfect circle, having a slight tendency to an oval; it was complete. β was an excentric cutting α at points δ and ε, and was incomplete, only having about 90° of its circle visible, being lost to sight at ζ and η. γ had the zenith for its centre, cutting the moon; its circle was complete, intenser in the western sky and dimmer at its nearer proximity to the moon, thus forming a belt round the whole heavens, cutting off the upper portion.



I venture to consider these interesting sights worthy of a space in NATURE, as they took up so large a space in the heavens.

Liverpool F. J. J.

ON Wednesday evening, January 4th, while the frost still lasted, there appeared, about 7.25 P.M., two brilliant halos, of which a sketch is forwarded. Neither was perfect in outline. The greater spread out literally from the smaller, and made a circuit of the heavens, so that if an observer stood with his back to the moon, there was seen facing him in the N.W. an inverted lunar rainbow—to all appearance. This enormous arc scarcely seemed a part of a circle on account of the great size: the zenith was about its centre. After meeting the smaller halo it was scarcely visible, but if produced would have passed through the moon's disc. At the point of intersection towards the east, a faint paraselene appeared.



When this singular sight was seen, there was but little cloud in the sky. The blue was, however, rather turbid. The prismatic colours were tolerably distinct in the inner arc. The diameter of the large circle was about 90°. There has been a considerable amount of rain and snow since the appearance.

SAMUEL BARBER

Aigburth, Liverpool, Jan. 9

Yellow

In the paper on colour read by Mr. Strutt before the British Association (Nature, Jan. 19) two things are stated as requiring explanation (p. 238), both of which, it seems to me, are explained by one of the results furnished by such experiments as his own.

The first is the difficulty usually found in recognising the demonstrated fact that yellow is a compound colour. The other is that we generally distinguish different kinds of yellow more strictly than of other colours. "A dark yellow or orange . . . . suggests its colour so little as to be called by a new name (brown), while a dark blue is blue still;" upon which I must observe that we thus distinguish degrees of impurity rather than degrees of darkness, and that an impure yellow is called brown when it is dark, and drab when it is light.

Both things, however, are explained if it is true that *natural yellows differ less from the nearest colours of the spectrum than other natural colours do*. For, in the first place, the consequence will be that there will be many yellows in nature which we could not compound by our ordinary reds and greens, and we therefore find it difficult to imagine it could be compounded by any red and green. Secondly, there will be a greater relative range, so to speak, of yellowness, which we shall naturally subdivide according to degrees of purity and brightness. It may be added that, so far as brightness is concerned, the greater maximum brightness of yellow would act in the way last described.

But is it true that our yellows differ less from the nearest colours of the spectrum than our other colours do? I certainly think this is the result of the experiments, but I will only show that it is the case with the pigments employed by Mr. Strutt. He says, indeed, that "the most saturated yellow can be compounded of red and green." So it may in the spectrum (Maxwell, *Phil. Trans.*, 1860, p. 57—84); but Mr. Strutt's yellow cannot be compounded of Mr. Strutt's red and green. On the contrary, we see from his last "calculated" equation but one, that his yellow had to be diluted with nearly two-thirds as much of his white (the brighter of the two, I presume) before it could be matched by his red and green.

We can test the matter more closely. Denoting Mr. Strutt's red, green, and blue by r, g, b, and the "primary" red, green, and blue of the spectrum by R, G, B (Maxwell, *ubi supra*, p. 74), we may thus express the former in terms of the latter:—

$$\left.\begin{aligned} r &= LR + mG + n'B \\ g &= l'R + MG + nB \\ b &= lR + m'G + NB \end{aligned}\right\}(1);$$

where we know thus much about L, m, &c.; first that they are all positive, or else extremely small; secondly that, among them, the large letters must denote comparatively large quantities, and the small letters small quantities. Now, by the second "calculated" equation of the second batch, we find that Mr. Strutt's yellow is

$$\frac{1}{57}(128{\cdot}4\,r + 63{\cdot}5\,g - 49\,b);$$

and, if we substitute in this expression the expressions given above (1) for r, g, b, we shall have, for the coefficient of B,

$$\frac{1}{57}(128{\cdot}4\,n' + 63{\cdot}5\,n - 49\,N)$$

Now this must not be negative, or else must be very small in comparison with the coefficients of R and G (Maxwell, *ubi supra*, Tables VI. and IX.). Therefore N cannot be greater than $2{\cdot}6\,n' + 1{\cdot}3\,n$; that is to say, there is no more "primary" blue in Mr. Strutt's blue than about 2½ as much as in his red, *plus* 1⅓ as much as in his green. It is true that blue is rather a dark colour in pigments; but so it is in the spectrum; and N measures, not a quantity of colour simply, but its ratio to the "primary" blue of the spectrum. Either the yellow was very pure, or the red and green very impure; and, if Mr. Strutt provided himself with good representatives of natural colours, this proves my point.

C. J. Monro

Jan. 20

The Primary Colours

I have been greatly interested in reading Mr. Strutt's curious experiments on colour in the last number of Nature. I am glad to see that he is able to assume as proved the theory that green and not yellow is the middle primary. The true position of green is well illustrated in Mr. W. Benson's "Principles of the Science of Colour," both by argument and by diagrams. There is, however, one piece of evidence which seems to me conclusive as against yellow, but which I have not seen noticed.

When a solid body is gradually heated to incandescence, the light given out is first *red*, then *orange*, afterwards *yellow*, and finally *white*. If yellow were a primary, it would be impossible for it to appear in this series, which is formed upon the basis of the first primary, red, by successive additions of more and more rapid vibrations. Every colour in the series except the first must be a compound. If the heat is not sufficient to generate the most rapid vibrations which the eye can appreciate, white light is not given off at all, the series terminates with the yellow. The light of a glowing coal, without flame, in an ordinary fire, rarely passes beyond the yellow stage, and such light yields to the prism abundance of red and green rays, but scarcely a trace of blue or violet.

But, if red is the first primary, and green the second, which is the third? Shall blue still sit upon the throne on which Newton placed him, when his brother yellow is deposed? I think his position has become extremely precarious, and that he would be wise to abdicate with dignity before he is ignominiously turned out as a usurper.

If the kingdom of light is really divided into three principalities, is not violet the rightful heir to the third throne? Violet is said to be a mixture of blue and red. But how should red make its appearance at the wrong end of the spectrum? If it has no definite limit, but gradually thins out from its own place to the other extremity of the spectrum, then the whole of the other colours must be more or less affected by it, and red must be the only true primary among them. If it is said that the red in violet is clearly recognised by the eye, I think it may be answered that this is only because we have been taught to think of it as a compound, and that we might just as truly say that we can see yellow in green or orange in red.

Leicester, Jan. 21

Fredk. T. Mott

Utilisation of Sewage

While heartily thanking your reviewer for the very valuable suggestions which he has given me with regard to the second edition of my "Digest of Facts relating to the Treatment and Utilisation of Sewage," I wish to point out a slight oversight which he has made.

He says, "Mr. Menzies' name, however, has somehow or other slipped out of his pages (159, 169) where he treats of this improvement on the older plans for sewage."

The fact is that page 145 has somehow or other escaped the reviewer's critical eye. On that page the following sentence occurs:—"Some of these towns, then, it will be seen, are provided with sewers much upon the plan which Mr. Menzies has the credit of having first brought prominently forward; that is to say, with impervious pipes for the sewage properly so called, and brick drains for the surface and storm water, the former being laid deeply and the latter being superficial."

I should be very sorry to have it thought that I had forgotten "to emphasise the name of the Windsor Sanitarian."

W. H. Corfield

Ocean Currents

The differences of barometric pressure to which Mr. Keith Johnston refers (Nature, Jan. 19, p. 227) have a well-ascertained geographical existence, but his suggestion that they may originate or direct the Ocean Currents is clearly inadmissible. The high pressure over a large patch of the North Atlantic to the south or south-west of the Azores—and similarly in each of the other oceanic basins—is there permanently; and whatever disturbance might be produced by it was produced once for all when the high pressure was first formed. It would then displace a certain quantity of the water over which it rested, would thrust it out, and keep that particular part of the ocean at a slightly lower level than that over which the pressure of the air was not so great. But having done this, the adjacent bodies of water would be in hydrostatic equilibrium, and the high pressure could not continue to thrust water out towards the place of low pressure. My meaning may be at once illustrated by putting one end of an open glass tube into a basin of water, and partially exhausting the air inside it. The adjacent surface is thus exposed to a higher pressure than the surface inside the tube, and a certain portion of the water is immediately thrust from the place of greater to the place of less pressure; the column of water inside the tube is raised until the weight of the excess balances the difference of pressures. At that height it remains, and no further movement takes place, so long as the relative pressures remain the same. A fluctuation of the pressures will give rise to alternate ingoing or outgoing currents, but a continuous stream in one direction can only be produced by a continuous increase or decrease of one or the other pressure.

But independently of the equilibrium, which must, so far as the pressure is concerned, be established, the difference of level caused by these differences of pressure is extremely trifling. The barometric difference between the patch of high pressure in latitude 30° and the equator is about $\frac{3}{10}$ths of an inch, or equivalent to a column of water 4 inches in length. A difference in level of 4 inches in 1,800 miles can scarcely under any circumstances give rise to a current of twenty miles an hour.

J. K. LAUGHTON

Royal Naval College, Portsmouth, Jan. 23

IT is singular that diversity of opinion should still exist as to whether ocean currents are due to the impulse of the winds, or to difference of specific gravity. That ocean currents are not caused by difference of specific gravity between the waters of equatorial and polar regions can be proved, as the amount of force from this cause acting on the ocean to produce a current, can be readily calculated.

Assuming, which is not the case, that difference in saltness between the water of equatorial and polar regions does not in any wad tend to neutralise the effect resulting from difference of temperature, in other words, that the sea in polar regions is as salt as the sea in equatorial regions, it can be shown that the force resulting from the difference of temperature, tending to produce a current towards the poles, amounts to only $\frac{1}{1820000}$ of that of gravity. For example, the force impelling a cubic foot (64lb.) of sea water at the surface of the ocean towards the poles is scarcely equal to the weight of one-fourth of a grain.* A force so infinitesimal, acting on a fluid even so perfect as water, can produce absolutely no motion. M. Dubuat found by direct experiment that it requires a force four times greater than the above to produce even sensible motion.

Ocean currents are due alone to the impulse of the wind. In the latter half of my paper on the Cause of Ocean Currents, which will shortly appear in the *Philosophical Magazine*, I hope to be able to show that the objections to this theory are founded upon misconceptions regarding the way in which winds produce the great system of oceanic circulation.

JAMES CROLL

Dr. Frankland's Experiments

IN last week's NATURE Dr. Frankland describes some experiments, apparently under the impression that they were similar to one (No. 20) published by me in NATURE, No. 36, p. 200. The results which he obtained were in reality totally different from those which I obtained, although those who read Dr. Frankland's communication are lead to believe that the results were almost wholly similar. The inference to be drawn from what he has written is that we differ merely as regards the interpretation of the nature of what was seen.

Dr. Frankland says he made use of tubes of "hard Bohemian glass," and that, on examining them "when they came out of the digester, it was evident that the *interior walls of the glass tubes had been corroded* by the enclosed fluid." After a time the "liquid in all the tubes became more or less turbid, and, in some cases, a small quantity of a light flocculent precipitate subsided to the bottom." After five months two of the tubes, which exhibited "the greatest turbidity," were selected for examination, and the "flocculent sediment" in the tubes was more especially subjected to a careful microscopical examination. This scrutiny was conducted by Professors Frankland and Huxley and Mr. Busk. Dr. Frankland then says: "So far as the optical appearances presented by the sediment go, they may be appropriately described in the terms which Dr. Bastian applied to the matter found by him in a solution of like composition, and similarly treated."

Now, that any real similarity did exist, I feel most strongly inclined to doubt, because the solution examined was not similar in constitution to my own, and because no such "flocculent sediment," as that to which Dr. Frankland alludes, ever existed in my flask.

In the experiment of mine to which reference is made, the precise quantities of carbonate of ammonia and phosphate of soda employed are not known. In this first experiment the ingredients were not weighed, although, subsequently, solutions have been prepared for me of the strength which Dr. Frankland names.

Then, although my tube with its contained solution was exposed to the same temperature as that employed by Dr. Frankland, its internal walls were not in the least corroded, and no "flocculent sediment" appeared in the solution. And, in addition, two other tubes which were prepared for me by Dr. Frankland (which did contain solutions of the same strength as those which he employed) have not had their transparency in the least impaired, although they were submitted to precisely the same temperature; neither have they shown a trace of the "flocculent sediment" previously mentioned. Seeing, however, that in one experiment (May 11th), with a solution of the same strength, a tube of English glass was employed by Dr. Frankland's assistant, and that the internal walls of this tube were corroded; and seeing, moreover, that a "flocculent sediment" did form also in this particular tube of mine, I cannot help fancying that Dr. Frankland may be mistaken as to the nature of the glass employed in his experiments. If, as in this experiment of mine, it was really English glass instead of hard Bohemian, almost the whole of the small quantity of phosphoric acid originally in the solution would probably have been deposited in the form of an insoluble phosphate of lead, and thus the character of the solution would have been entirely changed.

In my previously published experiment the fluid was examined at the end of thirty days. "When this flask was received from Dr. Frankland, the fluid was somewhat whitish and clouded. During the last ten days a thin pellicle had been seen gradually accumulating on its surface, and in the latter four or five days this increased much in thickness, and gradually assumed a distinct mucoid appearance. The fluid itself was tolerably clear, though an apparent turbidity was given by the presence of a fine whitish deposit on the sides of the glass. When the flask was opened the reaction of the fluid was found to be *neutral*. Portions of the *pellicle* were at once transferred to a glass microscope slip," &c. (NATURE, No. 36, p. 200.) In portions of this pellicle were found the "*five spherical or ovoid spores*," upon the finding of which alone I laid any stress as indicative of the presence of living things. The presence of mere particles having a movement indistinguishable from Brownian movements, has never been adduced by me as evidence that living things had been evolved in a solution, although the representations of others would lead the public to believe that I have done so.

In the face of these differences, therefore, I was somewhat surprised at the intimations contained in Dr. Frankland's letter. He believes, and would lead your readers to believe also, that the microscopical appearances presented by the "flocculent sediment" and *débris* of corroded glass obtained from his tubes were similar to the microscopical appearances of a *pellicle* obtained by me from a tube in which there had been no corrosion. This, however, I am the less inclined to believe, because I also have had the opportunity of examining a flocculent sediment and *débris* of corroded glass from a tube previously referred to, which was opened on October 21, and in which also no living things were found. Microscopical specimens of the "pellicle" and of the "sediment" are now in my possession.

Perhaps I may venture to recommend Dr. Frankland to destroy the other two tubes which are corroded, as being worthless, and to hope that, in any future experiments, he will subsequently expose his fluids to a somewhat higher temperature, and also, before immersing his experimental tubes in any fluids, that he will thoroughly satisfy himself as to the transparency of such fluids to the actinic or chemical rays of light. We are informed that his tubes were "exposed to bright diffused daylight, and sometimes to sunlight," but any amount of exposure to light would be more or less useless if strong sulphuric acid and strong carbolic acid are as black to the chemical rays of light as nitrite of amyl and other fluids have been shown to be. Dr. Frankland makes no statement concerning this very important point.

H. CHARLTON BASTIAN

20, Queen Anne Street, W., Jan. 22

The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena

MY attention has been called to a rudely worded attack by a certain Mr. Bedford, Phil.D., on Professor Reynolds, of Owens College. This is not the first time Mr. Bedford has offended in this way. Prof. Reynolds has not seen Mr. Bedford's pamphlet.

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* *Philosophical Magazine* for October 1870, p. 249.

I have. A copy was sent to Messrs. Groombridge in support of certain claims on my views about the stars. Let me hasten to assure Prof. Reynolds that, as he surmises, the views expressed in this very scarce treatise bear not the remotest resemblance to his.

I read several weeks ago Prof. Reynolds' interesting paper, the views expressed in which are, in a general way, similar to those I advocated in a paper entitled "Strange Discoveries respecting the Aurora" in *Frazer's Magazine* for February 1870. As it was quite clear to me, however, that Prof. Reynolds' views had been formed quite independently, it seemed wholly unnecessary to comment on that resemblance. I could only rejoice that so competent an authority should have been led to conclusions agreeing in general so satisfactorily with those I had deduced; and also, be it noted, with the results of the observations made on the recent eclipse.

RICHARD A. PROCTOR

Browning's Spectroscope

A LETTER from Mr. Browning, in the number of NATURE for December 15th, has just come to my notice, and seems to require a word from me. I regret exceedingly that he should have supposed that I intended to imply that he had committed any impropriety in employing in his own automatic combination an arrangement of Mr. Rutherfurd's from a spectroscope which was not automatic. I did not "go out of my way" in making the allusion, but only stated what I supposed to be a fact, in order to show that the proposed arrangement of radial bars was good and practicable, having already been endorsed by most eminent authority.

At the time when the article was written, Mr. Browning had but recently published the account of his instrument, and, of course, I knew nothing about its earlier history.

On the other hand, a full description of Mr. Rutherfurd's arrangements with an illustrative figure had appeared in *Silliman's Journal* in March 1865, more than four years earlier. This article is dated December 10, 1864, and will be found in the Journal referred to: vol. xxxix., p. 129.

Possibly the tone of my allusion may have been unintentionally affected by the fact that I supposed that Mr. Browning had seen this article. In common with many other Americans, who have spoken to me about it, I thought it singular that, in describing his own instrument, he made no reference to Mr. Rutherfurd, and am very happy to find him blameless in the matter. At the same time, I think he has no ground of complaint against me for referring to the arrangement as "first devised by Mr. Rutherfurd, and since adopted by Mr. Browning;" although, if I were to write the sentence again with my present knowledge of the facts, I should put it quite differently.

Let me add also that, having seen the instrument to which Mr. Lockyer refers in his note, I cheerfully concede to him the priority in respect to the use of an elastic spring, and the half prism at the beginning of the train, as well as the idea of sending the light twice through the train by a right-angled prism at its extremity. As he has never published an account of his instrument, however, I suppose I can hardly be held blameworthy for re-inventing it, and publishing it myself. Without one unkindly feeling the words of the old poet still sometimes come to mind, "*Pereant qui ante nos nostra dixerint.*"

The magnetic record at Greenwich shows a well-marked disturbance of all the elements precisely simultaneous with the eruption observed on the sun's disc September 28th. The declination was affected to the extent of five minutes of arc, and the disturbance was compounded of two waves, following each other, and partly superposed, probably corresponding to the ejection of the two masses of protuberance-matter which are shown in the figures.

C. A. YOUNG

London, Jan. 21, 1871

St. Mary's Hospital

I SEE in this week's NATURE the announcement that Dr. Wood has been appointed Lecturer on Chemistry at St. Mary's Hospital Medical School. This is an entire mistake; no appointment has yet been made, since Dr. Russell will continue to hold the post until the end of the Winter Session. The vacancy has not therefore actually occurred yet, although it will be declared shortly, and a fresh appointment made in due course.

W. B. CHEADLE,
Dean of St. Mary's Hospital Medical School

Jan. 20

[We were misled in making the announcement referred to above by our contemporary the *British Medical Journal*.—ED.]

IMPROVEMENT OF GEOMETRICAL TEACHING

A CONFERENCE was held at University College, London, on Tuesday, the 17th inst., to take this subject into consideration, and to form an Association for the improvement of geometrical teaching throughout the United Kingdom.

Previous to the meeting a large number of head and mathematical masters and others interested in the subject had given in their adhesion to the principles upon which it was proposed to form the Association. These included representatives of the following important schools:—Winchester, Eton, Harrow, Rugby, Charterhouse, Christ's Hospital, Marlborough, Wellington, Clifton, Uppingham, Sherborne, Birmingham, Dulwich, University College School, London, Repton, Durham, Manchester, King William's College, Isle of Man; Tiverton, Taunton, Leeds, Huddersfield, Nottingham, Yarmouth, Windermere, Mill-hill School, Middlesex, Middle Class School, Cowper Street, Middle Class School, Bedford, the majority of whom were present at the Conference. The movement was further supported by Dr. Hirst, F.R.S., of London University, Mr. W. Spottiswoode, F.R.S., president of the London Mathematical Society, Mr. C. W. Merrifield, F.R.S., Principal of the Royal School of Naval Architecture, South Kensington, and others.

Dr. Hirst, the president, took the chair, and resolutions were passed bearing upon the organisation and future working of the Association. It was proposed to invite the mathematicians of the country to prepare syllabuses of elementary geometry, embodying their views of the principles which should be adopted in any new text-book which is to supersede Euclid. Further particulars may be obtained by application to Mr. R. Levett, honorary secretary, King Edward's School, Birmingham.

A HINT TO ELECTRICIANS

MR. MANCE'S method for measuring the internal resistance of a single galvanic element or battery, communicated to the Royal Society at its meeting of last week, and the modifications of Wheatstone's bridge suggested by myself for finding the resistance of a galvanometer coil from the deflection of its own needle, supply desiderata in respect to easy and rapid measurement, which have been long *felt* by telegraph electricians and *needed* by other scientific investigators and by teachers of science. Year after year the latter, in their arrangement of batteries, electrodes, and galvanometers, have darkly and wastefully followed the method which from workmen we learn to call rule of thumb; while the former, with admirable scientific art, measure every element with which they are concerned, in absolute measure. How many physical professors are there in Europe or America who could tell (in millions of centimetres per second) the resistance of any one of the galvanometers, induction coils, or galvanic elements which they are daily using? How many of them, in ordering an electro-magnet, require of the maker that the specific resistance of the copper shall not exceed 16,000 (gramme centimetre-seconds)? How many times have eight Grove cells been set up to produce a degree of electro-magnetic effect which four would have given, had the professor exacted of the instrument-maker the fulfilment of a simple and inexpensive scientific condition, as submarine telegraph companies have done in their specifications of cables? If every possessor of an electro-magnet were to cut a metre off its coil, weigh the piece, measure its resistance, and send the result to NATURE, and if every maker of Ruhmkorff coils would do the like for every coil of copper wire designed for his instruments, a startling average might be shown. And what of the items? I venture to say that (provided the instruments of the great makers are not excluded) specific resistance above 30,000 would not be a singular case. I could tell something of galvanometers of 1869, comparable only to submarine cables of 1857. I refrain:—but let makers of galvanometers, Ruhmkorff coils, and electro-magnets beware; surely NATURE will find them out if they do not reform before 1872. W. THOMSON

THE GAUSSIAN CONSTANTS OF TERRESTRIAL MAGNETISM

I THINK you will be doing good service to the cause of Natural Science by giving insertion in your valuable pages to the following translation of a notice which appeared in No. 1,825 of the *Astronomische Nachrichten* (Vol. 77, p. xi), on the subject of Prof. Petersen's recomputation of the Gaussian Constants of Terrestrial Magnetism, in aid of which the British Association at their last meeting voted a grant of money. It has been communicated to me by Prof. Erman of Berlin, who, in reference to the grant in question, writes as follows:—"This new act of British generosity would in other times have scarcely needed a special mention, being equalled by so many former ones of the same kind; but in the present moment, when the raging war makes petty jealousies spring up between our two befriended [friendly] nations, it is a most sacred duty to publish the fact of two Prussians having found in England a most generous and most wanted help for their scientific endeavours."

Mr. Petersen's calculations are progressing in a very desirable manner, and he hopes fully to bring them to their end (D.V.). J. F. W. HERSCHEL

Collingwood, Jan. 21

TRANSLATION

"We learn by a communication from Prof. Erman that M. Petersen, of Kiel, has undertaken to extend his great work on Terrestrial Magnetism, so as to afford for the whole earth, and for the epoch 1829, a *Fundamental Determination of the Potential Constants*, which, according to laws yet unknown, are subject to secular variation. From the knowledge of these fundamental values so obtained by the researches of Erman and Petersen, will then come to be securely connected, as a second step in advance, the determination of the laws of secular change. Since, however, the material obstacles to so laborious a work, with whatever personal devotion, would have proved insurmountable without public aid, it becomes our duty most gratefully to announce that such aid has been granted from the same quarter which afforded it to the earliest portion of this undertaking. The British Association for the Advancement of Science, at its last annual meeting, has appointed a committee, consisting of Sir J. Herschel and Prof. A. Erman, for the purpose of engaging M. H. T. R. Petersen to prosecute the continuation of his computations of the constants in question for 1829, so as to embrace all observations not included in the previous calculations, and to this end has placed a sum of 50*l.* at their disposal.

"In pursuance of this object Prof. Erman addresses to the readers of this notice his request for the communication of citations of, or references to, works and treatises or essays in which may be found recorded measured values for any station of the globe, of the magnetic *declination*, *inclination*, and *intensity* during any portion of the last ten years, as also any researches on the annual variations of these elements at determinate stations. Of course it is not meant to call for even an approximately complete catalogue of works of this kind, to furnish which would of itself require no small amount of labour. But many astronomers [and others] must have access to a variety of journals, accounts of travels, records of measures and observations, &c., which may not have come under the notice of Messrs. Erman and Petersen, notices of which, communicated to the Editor of the *Astronomische Nachrichten* * in the form of a letter, with a postcriptum or memorandum such as:—Magnetic Observations for 18 . . are to be found in . . . Volume . . . page . . are requested."

ACCOUNT OF THE AUGUSTA ECLIPSE EXPEDITION

IN consequence of the unfortunate wreck of the *Psyche* on a sunken rock on the coast of Sicily, about nine miles north of Catania, the arrangements of the Sicilian Expedition were considerably modified. Catania was made the headquarters of the expedition, and the garden of the Benedictine Monastery was given up by the authorities of the city to the English and American observers. It was finally arranged that Prof. Roscoe should take charge of the Etna Expedition, and I was asked by Mr. Lockyer to take charge of the Expedition to Augusta. Mr. Brett, Mr. Burton, Mr. Clifford, Mr. Ranyard, Mr. Samuelson, and myself formed the party.

It was also arranged with Mr. Ranyard at Catania, that on the morning of the 22nd, he and another of our party should drive some miles up from Augusta in the direction of the hills of Carlentini, to observe the Eclipse. At Augusta we were to live in camp, and Colonel Porter, with a body of sappers, had been landed there by the *Pysche* on her way to Naples.

Mr. Brett and Mr. Ranyard went first to Augusta to make arrangements with Colonel Porter for our encampment and observatory, and they met with every assistance from the Syndic of the City of Augusta, and were very kindly received by the Italian astronomers, among whom were Prof. Cacciatore, Prof. Donati, Father Secchi, and Father Denza, who were stationed inside the fort. Our encampment, and a wooden observatory sixty feet long, were pitched on the southern slopes of the glacis of the fort, with a full view of the sea to the east.

I cannot speak too highly of the way in which Colonel Porter exerted himself to make all arrangements satisfactory and complete, and even to introduce elements of comfort into our camp life; and the energetic way in which his men carried out his instructions is beyond all praise. Up to Monday the 19th, the terraces of the Monastery at Catania were made the general practising ground, and those who were to observe for polarisation, except Mr. Ranyard, who was at Augusta, tested and compared their instruments for rapidity of correct observation, and for delicacy.

For my own telescope I had two eye-pieces, one with plates of double-rotating quartz, and the other with a Savart polarimeter. When the polarisation was not very strong, I found the polarimeter more delicate than the biquartz for detecting the plane of polarisation, and with it I was able to measure the amount of polarisation readily. On observing the same points with Mr. Griffiths, who also used a Savart polarimeter, we found that in from ten to fifteen seconds we could determine the plane and amount of polarisation, and in some cases we found that our readings for both were absolutely identical.

At about 6.30 on Monday evening, and again soon after 7 o'clock, when Mr. Clifford and I were on the sea on our way to Augusta, we saw a brilliant display of the zodiacal light, consisting of brilliant pink streamers, stretching up perpendicularly to the horizon, the planet Jupiter being just on the most brilliant streamers. Towards the north and round the horizon there were also streamers and a faint hazy light, and the sky became covered with a pinkish mauve colour. One of these displays was also seen by the rest of our party at Augusta.

As the evening grew darker, there was strong phosphorescence on the sea. The drops scattered by the oar as it struck the water glowed with phosphorescent light, and the forms of the eddies, caused by the bending of the oar, were distinct and brilliantly illuminated.

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* Prof. Dr. C. A. F. Peters, Direktor der Sternwärte in Altona.

At Augusta, through the kindness of Prof. Cacciatore and Father Denza, I was able to obtain the latitude and longitude, as well as the local times of the different phases of the Eclipse:—

Latitude of Fort Augusta	37° 14′ 0″·6 N.
Longitude „ „	1^{h} 0′ 52″·2 E. of Greenwich.
Local time of beginning of totality	2^{h} 1′ 56″·7
„ middle „	2^{h} 2′ 52″·1
„ end „	2^{h} 3′ 47″·5
Making the duration of totality	1′ 50″·8

The barometer fell from the morning of the 20th to the morning of the 22nd, then rose a little, and again began to fall about 12 o'clock, and was lowest about the time of totality. On the evening of the 21st the Italian astronomers reported to us the bad state of the weather throughout Italy and Sicily, the wind being westerly, and that a sirocco was expected everywhere. From about 2 o'clock in the night there was a heavy storm of wind and rain, with thunder and lightning, and our tents were in danger of being blown away. By 6 o'clock the rain had ceased, and the wind moderated, but there were still frequent flashes of lightning on the eastern horizon; in half an hour thick clouds had again covered the sky, and we had rain. The wind again became violent, and swept away the clouds, but the weather did not look promising.

According to previous arrangement, Mr. Ranyard started in a carriage to go up to the hills, and Mr. Samuelson accompanied him, and they took two sappers with them.

When the moon had entered about one-third of her diameter Mr. Brett, with his 8·5 inch reflector, observed the corona round the limb of the sun as a hazy light most brilliant nearest the sun's limb, and the limb of the moon could be traced on this corona for about 2′ from the cusps. On these points Mr. Burton confirmed Mr. Brett's observations. Soon after a sudden chill was felt, and there was a sudden change in the light. About three minutes before totality there were brilliant and very remarkable patches of red and yellow light on the cloud to the right of and below the sun. Mr. Burton describes them as bows, apparently concentric with the sun. During the morning Mr. Burton had been able to indicate the positions of some of the most remarkable prominences, but the stormy wind prevented him from mapping them accurately. Father Secchi had also kindly sent us the positions of those which he had observed.

Just before totality, Mr. Burton saw and made a diagram of a prominence at the lower horn, and saw several lines in the spectrum of the chromosphere between D and E. Using his large telescope Mr. Brett was able to make a sketch of the corona during the totality.

On account of the cloud, Mr. Burton was able to make only one of the four observations he had hoped to make on the Corona with his five-inch equatorial and spectroscope. At the beginning of totality, placing his slit tangential and very near to the east limb, but not on a prominence, a bright line was distinctly seen in the spectrum, very near E, and a little less refrangible. The line was less defined than the hydrogen lines of the prominence. No dark lines were seen on the Corona.

At the end of totality, Mr. Burton had a momentary glimpse of the Corona, but had no time to get the telescope on it before the totality was over. With regard to the amount of light, he says that it was sufficient to see a pencil diagram at a distance of two feet from the eye. I can confirm him in the view that the darkness was not intense, and have no doubt that the diffusion of light by the cloud gave us more light than we should otherwise have had. Venus and some stars were seen.

Colonel Porter had kindly volunteered to make a sketch of the Corona, but the cloud prevented him from obtaining any satisfactory result.

I did not see the Corona at the beginning of totality with my telescope. As the band of sunlight became exceedingly thin, and at the instant of its disappearance broke up into sections, I could not decide whether the lunar mountains had pierced the rim of light, or whether the dense cloud coming over the moon had cut out certain portions of the rim before obscuring the whole.

After this, I could detect nothing of the disc of the moon for a full minute, then the cloud became thinner, and I found that by slowly moving the telescope I had kept the moon in the centre of the field. At the top and bottom, the limb was visible, but no light was seen outside it at these points. I saw light of the Corona near the point of beginning of totality covering some 20° of the limb, and also a trace of light near the point of emergence. I could not perceive any colour on these portions of the Corona, nor could I detect any difference of colour on the two plates of my bi-quartz, the line of division of which was at right angles to the sun's path, *i.e.*, inclined at 15° to the vertical. The moon was again observed, and again I detected light near the point of emergence; and placed the line of division of my bi-quartz radial to the moon, having the light in the centre of the field, but I could detect no trace of colour on the two parts of the crystal, showing that the bi-quartz was not sufficiently delicate to detect the polarisation under such unfavourable circumstances. The rim then became continuous, and the totality was over.

Mr. Clifford observed polarisation on the cloud to the right and left and over the moon, in a horizontal plane through the moon's centre, and found the plane of polarisation to be inclined at from 15° to 20° to the vertical towards the west. At his last observation, which was on the moon, when it could be seen near the end of totality, he determined the plane of polarisation to be vertical.

The comparisons made at Catania, as well as a comparison of my observations with Mr. Clifford's, seem to show that bands, rather than a difference of shades of colour, should be employed to detect delicate polarisation.

Mr. Ranyard had a very clear view at Villasmunda, although it was raining during the totality, and made *three* observations, two of which he described to me as agreeing with what should be observed in the case of radial polarisation.

Mr. Samuelson and the two sappers made independent rough drawings of what they saw, and their drawings agree well as to the Corona and the positions of the rays. Mr. Samuelson also used a Nicol's prism, with Savart's bands, to determine the polarisation on the sky at three points, and at two of these points found the plane of polarisation vertical.

I have not yet seen the details of Mr. Ranyard's or of Mr. Samuelson's reports.

Although the other successful observers of the Eclipse in Sicily were not attached to the Augusta expedition, of which Mr. Lockyer had put me in charge, I may add that at Syracuse the weather was favourable, and Mr. Griffiths was able to determine the plane and amount of polarisation at different points of the Corona; also, that Messrs. Brothers and Fryer were able to take some good photographs of the Corona, one of which is very remarkable for its clear definition of the Corona and of the rays extending out to a distance of two diameters from the moon's limb. This photograph and a careful sketch of the Corona by Mr. Watson, one of the American observers, show a very remarkable agreement, and prove the existence of the remarkable dark cusps on the Corona.

At Augusta, two bright lines were seen in the spectrum of the Corona by Father Denza, one of the Italian astronomers, but no dark lines have been seen.

We experienced every kindness from the Italian astronomers at Augusta and from the authorities; and, on the day after the Eclipse, we were invited by the Syndic of the City to meet the Italian Astronomers at a grand public dinner given by the City in honour of our visit.

King's College, Jan. 16 W. G. ADAMS

FLOWER'S OSTEOLOGY OF THE MAMMALIA *

PROF. FLOWER'S "Introduction to the Osteology of the Mammalia" is a thoroughly satisfactory addition to English anatomical literature. It supplies a much-felt want, and combines the rarely united qualities of completeness with brevity; and, while thoroughly scientific, is remarkable for its clearness and simplicity of expression.

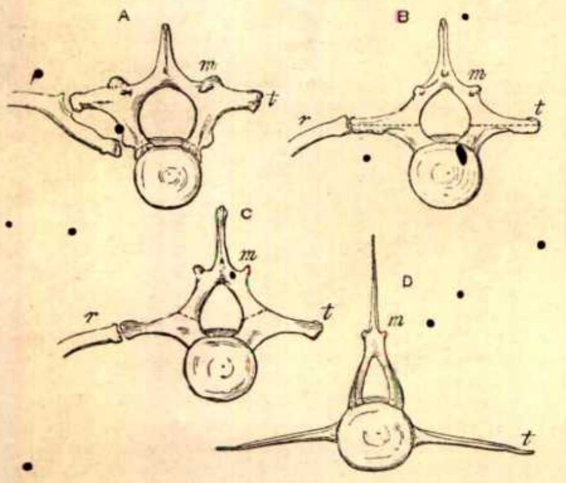

FIG. 1.—Anterior surface of vertebræ of Dolphin (*Globiocephalus melas*), $\frac{1}{4}$. A fifth thoracic; B seventh thoracic; C eighth thoracic; D first lumbar: *r* rib; *m* metapophysis; *t* transverse process. The dotted lines indicate the position of the neuro-central suture.

Of convenient size for the pocket of the student, it consists of three hundred and thirty-seven pages of excellent letter-press, and is illustrated by one hundred and twenty-six woodcuts, nearly all original. These are exceedingly well drawn, and make the book attractive in appearance (though it is to be regretted that they have somewhat suffered in the printing), while the subjects chosen have evidently been selected with great care as regards their utility in illustrating the text.

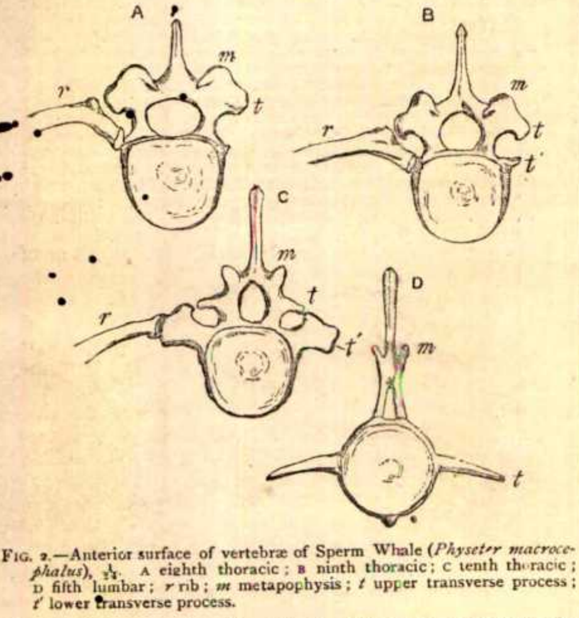

FIG. 2.—Anterior surface of vertebræ of Sperm Whale (*Physeter macrocephalus*), $\frac{1}{24}$. A eighth thoracic; B ninth thoracic; C tenth thoracic; D fifth lumbar; *r* rib; *m* metapophysis; *t* upper transverse process; *t'* lower transverse process.

The first chapter contains a very short account of the classification of the Mammalia, accompanied by an interesting diagram "intended to exhibit the relationships which appear to exist between the different groups of the Mammalia." The small size of this diagram hardly affords space enough to express fully the degrees of affinity between the different groups. It is owing to this, perhaps, that the Carnivora are separated from the Ungulata by a less interval than that which divides them from the Insectivora, and that the Hominina are but very slightly more approximated to the Simiina than are the latter to the Lemurina, although the structural difference between the last-named groups and all the higher primates are so great that Professor Flower himself hesitates "whether they should be associated with the monkeys, or should constitute a distinct order by themselves." Nevertheless, the diagram is very instructive, and well expresses the more important relationships existing between the groups as far as their affinities have been demonstrated, or shown to be probable by the present state of zootomical science.

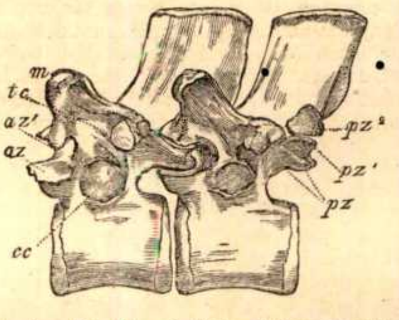

FIG. 3.—Side view of twelfth and thirteenth thoracic vertebræ of Great Anteater (*Myrmecophaga jubata*), $\frac{2}{3}$ *m* metapophysis; *tc* facet for articulation of tubercle of rib; *cc* ditto for capitulum of rib: *az* anterior zygapophysis: *az*¹ additional anterior articular facet; *pz* posterior zygapophysis; *pz*¹ and *pz*² additional posterior articular facets.

Thus the distinctiveness, yet close affinity, between the ordinary Carnivora and the seals is made evident, as also the remoteness of the Sirenia from the Cetacea, and the approximation of the former to the Ungulata.

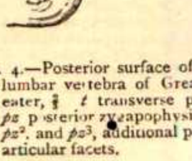

FIG. 4.—Posterior surface of second lumbar vertebra of Great Anteater, $\frac{2}{3}$ *t* transverse process; *pz* posterior zygapophysis; *pz*¹, *pz*², and *pz*³, additional posterior articular facets.

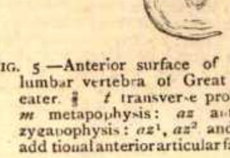

FIG. 5.—Anterior surface of third lumbar vertebra of Great Anteater. $\frac{2}{3}$ *t* transverse process; *m* metapophysis: *az* anterior zygapophysis: *az*¹, *az*² and *az*³, additional anterior articular facets.

The complex relationships of the subordinate groups of hoofed beasts are also well exhibited, and though, perhaps, some objection might be made to the position of the anomalous little group, Hyracoidea, it would be difficult to put it in any other spot not also open to criticism.

A laudable desire not to increase too much the bulk of his volume has, doubtless, induced the author to make his chapter on Classification so very brief. It is, nevertheless, to be hoped that, in the next edition, it may be made at least as long again; as, in its present condition, the student can hardly find in it all the information necessary for the comprehension of the other parts of the work.

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* "An Introduction to the Osteology of the Mammalia." By W. H. Flower, F.R.S., Hunterian Professor of Comparative Anatomy and Physiology. (London: Macmillan and Co.)

Thus, in describing the skull, the family names *Cebidæ* and *Hapalidæ* are used, as also *Ursidæ*, *Procyonidæ* and *Mustelidæ*, though nothing is said as to these groups either in the text of the first chapter, or in its explanatory diagram. The addition of three more pages would do away with this imperfection.

After the introductory chapter on Classification we have the skeleton as a whole, the vertebral column with its several regions, the sternum, ribs, skull, shoulder girdle, arm and hand, pelvic girdle, leg and foot, successively described in eighteen different chapters. The maximum degree of complication in the Mammalian spine (*i.e.*, in that of the great ant-eater) is for the first time clearly and accurately described.

In each case the part of the skeleton treated of is first described in its normal and typical condition, and afterwards each order of mammals is passed in review, and the leading modifications which such part presents in each ordinal group are stated.

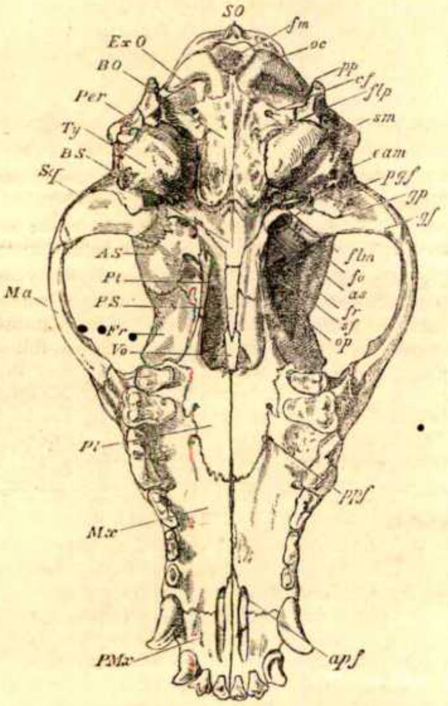

FIG. 6.—Under surface of the cranium o a Dog. ½. *SO* supraoccipital; *ExO* exoccipital; *BO* basioccipital; *Per* mastoid portion ot periotic; *Ty* tympanic bulla; *BS* basisphenoid; *Sq* zygomatic process of squamosal; *Ma* malar; *AS* alisphenoid; *Pt* pterygoid; *PS* presphenoid; *Fr* frontal; *Vo* vomer; *Pl* palatine; *Mx* maxilla; *PMx* premaxilla; *fm* foramen magnum; *oc* occipital condyle: *pp* paroccipital process; *cf* condylar foramen; *flp* foramen lacerum posterius; *sm* stylo mastoid foramen; *eam* external auditory meatus; *pgf* postglenoid foramen; *gp* postglenoid process; *gf* glenoid fossa; *flm* foramen lacerum medium; *fo* foramen ovale; *as* posterior opening of alisphenoid canal; *fr* foramen rotundum and anterior opening of alisphenoid canal; *sf* sphenoidal fissure or foramen lacerum anterius; *op* optic foramen; *ppf* posterior palatine foramen; *apf* anterior palatine foramen.

Thus, with regard to the skull, we have first a chapter on the skull of the dog as a type—an admirable description, and a model for writers on kindred subjects from its completeness, its clearness, its thoroughness, and its simplicity. Next, we have a chapter on the skull as it exists in the orders Primates, Carnivora, Insectivora, Chiroptera, and Rodentia. After this follows a chapter on the skull in the Ungulata, Hyracoidea, and Proboscidia; then one on the same part in the Cetacea and Sirenia, and finally one on the skull in the Edentata, Marsupialia and Monotremata. This last chapter may hereafter be judiciously expanded. The skulls of the Echidna and Ornithorhynchus alone might well take up half the space which is here allotted to these three orders.

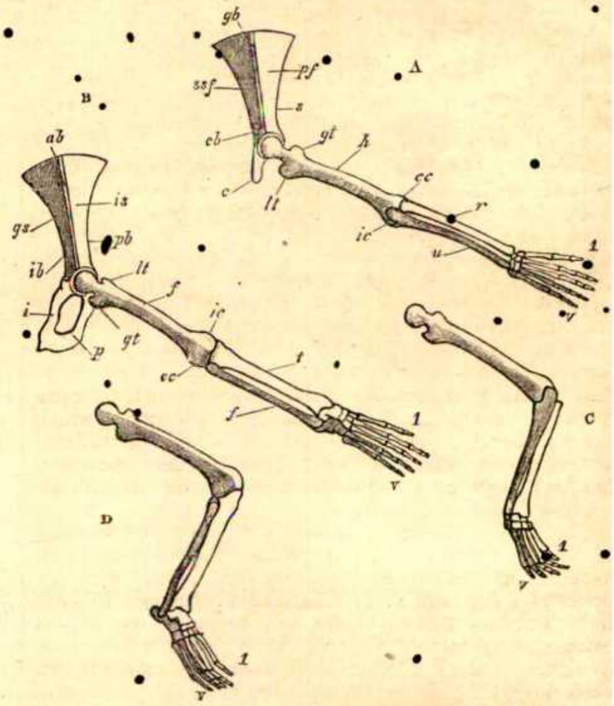

FIG. 7.—Diagrammatic representation of the positions of the limbs of Mammalia. The preaxial border is left light, the postaxial border shaded, in all the figures. Limbs of the right side are represented in all cases. A dorsal aspect of the anterior extremity in its primitive unmodified position; *gb* glenoid border of the scapula; *s* spine; *cb* coracoid border; *ssf* subscapular fossa; *pf* postscapular (infraspinal) fossa; *c* coracoid; *h* humerus; *gt* greater, radial, or preaxial tuberosity; *lt* lesser, ulnar, or postaxial tuberosity; *ec* external (in the modified position), radial, or preaxial condyle; *ic* internal, ulnar, or postaxial condyle; *r* radius; *u* ulna; I pollex; V fifth digit. B dorsal aspect of the posterior extremity in the same position; *ab* acetabular border of the ilium; *pb* pubic border; *ib* ischial border; *gs* gluteal surface; *is* iliac surface; *i* ischium; *p* pubis; *f* femur; *lt* lesser, tibial, or preaxial trochanter; *gt* greater, fibular, or postaxial trochanter; *ic* internal (in the modified position), tibial, or preaxial condyle; *ec* external fibular, or postaxial condyle; *t* tibia; *f* fibula; I hallux; V fifth digit; C the anterior extremity, with the humerus in the same position, but the elbow and wrist joints bent; D the posterior extremity in the same position.

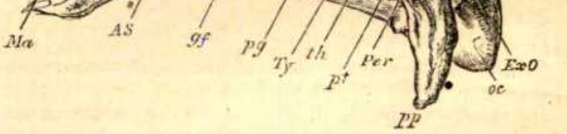

FIG. 8.—Side view of the posterior part of the skull of a Horse, ¼. *Fr* frontal (the line points to the postorbital process); *Sq* squamosal; *Pa* parietal; *SO* supraoccipital; *ExO* exoccipital; *oc* occipital condyle; *pp* paroccipital process; *Per* mastoid portion of periotic; *pt* post-tympanic process of squamosal; *th* tympanohyal; *Ty* tympanic; *pg* postglenoid process of squamosal; *As* alisphenoid (the line points to the plate of the bone which bridges over the alisphenoid canal); *Ma* malar.

The chapters on the hand and foot (Manus and Pes) are excessively instructive and interesting; the better to understand the conditions presented, the author steps beyond the limits of the class Mammalia and exhibits the condition of the manus in a water tortoise (*Chelydra serpentina*). At the end of the work is a chapter on "The Correspondence between the Bones of the Anterior and Posterior Extremity and the Modifications of the Positions of the Limbs," and this is the only speculative and theoretical portion of the book. All the rest is a plain and clear statement of observed facts.

Some may be disposed to regret that there is not more "theory" in the work, but the reviewer believes that, considering the scope and object of the treatise, the introduction of theoretical views would be a blemish rather than a gain.

The work is addressed to students who are in earnest and really want to learn "Comparative Anatomy." Do such need to be attracted or encouraged by brilliant or startling theories, or will, on the other hand, they follow teaching which is simply the clear expression of actual facts? To this question it may be replied *solvitur ambulando*. Last year Professor Flower delivered in the theatre of the Royal College of Surgeons the very lectures which are now being reviewed, and the attendance of students was most remarkable, not only as to numbers who came but also as to their perseverance and constancy in attending.

The book is eminently a student's book. Obscurities of expression and unnecessary technicalities are carefully avoided; and any youth who provides himself with the bones of a dog, and reads it patiently, referring with care to his dog-bones all the while, may become a respectable osteologist. While if such a youth has access to a museum like that of the College of Surgeons, he may, with this work in his hand, lay the foundation of a really good knowledge of comparative anatomy; for every anthropotomist and teacher of human anatomy will admit how readily a knowledge of "soft parts" is acquired by one who thoroughly "knows his bones."

An excellent exercise for a student would be to take the skull of a rabbit and a pig, and write out a detailed description of each, modelled on the author's description of that of the dog.

Although the work is of such moderate bulk, yet owing to its convenient arrangement, and the thorough knowledge of his subject which the author professes, it is possible, by referring to it, to get an answer to almost any ordinary question respecting the bony structure of any mammal.

Professor Flower's new human bone, the *tympano-hyal* (first described by him at the Liverpool meeting of the British Association), is shown to exist in other mammals, being very large, *e.g.* in the horse and the sheep.

Amongst so great a mass of facts, it is impossible but that there should not be some inaccuracies, without the slightest discredit thereby accruing to the author; for the laws of animal structure being but empirical, a "black swan" is at any moment liable to turn up and falsify the simplest and most useful generalisation. Thus it may turn out that the generalisation, "the *Simiina* are remarkable in never having an ossified stylohyal," is not strictly accurate, but the author having examined hundreds of skulls and never having found one so furnished, was amply justified in making the assertion. Any attempt to depreciate a work by carping criticism upon details of that kind can result only in discredit to the critic himself.

In conclusion, the reviewer is of opinion that Prof. Flower's "Osteology" should be warmly recommended to all students of comparative anatomy; while even to many advanced zoologists it will prove a work not only of much interest, but one conveying substantial, important, and trustworthy information.

F. R. S.

NOTES

So far as the delegates are concerned, the amalgamation of the Ethnological and Anthropological Societies is complete, under the name of the Anthropological Institute of Great Britain and Ireland, and the separate societies have ceased to exist. All property, effects, debts, and liabilities are to be taken over by the new society. The new officials taken from the two societies are: President, Sir John Lubbock (E.), Vice-Presidents, Prof. Huxley (E.), Tylor (E.), Prof. Busk (E.), Dr. Charnock (A.), Dr. Beddoe (A.), G. Harris (A.). Treasurer, Flower (E.). Council.—Blackmore (E), Bohn (E.), Archibald Campbell (E.), Hyde Clarke (E., A.), Boyd Dawkins (E.), Dunn (E.), David Forbes (E.), Col. Lane Fox (E., A.), T. M'K. Hughes (E.), M'Lennan (E.), Pusey (E., A.), Braybrooke (A.), W. C. Dendy (A.), Dr. King (A.), F. G. H. Price (A.), Dr. Harcourt (A.), Des Ruffieres (A.). Director, C. Staniland Wake (A.); Assistant-editor of the Journal, F. W. Rudler (E.). The last meeting of the Ethnological was held on Tuesday, and the last meeting of the Anthropological will be held on Tuesday next, when the old arrangement will cease, and other meetings will be fixed by the new council.

A DETERMINATION to take advantage of any opportunity for making scientific inquiries, is the principal feature of the present crisis among French scientific men. It is carried so far that the French Institute has appointed a committee to inquire into the effects of the shelling as well on the buildings as on the inhabitants. The specialities presented by the wounds have been reviewed in a very able essay, written by Baron Larey, in the *Revue des Cours publiques*, published by Baillière, but which has not appeared regularly. Some other essays on the same subject have been written by different medical men in the same periodical, and in some political papers.

THE increase of the mortality is a feature of the present crisis in Paris. The winter number of deaths is generally 1,000 or 1,200 a week; but in the last few weeks it has amounted to 3,000. This is not owing to any particular epidemic, although the small-pox has destroyed many victims. The larger amount of fatal cases is owing to the exposure, the want of proper food suited for infants and invalids, and the scarcity of fuel. Moral causes have also seriously affected invalids and old persons to a very large extent. Affections of the lungs are prevalent in that portion of the population.

THE large conservatory for *Orchidaceæ* in the *Jardin des Plantes* at Paris has been destroyed by a Prussian shell. The glass-work was broken, and the plants, which are so delicate, were lost without any hope of recovery. The losses are very serious, as the collection had many valuable specimens obtained from the occupation of Mexico, and from the exertions of the French Scientific Commission, which had been sent to America to explore the Empire of the unfortunate Maximilian. M. Chevreuil, the Director of the Museum, has addressed to the Academy of Sciences the following protest:—"The garden of medicinal plants, founded in Paris by an edict of King Louis XIII., dated January 3, 1626, became a Museum of Natural History on the 23rd May, 1794. It was bombarded in the reign of William I., King of Prussia, Count Bismarck being Chancellor, by the Prussian army, on the night of the 8th-9th January, 1871. Until then it had been respected by all parties, and by all national and foreign authorities. Paris, January 9, 1871." The Academy has determined that the protest of M. Chevreuil shall be printed at the head of its reports, and the Committee of Professors of the Museum have decided that a marble monument, with an inscription of the protest, shall be placed in one of the galleries of the building, surrounded with projectiles thrown from the enemy's batteries.

THE Polytechnic School has been opened at Bordeaux. M. Gambetta delivered an inaugural address. The director is M. Serrey, Member of the French Academy; but the larger number of the professors are now besieged in Paris, serving in the ranks of the National Guard. They mostly belong to the artillery. Among them are M. Janin, Professor of Physics; M. Laussedat, Professor of Topography; M. Moutard, Professor of Mathematics; M. Manheim, Professor of Descriptive Geometry.

ARRANGEMENTS for instruction in Practical Chemistry have now been completed at the London Institution, Finsbury Circus, by the opening of the Chemical Laboratory, under the direction of Dr. Henry E. Armstrong, for the reception of students requiring instruction in Analytical Chemistry and the methods of Original Investigation. The evening class for Elementary Chemical Analysis will commence work on Feb. 13, and will meet three times a week, on Monday, Wednesday, and Friday, from 6 to 8 P.M. from February to May. All students must be nominated by a proprietor, and the sons of proprietors have an advantage in the fees charged.

THE Young Men's Christian Association of New York announces a course of popular scientific lectures by Dr. Doremus, on the "Triumphs of Modern Science." "The subject is an interesting one, but we regret," says the *New York Technologist*, "to see that the Association has placed the price of tickets at 1½ dols. per lecture, or 5 dols. for the course of four. At this rate, many young men who would otherwise attend and be interested, will prefer to pay a less amount and visit Booth's, or even the opera-house of Jim Fisk, jun. We presume that the managers will claim that the enormous expenses incurred for chemical and physical illustrations render this high price necessary. This may be true in their case, but it is a pity that they and their lecturer should forget that neither Science itself nor its popularisation depends upon magnificent spectacular effects. If this were the case, then Tweed, Sweeny, and Hall, with their free entertainments given every 4th of July at the expense of the city, can beat any scientific lecturer in the country, and even Fisk, with the gorgeous scenery of his Twelve Temptations, will prove more attractive than Huxley or Tyndall. A good experiment, so extensive as to be visible in all parts of the audience hall, and so pertinent that it gives a perfect illustration of the point under discussion, is a thing that we admire above all things. But the burning of a pound of potassium, merely for the sake of making a blaze and a smoke; the burning of diamonds, which, when performed on the lecture table of a popular audience hall, proves nothing, and is the sheerest mountebankery; the dragging in of an aquarium, merely for the sake of advertising the maker's name; and in short, everything that does not aid the hearer in obtaining clear ideas in regard to the matter in hand, ought to be rigorously excluded. The simplest experiment, performed with the cheapest apparatus, becomes beautiful and interesting beyond any display of fireworks, when it clearly illustrates some great physical truth, while the most gorgeous display will, of itself alone, fail to excite that intellectual interest that is so far superior to mere physical emotion. In the name of Science, therefore, and of that intellectual progress, of which our Young Men's Christian Association should be the promoters and advocates, we protest against a system which degrades the scientific lecture to a level with the performances of Houdin or Signor Blitz." Sensible language this, and deserving of note in this country as well as America.

WE learn from the *American Technologist* that the scientific lectures delivered and to be delivered this session before the American Institute are as follows:—Tuesday evening, December 20, 1870, The Struggles of Science, by George B. Loring, M.D., of Salem, Mass.; Tuesday evening, December 27, 1870, How we stand and walk, by Prof. Burt G. Wilder, of Cornell University, Ithaca, N.Y.; Friday evening, January 6, 1871, The Triumphs of Modern Surgery, by Prof. F. H. Hamilton, of Bellevue Hospital Medical College, New York; Friday evening, January 20, 1871, On Water, by Prof. C. F. Chandler, of Columbia College, New York; Friday evening, January 27, 1871, On Tides and Tidal Currents, and their effects upon Harbours, by J. E. Hilgard, of the U.S. Coast Survey, Washington, D.C.; Friday, February 3, 1871, On Light, by Henry Morton, President of Stevens Institute, Hoboken, N.J. These lectures are free to members of the Institute and their families, and are an evidence of the earnest work which this association is accomplishing in the way of the diffusion of knowledge.

THE centenary anniversary of the birthday of Humboldt was celebrated in Boston by the raising of a subscription to the amount of 7,040 dollars, which has been placed in the hands of the trustees of the Museum of Comparative Zoology as a Humboldt Scholarship for the benefit of young and needy persons engaged in study at the Museum, and the officers of the Museum have formally accepted the trust.

THE wonderful bore of five miles through the Hoosac Mountains goes forward with persistent steadiness, and bids fair to be an accomplished fact in 1874, as promised by the contractors. Some conception of the magnitude of this great work may be formed when it is known that at the west end the workmen have fully one-third of a mile of solid mountain above their heads.

A ZOOLOGICAL Garden in Central Park, New York, has been objected to on the following grounds:—The defect of the Central Park is a lack of breadth and repose. This defect grows out of the natural limitations fixed by the original rocky surface of its site, and from the necessity of providing structurally for the convenience and safety of great throngs of people in a public pleasure-ground that is expected finally to be situated in the heart of a densely populated city. The impracticability of making, in either section of the Park, open spaces of greensward as large as desirable was recognised from the outset, but as much as possible was done to gain ground in this direction, and the central meadow stretches are the result, in the upper Park. They supply two connected spaces, each about a quarter of a mile in extent, partially separated by a mass of rock and almost completely surrounded by a border of indigenous trees, which are already beginning to take on umbrageous forms and to cast broad shadows over the now well-established turf. These meadows constitute the only broad space of quiet rural ground on the island which has been left undisturbed by artificial objects, and much labour has been expended to render practicable the preservation of their present general character. A Zoological Garden must be made up to a considerable extent, if not altogether, of small scattered buildings and small fenced yards; it requires little breadth or unity of surface in its site, and it must be adapted to recreation of a completely diverse character from that which this ground has been prepared to serve.

THE busy town of Sheffield, we are glad to see, is waking up to a sense of its position as one of the industrial centres of England. A meeting was recently held to consider the propriety of presenting the Museum of the Literary and Philosophical Society to the town, on condition that a Free Public Museum be established by the Town Council, and a suitable building be erected. The President of the Society, Mr. H. C. Sorby, occupied the chair, and briefly opened the meeting. The Master Cutler, Mr. W. Bragge, then rose and proposed the following resolution, "That upon the establishment by the Town Council of Sheffield of a Free Public Museum and the provision of such accommodation for it as the Council of the Literary and Philosophical Society shall deem suitable, the said Society hereby authorises its Council to transfer to the said Free Public Museum its collections of specimens illustrating natural history, geology, mineralogy, antiquities, numismatics, ethnology, and industrial art, and also such apparatus as is of historical interest, together with the cases containing the said articles, but not its library nor modern scientific apparatus." After some discussion, the resolution was put to the meeting and carried unanimously.

DR. COBBOLD describes, in the *British Medical Journal*, an Entozoon which, if not actually, is practically unknown to the most experienced helminthologists. The *Stephanurus dentatus* is a species of Strongylus which has hitherto been described only by the late Professor Diesing of Vienna in the *Annalen des Wiener Museums*, a scarce book to obtain. It is not fully described in any of the systematic works, and appears never to have been seen by Kuchenmeister or Von Siebold. The interest of this observation consists less in the ready identification of the parasite by Dr. Cobbold, than in the abundance with which, according to Dr. Fletcher of Indianopolis, who forwards the specimen for examination, it is found in the hogs slaughtered in that part of the world. The bearing of this observation on the extension of parasitic disease remains yet to be determined. The suggestions arising out of Dr. Cobbold's observation will, no doubt, be followed out by local inquirers; and we shall expect to hear more of this hitherto rare, and probably interesting, stranger.

AT a recent meeting of the Scientific Committee of the Royal Horticultural Society, Mr. Alfred Smee exhibited some lemons from Sicily attacked by a species of coccus, quite distinct from the well-known coccus of the orange, and apparently an undescribed species. It was stated that nearly the whole of the lemon-crop in Sicily is attacked by this parasite, which renders it almost valueless for the English market. Although the juice is not much affected, the skin is completely spoilt and rendered uncrystallisable; by far the most important use of lemons in this country being of the rind for use by confectioners. The root appears to be at the same time attacked by a fungus.

AT the close of 1870 the numbers of the several classes belonging to the Institution of Civil Engineers were:—16 honorary members, 709 members, 1,010 associates, and 201 students, together 1,936, as against 1,802 at the same date last year, showing an increase at the rate of 7½ per cent. in the twelve months.

IN analysing the statistics of inquests held as Coroner of Central Middlesex, Dr. Lankester points out, in his seventh annual report just prepared, that the proportion of suicides to the population in England and Wales is 1 in 12,000 of the population, while the proportion in Central Middlesex is about 1 in 13,000 of the population. The figures seem to show that of all causes of death suicide is the most constant. The proportion in which the sexes commit suicide is nearly everywhere the same. It may be stated that the proportion of males to females is as five to two. The ages at which suicide is committed are for the seven years nearly the same. One in twelve are young people under 20 years of age; a larger proportion amongst people above 60; and the remainder, four-fifths of the whole, are equally divided amongst people from 20 to 40 years of age. A further analysis of the cases shows that, as a rule, women prefer taking poison and drowning themselves. Of the twenty-three cases of female suicide in 1868-9, six were from poison and ten from drowning. Women seldom cut their throats or hang themselves, whilst, of the sixty-six cases of male suicide, exactly half chose these methods of self-destruction. Men are also more given to jumping out of windows and from the tops of high places.

AN admirable paper on "The Food and Habits of Beetles," by Mr. Townend Glover, appears in a recent report of the Commissioner of Agriculture for Washington. It contains brief descriptions of the beetles which are injurious to vegetable or animal substances, with their common and scientific names, and nearly 200 figures, roughly but characteristically executed. An alphabetical list of the principal substances frequented by beetles is given at the end, with cross references to the first part of the paper.

THE Winchester and Hampshire Scientific and Literary Society, which was formed in 1869, has issued its first report. From this we learn that over 100 members have joined, and that the meetings are frequent and well attended. The papers are not confined to subjects connected with natural history, but embrace archæology and kindred topics. There is a botanical section, of which Mr. Frederick J. Warner is secretary; and we gather from the report that a flora of Winchester is in contemplation. It is perhaps not too much to hope that, with the aid of the Newbury Club, a complete flora of Hampshire may be undertaken by the society. The President is the Rev. C. A. Johns, Mr. A. Angell, jun., acting as the hon. secretary. The meetings are held on the second Monday of each month. The annual subscription is 10*s*.

THE *Scottish Naturalist* states that a work on the Birds of Scotland, by Mr. Robert Gray, Secretary to the Glasgow Natural History Society, is in the press; and that Mr. Howie, Secretary of the Largo Naturalists' Field Club, is drawing up, for publication, a catalogue of the plants of Fifeshire.

A JOURNAL is published in Liverpool, under the title of "Cope's Tobacco Plant, a Monthly Periodical, interesting to the Manufacturer, the Dealer, and the Smoker," from the January number of which we quote the following:—"Huxley and Friends—Attention! An American exchange says:—'Professor Huxley may become a good Christian yet; he smokes now, after forty years' hostility to tobacco.' Now, at cost of losing ground for ourselves, we ask Professor Huxley and his friends—Will they admit that the distinguished gentleman was not a Christian during those forty years when he abstained from tobacco? And that the first step towards his salvation was—smoke? We expect an answer; because, should we be in danger of yielding to the arguments of zealous anti-tobaccoites, we might fall back upon that most infallible of argument-stoppers, the religious sentiment."

A LETTER in the *Gardener's Magazine*, which has just come to our notice, imputes a grave charge of false teaching to the authorities of the South Kensington Museum. The writer says:—"In the Educational Series is a large glass case of British butterflies, conspicuously labelled 'The Gardener's Foes.' The case contains forty-three out of the sixty-six native species. Now, the idea of all these, or even the majority of them, being injurious, is most absurd, and tends to perpetuate erroneous notions, and to exterminate these beautiful creatures from the land." Then follows an enumeration of the species exhibited, and the names of the plants upon which, according to the writer, the insects actually feed, most of them being roadside weeds. We draw attention to this in order that "erroneous notions," on whichever side they may be, may *not* be "perpetuated."

GOLD ore having been discovered in Madagascar, the Government of the island has prohibited the search. If gold is discovered in remunerative quantities, there will be such a rush of Europeans to the country as will dispossess the native inhabitants.

THE African diamond fields are being overdone; they have become the seat of an extensive population. A little canvas town has rapidly sprung up on the banks of the Vaal River. Stores have been opened, the rival proprietors of which advertise their wares in a newspaper devoted to the diamond interest, and printed on the field; and, finally, a music-hall has been started for the amusement of the thousands of diggers, who with their wives and children are now encamped in the "happy valley."

SCIENCE IN AMERICA

THE following appropriations by the U.S. Congress were made at the session of 1869-70 for the ensuing year, July 1, 1870, to June 30, 1871, in aid of Science, Literature, &c.

It should be observed that the undermentioned appropriations are those of the General Government, and not those of the separate States, which, in the aggregate, would far exceed the amount here presented.

	$	$
Museums.		
National Museum in charge of Smithsonian Institution . . .	20,000	
Army Medical Museum. . . .	5,000	
Agricultural Departmt. Museum .	8,000	
		33,000
Botanic Gardens and Greenhouses.		
Of the U.S. Capitol	35,996	
„ „ President's House . . .	2,500	
„ „ Agricultural Department	38,200	
		76,696
Agriculture.		
Department of Agriculture, Miscellaneous Expenses		138,070
[To this is to be added, items already given,—		
Botanic Garden and Living Plants 38,200		
Museum 8,000		
Library . . . . . . 3,800—50,000		
or an aggregate of $188,070.]		
Astronomy and Meteorology.		
Observations of Eclipse, Dec. 1870, under Coast Survey . .	29,000	
U.S. Nautical Almanac. . . .	20,000	
National Observatory	19,800	
New Telescope for National Observatory	50,000	
Telegraphic Notices of Storms .	50,000	
		168,800
Surveys, &c.		
U.S. Coast Survey	703,000	
Survey of Lakes	150,000	
„ Nicaragua and Tehauntepec Ship Canals	30,000	
Military Surveys west of Mississippi	100,000	
Prof. Powell's Survey of Colorado of West	12,000	
Polar Explorations	50,000	
Dr. Hayden's Geological Survey	25,000	
Statistics of Mines and Mining .	10,000	
		1,080,000
Light-house Establishments		1,431,207
Libraries.		
Library of Congress	36,220	
„ of Medical Department, U.S.A.	3,000	
„ of Agricultural Department	3,800	
		43,020
Education.		
U.S. Department of Education .	14,500	
Wilberforce and Lincoln Universities	37,000	
		51,500
Benevolent Objects.		
Life-boat Service on the Coast .	48.883	
Government Hospital for Insane	149,980	
Columbia Institution for Deaf and Dumb	40,775	
Columbia Hospital for Women .	18,000	
National Association for Destitute Coloured Women, D.C. .	10,000	
National Soldiers' and Sailors' Home, D.C.	15,000	
Care of 60 transient Paupers . . .	12,000	
		234,635
Total		$ 3,316,928

THE INFLUENCE OF INTENSE COLD ON STEEL AND IRON

THERE has recently been a most interesting discussion at the Literary and Philosophical Society, Manchester, on the above subject, the result of which seems to be that we must at once give up the idea that such accidents as the one, for instance, near Hatfield, are due to anything beyond the control of the Railway Companies concerned.

The paper which gave rise to the discussion was by Mr. Brockbank, who detailed many experiments, and ended by stating his opinion that iron does become much weaker, both in its cast and wrought state, under the influence of low temperature; but Mr. Brockbank's paper was immediately followed by others by Sir W. Fairbairn, Dr. Joule, and Mr. Spence, which at once put an entirely new complexion on the matter.

As Dr. Joule's results are the most to the point we may take them first. He says:—

"As is usual in a severe frost, we have recently heard of many severe accidents consequent upon the fracture of the tires of the wheels of railway carriages. The common-sense explanation of these accidents is, that the ground being harder than usual, the metal with which it is brought into contact is more severely tried than in ordinary circumstances. In order apparently to excuse certain Railway Companies, a pretence has been set up that iron and steel become brittle at a low temperature. This pretence, although put forth in defiance, not only of all we know of the properties of materials, but also of the experience of everyday life, has yet obtained the credence of so many people that I thought it would be useful to make the following simple experiments:—

"1st. A freezing mixture of salt and snow was placed on a table. Wires of steel and of iron were stretched so that a part of them was in contact with the freezing mixture, and another part out of it. In every case I tried the wire broke outside of the mixture, showing that it was weaker at 50° F. than at about 12° F.

"2nd. I took twelve darning needles of good quality, 3in. long, $\frac{1}{24}$in. thick. The ends of these were placed against steel props, $2\frac{1}{8}$in. asunder. In making an experiment, a wire was fastened to the middle of a needle, the other end being attached to a spring weighing-machine. This was then pulled until the needle gave way. Six of the needles, taken at random, were tried at a temperature of 55° F, and the remaining six in a freezing mixture which brought down their temperature to 12° F. The results were as follow:—

Warm Needles.	Cold Needles.
64 oz. broke	55 oz. broke
65 „ „	64 „ „
55 „ „	72 „ „
62 „ „	60 „ bent
44 „ „	68 „ broke
60 „ bent	40 „ „
Average $58\frac{1}{3}$	Average $59\frac{5}{6}$

"I did not notice any perceptible difference in the perfection of elasticity in the two sets of needles. The result, as far as it goes, is in favour of the cold metal.

"3rd. The above are doubtless decisive of the question at issue. But as it might be alleged that the violence to which a railway wheel is subjected is more akin to a blow than a steady pull; and as, moreover, the pretended brittleness is attributed more to cast-iron than any other description of the metal, I have made yet another kind of experiment. I got a quantity of cast-iron garden nails, an inch and a quarter long and ⅛in. thick in the middle. These I weighed, and selected such as were nearly of the same weight. I then arranged matters so that by removing a prop I could cause the blunt edge of a steel chisel, weighted to 4lb 2oz., to fall from a given height upon the middle of the nail as it was supported from each end, $1\frac{1}{16}$in. asunder. In order to secure the absolute fairness of the trials the nails were taken at random, and an experiment with a cold nail was always alternated with one at the ordinary temperature. The nails to be cooled were placed in a mixture of salt and snow, from which they were removed and struck with the hammer in less than 5^s."

The collective result of the experiments, the details of which need not be given, was that 21 cold nails broke and 20 warm ones.

Dr. Joule adds, "The experiments of Lavoisier and Laplace, of Smeaton, of Dulong and Petit, and of Troughton, conspire in giving a less expansion by heat to steel than iron, especially if the former is in an untempered state. Such specimens of steel-wire and of watch-spring as I possess expand less than iron. But this, as Sir W. Fairbairn observed to me, would in certain limits have the effect of strengthening rather than of weakening an iron wheel with a tire of steel.

"The general conclusion is this: Frost does *not* make either iron (cast or wrought) or steel brittle, and that accidents arise from the neglect of the companies to submit wheels, axles, and all other parts of their rolling stock to a practical and sufficient test before using them."

Mr. Spence in his experiments decided on having some lengths of cast-iron made of a uniform thickness of ½in. square, from the same metal and the same mould.

He writes:—"Two of the four castings I got seemed to be good ones, and I got the surface taken off, and made them as regular a thickness as was practicable.

"I then fixed two knife-edged wedges upon the surface of a plank, at exactly nine inches distance from each other, with an opening in the plank in the intervening space, the bar being laid across the wedges, a knife-edged hook was hung in the middle of the suspended piece of the bar, to the hook was hung a large scale on which to place weights.

"The bar was tried first at a temperature of 60° F.; to find the breaking weight I placed 56lb. weights one after another on the scale, and when the ninth was put on the bar snapped. This was the only unsatisfactory experiment, as 14 or 28lb. might have done it, but I include it among the others. I now adopted another precaution, by placing the one end of the plank on a fixed point and the other end on to a screw-jack, by raising which I could, without any vibration, bring the weight to bear upon the bar. By this means, small weights up to 7lb. could be put on while hanging, but when these had to be taken off and a large weight put on, the scale was lowered to the rest, and again raised after the change was made. I may here state that a curious circumstance occurred twice, which seems to indicate that mere raising of the weight, without the slightest apparent vibration, was equal in effect to an additional weight. 3¾cwts. were on the scale, a 14lb. weight was added, then 7lb, then 4lb, 2lb., 1lb., and 1lb., making 4cwts. and 1lb. This was allowed to act for from one to two minutes, and then lowered to take off the small weights, and replaced by a 56lb., intending to add small weights when suspended, raised so imperceptibly by the screw, that the only way of ascertaining that it was suspended was by looking under the scale to see that it was clear of the rest. As soon as it was half-an-inch clear it snapped, thus breaking at once with one pound less than it resisted for nearly two minutes.

"Six experiments were carefully conducted at 60° F., the parts of the bars being selected so as to give to each set of experiments similar portions of both bars; the results are marked on the pieces. My assistant now prepared a refrigerating mixture which stood at zero, and the bars were immersed for some time in this, and we prepared for the breaking trials to be made as quickly as could be, consistently with accuracy, and to secure the low temperature each bar on being placed in the machine had its surface at top covered with the freezing mixture. *The bars at zero broke with more regularity than at* 60°, *but instead of the results confirming the general impression as to cold rendering iron more brittle, they are calculated to substantiate an exactly opposite idea, namely, that reduction of temperature,* cæteris paribus, *increases the strength of cast iron.* The only doubtful experiment of the whole twelve is the first, and as it stands much the highest, the probability is that it should be lower; yet, even taking it as it stands, the average of the six experiments at 60° F. gives 4cwt. 4lb. as the breaking weight of the bar at that temperature, while the average of the six experiments at zero gives 4cwt. 20lb. as the breaking weight of the bar at zero, being an increase of strength from the reduction of temperature equal to 3·5 per cent."

Sir W. Fairbairn's evidence is of great importance, for he not only gives facts showing that frost does not affect the tires, but he states the real cause of such accidents as are generally attributed to the frost.

He states:—"It has been asserted in evidence given at the coroner's inquest on the Hatfield accident, that the breaking of the steel tire was occasioned by the intensity of the frost, which is supposed to render the metal brittle, and of which this particular tire was composed. This is the opinion of most persons, but judging from my own experience such is not the fact, and provided we are to depend on actual experiment, it would appear that temperature has little or nothing to do with it. Some years since I endeavoured to settle this question by a long and careful series of experiments on wrought-iron, from which it was proved that the resistance to a tensile chain was as great at the temperature of zero as it was at 60° or upwards, until it attained a scarcely visible red heat. To show that this was the case, and taking, for example, the experiments at 60°, it will be found that the mean breaking weight, in tons, per square inch, was in the ratio of 19·930 to 21·879, or as 1 : 1·098 in favour of the specimens broken at the temperature of zero. The generally received opinion is, however, against these facts, and it is roundly asserted that the strength of iron and steel is greatly reduced in strength at a temperature below freezing. The contrary was proved to be the case in wrought-iron plates, and assuming that steel follows the same law, it appears evident that we must look for some other cause than change of temperature for the late fracture of the tire on the wheel of the break-van of the Great Northern Railway. . . . The immense number of purposes to which both iron and steel are applied, and the changes of temperature to which they are exposed, renders the inquiry not only interesting in a scientific point of view, but absolutely necessary to a knowledge of their security under the various influences of those changes; and when it is known that most of our metal constructions are exposed to a range of temperatures varying from the extreme cold of winter to the intense heat of summer, it is assuredly desirable to ascertain the effects produced by those causes on material from which we derive so many benefits, and on the security of which the safety of the public frequently depends. It was for these reasons that the experiments in question were undertaken, and the summary of results are sufficiently conclusive to show that changes of temperature are not always the cause of failure, as that which occurred near Hatfield on the Great Northern Railway. That such is the fact, I may adduce several accidents of broken tires all of which occurred during the spring and summer months when the temperature was high. One of them occurred on the Lancashire and Yorkshire Railway in the summer of last year when the temperature was 50° to 60° above freezing. I could enumerate others in which the winter frosts had nothing to do with the fractures which ensued."

After referring to some other experiments, Sir W. Fairbairn proceeded: "The danger arising from broken tires does not, according to my opinion, arise so much from changes of temperature *as from the practice of heating them to a dull red heat, and shrinking them on to the rim of the wheels.* This, I believe, is the general practice, and the unequal, and in some cases, the severe strains to which they are subject, has a direct tendency to break the tires. To show how easily this may be effected, let us suppose that a tire, two feet six inches or three feet diameter, is shrunk on to a wheel one-tenth of an inch larger than the tire, it then follows that the tire in cooling must be elongated to that extent, with a strain equivalent to the force of the shrinkage, and calculated to produce that amount of molecular disturbance. It may be more or it may be less, but supposing the strain to be one-half or three-fourths of that which would break the tire, it then follows that the constant action of its irregular motion on the rails must ultimately lead to fracture.* I am not surprised that this should be the case, as most, if not the whole, of railway tires, excepting those on engines and tenders, are not turned, but selected by hand, heated and shrunk upon the wheels with every degree of tension, as suits the convenience of the workman. So long as this process is pursued the public will be exposed to the risk of broken tires. What is required in this description of manufacture is, that the rim of the wheel and the inside of the tire should be *turned to a standard gauge,* accurately calculated to give the required amount of tightness with a larger margin of strength, and this done we should attain greatly increased security to the public, and a great saving in wear and tear—to say nothing of the large sums expended by companies in the shape of compensation for injuries and loss of life."

Here, then, is another potential triumph for more scientific accuracy and more hope for travellers.

SCIENTIFIC SERIALS

Poggendorff's Annalen der Physik und Chemie, 1870, *No.* 9.—The contents of this number are:—(1.) "Calorimetric Researches," by R. Bunsen. In the first part of this paper Prof. Bunsen describes the construction and method of using a new calorimeter, in which quantities of heat are measured by the amount of ice at 0° which they are capable of converting into water at the same temperature. The quantity of ice melted is in its turn indicated by the resulting diminution of volume. as shown by the movement of a mercury-column in a graduated capillary tube communicating with the vessel in which the ice is contained. In order to convert the results obtained by this method into absolute heat-units, it is necessary, either that the motion of the mercury-column produced by a known quantity of heat should be ascertained, or that the specific gravity of ice at 0° and its latent heat of fusion should be known. The first of these quantities was found by observing the effect produced by a given weight of boiling water, and the second by a process which may be described as consisting in the application of the principle of the weight-thermometer to measure the change of volume which water undergoes on freezing. From these data the third of the quantities mentioned, or the latent heat of fusion of ice, is readily calculated. Of the numerical results, given in the paper, we will quote only the following:—

Specific gravity of ice at 0° C	0·91674
Latent heat of fusion of ice	80·025
Specific heat of indium	0·0570
Specific heat of calcium	0·1704

One special advantage of this method of calorimetry is that it allows good results to be obtained with very small quantities of material; for instance, for specific heat determinations, from 0·3 gramme to, at the most, 4 grammes is sufficient. (2.) "On the relations between the crystalline form and chemical constitution of some organic compounds," by P. Groth. (3.) "Experimental and theoretical investigation of the figures of Equilibrium of a liquid mass without weight" (Eighth series), by J. Plateau. A translation of this paper, which relates to the conditions of the ready production and of the persistency of liquid films, to the superficial tension of liquids, and to their superficial viscosity, was printed in the *Philosophical Magazine* vol. xxxviii. p. 445 [1869.] (4.) "On the Absorption of Light," by Paul Glan. Among other results, the author finds that the absorbing power of a substance, when it is employed in solutions of different degrees of concentration, increases in a greater ratio than the concentration; also, that the absorbing power of a body in solution is affected by the nature of the medium in which it is dissolved. The experimental results are followed by a mathematical discussion of the mechanism of the absorption of light. (5.) "Additional researches into the behaviour of Vapours in relation to the Laws of Mariotte and Gay-Lussac," by Dr. Hermann Herwig. This paper has reference to an earlier one published in vol. cxxxvii. of *Poggendorff's Annalen.* The author finds that, when the pressure upon a vapour at a given temperature is diminished so far that the vapour obeys Mariotte's law, that is to say, so far that the product of the pressure into the corresponding volume becomes constant, this product bears to the similar product, when the pressure is great enough to cause the vapour to be saturated at the same temperature, a constant ratio which is proportional to the square root of the absolute temperature. In the present paper it is shown that ethylic bromide and carbonic sulphide conform to this law. (6.) "Some analogous Theorems in Photometry and in the Laws of Attraction," by Wilhelm von Bezold. The mathematical law of the inverse square of the distance applying equally to the illumination produced by a luminous point, and to the force exerted by an attracting particle, it follows that the mathematical expressions by which photometrical relations are expressed, will also admit of an interpretation in relation to the action of attracting particles. In this paper the double interpretation of the same formula is pointed out in several important cases. For example:—The author shows that the photometrical analogue of an equipotential surface drawn about several attracting particles, is a surface so placed, relatively to luminous points, whose luminosity is proportional to the masses of the particles, that the illumination of each element of the surface is greater than that of any other element passing through the same point. (7.) "On the Luminosity of Phosphorus," by W. Müller. The author finds that phosphorus vapour is not luminous in the absence of free oxygen; that it is not luminous at ordinary atmospheric temperatures when in contact with pure oxygen of atmospheric pressure, but that it becomes luminous, and at the same time absorbs oxygen, when the pressure is diminished to a certain amount, depending on the temperature, the necessary reduction of pressure being greater when the temperature is lower; and that phosphorus which has been for some time in contact with certain vapours, (notably hydrocarbons), is deprived by them of the property of becoming luminous on the admission of air, although air, mixed with the same vapours, is not thereby deprived of the power of exciting (temporary) luminosity in phosphorus. (8.) "On the Superoxides that can be prepared by Electrolysis," by W. Wernicke. (9.) "On a mechanical theorem applicable to Heat," by R. Clausius. (10) "On the Spectra of negative Electrodes, and of long-used Geissler's Tubes," by Prof. Edm. Reitlinger and Prof. Moriz Kuhn. (11.) "On the Meissner Lignite modified by contact with Basalt," by Dr. A. von Lasaulx. (12.) "On the analysis of Silicates," by E. Ludwig. Refers chiefly to the precautions required for the accurate separation of silica and alumina. (13.) "On the absorption-spectrum of liquid peroxide of Nitrogen," by August Kundt. On comparing the absorption-spectra of liquid and gaseous peroxide of nitrogen, the author found that the ill-defined black bands in the spectrum of the former coincided in position with strongly-marked groups of lines in the spectrum of the latter. (14.) "On the work done by Gases in Motion, or remarks on the paper so entitled," by Dr. A. Kurz. This is a reply to a criticism by Dr. Boltzman (noticed in NATURE, vol. ii. p. 364) of a previous paper by the author.

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* From long-continued action under strain, it has been proved that it is only a question of time when rupture takes place, as repeated increased and diminished changes with the same load ultimately leads to fracture.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, January 17.—Prof. Newton, F.R.S., V.P., in the chair. The Secretary read a report on the additions to the Society's menagerie during the month of December 1870, amongst which were particularly noticed a specimen of the Two-toed Amphiuma (*Amphiuma didactylum*), and an example of Erxleben's Monkey (*Cercopithecus Erxlebeni*). On concluding his report the Secretary called attention to the registers of accessions to and deaths in the Society's menagerie, which lay on the table, and showed, in contradiction to statements recently published by Dr. Gray, that they were faithfully kept up, and that a revised abstract of the former was published every year as an appendix to the Society's "Proceedings."—Mr. Howard Saunders exhibited and made remarks on a series of skins of eagles belonging to *Aquila imperialis*, *A. bifasciata*, and *A. nævioides*.—A letter from Mr. R. Brown was read, recommending the introduction of hogs into countries where poisonous serpents were frequent, in reference to a communication from the Governor of Santa Lucia, read at the last meeting of the Society.—Mr. Jules Verreaux made some remarks on the facility with which the colouring matter in the wings of the Touracous was soluble, and stated that he had observed it washed out in the living birds by heavy rain.—Mr. J. E. Harting exhibited and made remarks on a specimen of Sabine's Snipe, recently killed in Ireland.—Mr. Sclater made some remarks on the amphibians usually called Axolotls, now living in the Society's gardens, and pointed out that if what Prof. Baird had recently stated were correct, these were not the true Axolotl of the lakes of Mexico (*Siredon mexicanum*), but the larval form of a known Salamander—*Amblystoma mavortium*, Baird. Mr. Sclater likewise exhibited a typical specimen of *Ateles variegatus*, Wagner, and pointed out its unquestionable identity with *A. Bartletti*, Gray.—Mr. J. W. Clark read a paper on a fine skull of the Narwhal (*Monodon monoceros*) with two tusks in the Cambridge University Museum, to which were added full particulars as to all the known bidentate skulls of this animal.—A paper was read by Dr. J. C. Cox, C.M.Z.S., containing descriptions of some new species of Australian land-shells.—Prof. Newton exhibited and made remarks on some new and rare birds' eggs, amongst which were those of the Sanderling (*Calidris arenaria*) and Lesser Sheath-bill (*Chionis minor*).—Mr. St. George Mivart pointed out the characters of a new genus of Insectivorous Mammals proposed to be called *Hemicentetes*, founded on the *Erinaceus madagascariensis* of Shaw, to which was added a revised synopsis of the known genera of the order Insectivora.—A communication was read from Mr. A. G. Butler, containing descriptions of some new species of Exotic Lepidoptera.—Mr. G. F. Angas communicatad a list of additional species of marine mollusca to be included in the Fauna of Port Jackson and the adjacent coasts of New South Wales.—Mr. Sclater read some notes on the typical specimens of *Tyrannula mexicana*, Kaup, and *T. barbirostris*, Swainson.—A second communication from Mr. Sclater contained remarks on certain species of *Dendrocolaptidæ*, in the collection of the Smithsonian Institution.

Geological Society of London, January 11.—Joseph Prestwich, F.R.S., President, in the chair. The following communications were read:—1. "On the older Metamorphic Rocks and Granite of Banffshire," by T. F. Jamieson, F.G.S. The author indicated three divisions in the metamorphic strata a Banffshire:—At bottom of great thickness of arenaceous beds, more or less altered into quartz-rock, gneiss, and mica-schist; next a series of fine-grained clay-slates, in the midst of which is a bed of limestone; and then again an upper group of arenaceous strata. The author stated that the arrangement of the rocks is very similar to that occurring in Bute and Argyleshire. He remarked that the general texture of the beds is fine-grained, and considered that they were probably deposited in the depths of the sea, off the mouth of a great river, the deposition of the argillaceous strata having taken place during a period of increased depression. The deposition of the beds was said to have probably taken place after the formation of the (Cambrian) Red Sandstone and Conglomerate of the North-west Highlands, or in Lower Silurian times, the river by which the sediment was brought down being supposed to have drained the great Laurentian region to the north-west. After their accumulation the author supposed that "a glow of heat from beneath" approached them, causing expansion and the wrinkling of the mass into folds running from S.W. to N.E. The granites were considered by the author to owe their origin to the fusion and recrystallisation of the arenaceous beds. Prof. Ramsay observed that the general section wonderfully corresponded with that given many years ago by Sir Roderick Murchison, of the Silurian and Laurentian rocks at Cape Wrath, and it seemed to him that the large views originally propounded by Sir Roderick were confirmed by the author. He was glad that the metamorphic origin of granite was supported by Mr. Jamieson, as he had held that view for several years, and he was pleased to find that opinions which had formerly met with so many opponents were constantly gaining acceptance. The fusion of these sedimentary rocks by metamorphic action was not identical with the fusion of lava, but their fluidity might be the same; and if that were the case there could be no difficulty in accepting the possibility of the injection of such fused rocks into crevices and fissures. The crumpling of the beds, however, was due to more extensive causes than those contemplated by the author. The proportion of igneous rock injected into contorted rocks, like those of North Wales, was almost infinitesimal, and the crumpling could hardly be due to mere local causes.—Prof. Ansted referred to what he had observed in the north-west part of Corsica, where about 40ft. of granite were distinctly interstratified between perfectly unmetamorphosed beds of sandstone and limestone, without any alteration at the points of contact, such as would be produced by an igneous rock. He also cited the crumpled strata in the Maritime Alps, in which the granites were parallel with the other beds, and seemed to form part of them. Mr. Carruthers mentioned that the late Prof. Fleming twenty years ago had taught the same doctrine as to the nature of granite as that held by the last speakers.—Mr. David Forbes agreed that the crumpling of the strata was not due to the intrusion of any eruptive rock. He completely disagreed with Prof. Ramsay and the author as to the origin of granite, and maintained that, in the sedimentary rocks traversed by the granite, the requisite ingredients for the formation of granite did not exist. The proportion of felspar in quartzose rocks was infinitesimally small, as compared with that entering into the composition of granite. He could not accept the notion of the heat from the interior approaching gradually to some portion of the surface. Prof. Ramsay, in reply to Mr. Forbes, maintained that some of the slaty rocks of Wales, by extreme metamorphism, would pass into some kinds of granite. As to the conditions of metamorphism of the rocks, this process must have gone on at a time when these older rocks were overlain by a great thickness of more recent beds which have since been removed by denudation. 2. "On the connection of Volcanic action with changes of Level," by Joseph John Murphy, F.G.S. The author commenced by discussing the chemical theory of volcanic action, which he considered he had disproved. He remarked on the coincidence of volcanic action with elevation of the surface, but stated his opinion that the elevation of one part of the earth's surface and the depression of another, are the results of a movement of subsidence in the following manner:—The interior of the earth is constantly cooling; and as it cools it must contract. But the cold strata of the surface cannot contract in the same proportion; and as they must remain in contact with the core, they are compelled to form folds and ridges. The breaking out of volcanoes is due to the breaking of part of the earth's crust by these foldings. According to the author, "volcanic action is not the cause, but the effect of secular changes of level; and secular changes of level are due to the subsidence of the surface on the interior, as the interior contracts in cooling." 3. "On some points in the Geology of the neighbourhood of Malaga," by Don M. de Orueba. Communicated by Sir R. I. Murchison, F.R.S., F.G.S. After referring to the writings of previous authors upon the geology of the south of Spain, the author noticed a mountain-chain near Antequera, one branch of which, known as the "Torcal," he described as presenting a very singular appearance from the huge blocks of stone of which it is composed. The division of the rock into separate blocks, often of the most fantastic shapes, was attributed by the author to denudation by water. The "Torcal" consists of a compact limestone, generally of a red colour, resting conformably on the east upon a fine-grained white oolitic marble of considerable thickness. At the divisional line between the two formations many Ammonites were said to occur, and three of these were doubtfully identified with *A. giganteus*, *biplex*, and *annulatus*. These species would indicate the deposit to be probably of Portlandian age. The plain of Antequera was considered by the author to consist of Tertiary formations. One of these, at the south of the city, he regarded as analogous to the

"Calcaire grossier." He mentioned indications of the presence in the vicinity of a Miliolitic marble, and of a limestone containing Nummulites. Between Antequera and the Torcal, he noticed a small calcareous deposit containing many forms of *Gryphæa*. The paper was illustrated by photographs of two scenes on the Torcal, and of several species of Ammonites. Prof. Ansted remarked that the condition of the Torcal was similar to that prevailing in many other limestone districts, and was probably due to subaerial denudation. Mr. W. W. Smyth mentioned that he had lately had an opportunity of examining, at Cadiz, a collection of fossils formed by Mr. Macpherson in that district, which also contained specimens of Ammonites. It appeared that there were large tracts in which the rocks appeared almost destitute of fossils, which rendered their classification extremely difficult; and great credit was due to the author for his exertions in a country where unfortunately so little interest was taken in geology. He mentioned that some of these unfossiliferous rocks had been classified as Silurian by some French geologists; but for this there was not the slightest evidence. It appeared far more probable that they were of Jurassic age. Some red beds, which had been called Triassic, were also in all probability Tertiary. Mr. Gwyn Jeffreys, who had examined several collections in Spain and Portugal, stated that he had been much struck with the absence of newer Tertiary fossils, the latest being of Miocene age. These latter presented a tropical aspect, and differed from the mollusca now inhabiting the neighbouring seas. Mr. Blake was not satisfied with the determination of the Ammonites, which appeared to him rather of Cretaceous than Jurassic forms. Mr. Tate observed that the French geologists had determined the existence in Spain of the whole Jurassic series, from the Lower Lias to the Portlandian beds; and, judging from the photographs, he should consider the Ammonites to be Jurassic. Mr. Boyd Dawkins cited the remains of *Rhinoceros etruscus*, procured by the late Dr. Falconer at Malaga, as affording evidence of the presence of Pliocene age in that district. Prof. Duncan mentioned that he had found corals of the genus *Flabellum*, such as were found in the Tejares clays, in recent deep-sea dredgings in the Atlantic, and among specimens brought from Japan.

Linnean Society, January 19.—Mr. G. Bentham, president, in the chair. "Historical Notes on the Radix Galangæ of Pharmacy," by D. Hanbury, F.R.S. The introduction of this drug into Europe appears to have been due to the Arabians; its common use in the West does not date earlier than the 15th century. It is an aromatic stimulant, and may be used to replace ginger; but the high virtues ascribed to it by the ancients cannot be sustained.—"On the Vegetation of the Solomon Islands," by Mr. J. Atkin. The writer had spent some months in these little-known islands, chiefly in Christoval, the southernmost of the group, which lies between 10° and 11° S. lat., and between 162° and 163° E. long. The whole group extends for about 300 miles eastwards to Papua, over $4\frac{1}{2}°$ of longitude and 4° of latitude. They are mainly of volcanic origin; the low lands consist of coral, which reaches to an elevation of from 300 to 500 feet. Earthquakes are very frequent, almost every month, but not very severe. The nearest active volcano is Tinkalu, 200 miles to the westward. The wet season is in winter, especially the early part of July, when an enormous quantity of rain falls. The temperature is remarkably uniform; the writer had never seen the thermometer below 75° F. or above 88° in the shade, or 132° in the sun; the air is extremely damp. The highest land in Christoval is from 3,000 to 4,000 feet elevation, and is probably granite. The island is entirely covered with vegetation, except near the sea. Grasses are very few. In the forests are very few trees with trunks five feet in diameter. The bush is very thick, and climbers numerous. One Aroid was noticed, and eight or nine Orchids, all epiphytal. Several Zingibers, including the true ginger, native. Three or four species of Pandanus, which are extremely variable. The cocoa-nut and sago palms are native, the latter growing eighty feet high; also the areca-palm, and the betel-nut; the latter is universally chewed. The yam is grown, as well as five other roots probably belonging to the same order. The bread-fruit is abundant; and a variety of mango grows wild, as well as a bitter orange. The leaf of the sago-palm is used for thatch. There is a Cycas thirty or forty feet high, which is sometimes branched. Of ferns the genera most observed were Asplenium and Acrostichum; but no tree-ferns, although they are so abundant in the neighbouring Banks's group. Two Convolvuli were noticed and an Ipomœa; two Hibisci, two Casuarinæ, and two Acacias, a tree and a shrub; also a Begonia, the same species as in Banks's group; and a handsome species of nettle. The men are short, with dark curly hair. They use spears, and sometimes bows and arrows; their canoes are very beautifully ornamented. Animals are comparatively few. Dogs and pigs are abundant, both apparently native; also opossums and a small rat. There are many beautiful birds; the white cockatoo is never seen, though so abundant in islands separated by a channel only fifteen miles broad. Insects are plentiful. Snakes, both land and water, abound, but none are poisonous. Scorpions are numerous but small. Alligators were found, but not abundant. Frogs plentiful; lizards innumerable; one iguana was seen four feet long.—"Note on *Byrsanthus*," by Dr. M. T. Masters, F.R.S. The chief interest of this paper lay in the author's exposition of the relation between the glands and the perfect stamens.

DIARY

THURSDAY, JANUARY 26.

ROYAL SOCIETY, at 8 30 —On the Mineral Constituents of Meteorites XII. The Breitenbach Meteorite: Prof. Story-Maskelyne, F.R.S —On the Organisation of the Calamites of the Coal Measures. Prof W. C. Williamson, F.R.S.—On Approach caused by Vibration (a Letter to Prof Guthrie) Sir W. Thomson, F R S.

ROYAL INSTITUTION, at 3 —Davy's Discoveries: Dr. Odling.

SOCIETY OF ANTIQUARIES, at 8.30 —On Remains on the Site of Keynsham Abbey: Rev. H. M. Scarth, M.A.

FRIDAY, JANUARY 27.

ROYAL INSTITUTION, at 9 —Dr. Odling.

QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, JANUARY 28.

ROYAL INSTITUTION, at 3.—Laws of Life revealed in History: Rev. W. H. Channing.

SUNDAY, JANUARY 29.

SUNDAY LECTURE SOCIETY, at 3.30.—The Nature of the Earth's Interior: D. Forbes.

MONDAY, JANUARY 30

VICTORIA INSTITUTE, at 8.—Archæology: Rev. J. Titcomb.

LONDON INSTITUTION, at 4.—On the First Principles of Biology: Prof. Huxley (Educational Course).

TUESDAY, JANUARY 31.

ROYAL INSTITUTION, at 3.—Nutrition of Animals: Dr. Foster.

ANTHROPOLOGICAL SOCIETY, at 8.—On some of the Racial Aspects of Music: Joseph Kaines, F.A.S.L.

WEDNESDAY, FEBRUARY 1.

SOCIETY OF ARTS, at 8.—On the Preservation of Vegetables: O. Buchanan.

THURSDAY, FEBRUARY 2.

ROYAL SOCIETY, at 8 30.

SOCIETY OF ANTIQUARIES, at 8 30.

LONDON INSTITUTION, at 7 30.—On the Action, Nature, and Detection of Poisons: F. S. Barff.

LINNEAN SOCIETY, at 8.—Natural History of Deep-Sea Soundings between Galle and Java: Capt Chimmo, R.N.

CHEMICAL SOCIETY, at 8.—On the Development of Fungi in Potable Water: Dr. Frankland.

ROYAL INSTITUTION, at 3 —Davy's Discoveries: Dr. Odling.

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Jevons

THURSDAY, FEBRUARY 2, 1871

SPECTROSCOPIC OBSERVATIONS OF THE AMERICAN ECLIPSE PARTY IN SPAIN

THE complete and accurate account of the observations of the American Eclipse party in Spain, under the charge of Prof. Winlock, which was given by Prof. Langley in NATURE two weeks ago, renders any further report of our spectroscopic work almost unnecessary; and yet perhaps a somewhat fuller statement with reference to one or two points may not be wholly superfluous.

Of the four spectroscopes employed, two were what might be called *analysing*, and two *integrating* instruments. The analysing instruments are designed to study separately the spectrum of different portions of the prominences and corona, only a small part of the object being examined at a time; the integrating instruments, on the other hand, deal with the entire mass of light received from the whole luminous body, without distinction of parts. In the first class of instruments, a distinct image of the object is thrown upon the slit by the object-glass of a large telescope, each point in the slit receiving light from only one point in the object. In the second class no image is thrown upon the slit, every point of which receives light from every point in the object; and if a telescope is used at all (as was the case with Mr. Pye's instrument), it must have a field of view large enough to include the whole object, and must have its eye-piece adjusted for distinct vision of a star—*i.e.*, in such a manner that the rays from a star shall be parallel when they leave the eye-piece. In this case the telescope increases the angular diameter and area of the object, and consequently the amount of light received, without otherwise at all changing the conditions.

Prof. Winlock's instrument and my own were of the former class. Prof. Winlock had a spectroscope of two prisms attached to an equatorial of 5½in. aperture, and about 7ft. focus. My own instrument had (during the totality) the dispersive power of seven prisms, and was attached to an equatorial of 6$\frac{4}{10}$in. aperture, and 8½ft. focal length.

The instruments of Messrs. Abbay and Pye were of the integrating kind. Mr. Abbay's had the collimator and observing telescope and two of the prisms which belonged to my old five-prism instrument. It was provided with the means of comparing the observed spectrum directly with the spectra of hydrogen, sodium, magnesium, and iron.

Mr. Pye's instrument was much smaller, but as its prism was made of the extra-dense flint, its dispersive power was very nearly the same as Mr. Abbay's; and the addition of a small telescope in front of the slit, magnifying about 2½ times, and thus increasing its light about six-fold, made it, I think, fully the equal of the other in power and efficiency.

Professor Langley has so well stated what we saw, that it is not necessary to repeat it; but I cannot refrain from putting on record that the sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality, and their gradual dying out, was the most exquisitely beautiful phenomenon possible to conceive, and it seems to me to have considerable theoretical importance. Secchi's *continuous spectrum* at the sun's limb is probably the same thing modified by atmospheric glare; anywhere but in the clear sky of Italy so much modified indeed as to be wholly masked.

I wish at this time to call special attention to the evidence which we obtain as to the extent of the self-luminous corona, or "leucosphere," as it has been recently named,* by combining the indications of the two classes of instruments.

By my direction Mr. Pye recorded the brightness of the lines which he saw during totality on an arbitrary scale from 10 down. These are his numbers, C 8·5, D_3 5·5, 1474 10, F 3. I suppose the actual amount of light of each kind would be roughly proportional to the squares of these numbers, for we seem instinctively to call one luminous object twice or thrice as bright as another when it would give the same light at twice or thrice the distance.

If so, the numbers representing the relative amounts of light would stand C 72, D_3 30, 1474 100, and F 9, neglecting fractions.

Now, in the analysing spectroscope the case is very different, and it is difficult to make an accurate estimate; but I think those who have been accustomed to observe both C and 1474, would admit that their ratio of brightness is something the same as that between a first and fifth magnitude star; *i.e.*, C is at least 25 times and perhaps 50 times as bright as 1474. Even during the totality, 1474 can hardly be called conspicuous in an analysing instrument, while C blazes like a red Sirius. It seems necessary, therefore, to assume that the area which emits the 1474 light is to the area which gives C, roughly in the proportion of 100 × 25 (or 50) to 72—that is to say, the angular area of the self-luminous corona is from 35 to 70 times as great as that of the red stratum of hydrogen and prominences combined. I suppose these taken together would be about equivalent to a ring 15″ high surrounding the sun, and this would make the self-luminous corona equivalent to another ring from 8′ to 16′ high.

Of course I am aware that the numerical data of this calculation are very uncertain, and I have therefore neglected all considerations of shading and inequality of illumination. But the principle is, I think, correct, and it has this advantage. The presence of a light cloud or haze does not sensibly affect the result, because the calculation is based solely on the ratios between lights of two different kinds in the two different instruments, and these ratios would not be seriously affected unless the cloud absorbed one kind of light more than the other.

With the analysing spectroscope alone the case is entirely different; a light cloud or haze vitiates everything. Thus some of the observers, favoured with a less clear sky than we at Xeres, saw the C and F lines even on the moon, undoubtedly by reflection from thin clouds. I saw myself the C line as far as 6′ or 7′ from the sun, far above any possible hydrogen atmosphere.

Therefore, although Prof. Winlock and myself *both saw* the 1474 line to a distance of more than 16′ from the sun, I should not dare to lay much stress on that observation as showing the true limits of the self-luminous coronal matter. I base my belief that the limit of 15′ or 20′ is reached by it in some of its angular prolongations, more upon the observations of 1869, when the sky was exceptionally clear, than upon anything seen at this time; and yet all the observations of this year, so far as I can see, accord well enough with the idea.

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* This name seems inadmissible, except as one of the sub divisions of the chromosphere.—ED.

The two faint lines which I saw last year between D and 1474, and which I thought might also be corona lines, were not seen this year by any one, so far as I can learn. I certainly saw two such lines last year, but I was not then at all positive about their belonging to the corona (I have felt somewhat annoyed by finding them put on the same footing as 1474 in several publications of the last year or two), and my present impression is that they were two of the faint iron lines that often appear in protuberances in that portion of the spectrum.

The question has been raised whether the corona line *exactly* coincides with the 1474 dark line in the solar spectrum. The difference, if any (and I have not found the slightest reason to suspect the least want of coincidence in observations with the whole dispersive power of 13 prisms), is less than $\frac{1}{10}$ of one division of Kirchhoff's scale.

Just before the totality began, I placed the slit of my spectroscope exactly tangential to the sun's limb at the point which would be last covered, and brought the 1474 line, already bright, as is usually the case at the base of the chromosphere, exactly to the cross hairs. After the totality had fairly begun, I moved the equatorial in right ascension until the slit was more than 16′ east of the sun's limb, and the line remained continually visible, though of course growing fainter as the distance from the sun increased. There is not the slightest possibility of mistake, nor of error beyond the limit named, *i.e.*, $\frac{1}{10}$ of one division of Kirchhoff's scale.

And now a few words in relation to the nature of the Corona. It seems to me to be a complex phenomenon, made up of at least four, perhaps five different elements; and in the main I concur with the views put forth by Mr. Lockyer in a recent number of NATURE, with the exception that I should be disposed to assign a greater relative importance to the truly solar portion of the phenomenon than he appears to do.

1st. We have, I think, surrounding the sun, beyond any further reasonable doubt, a mass of self-luminous gaseous matter, whose spectrum is characterised by the green 1474 line. The precise extent of this it is hardly yet possible to consider as determined, but it must be many times the thickness of the red hydrogen portion of the chromosphere: perhaps, on an average, 8′ or 10′, with occasional horns of twice that height. It is not at all unlikely that it may even turn out to have *no upper limit*, but to extend from the sun indefinitely into space.

2nd. This region undoubtedly reflects to us a certain amount of the ordinary photospheric sunlight. This reflected light is of course polarised radially to a considerable extent. Its spectrum ought to show the ordinary dark lines, but they are partly masked in the manner Mr. Lockyer has so happily explained, and partly by the faintness of the spectrum.

3rd. Our own atmosphere, even when clearest, must apparently extend this corona, both outwards, and inwards upon the moon's disc. Since, however, the inner edge of the coronal ring is far the brightest, the inward extension of the corona should be most marked, except at the very beginning or end of totality, and I have no doubt it is: that is to say, at the middle of totality the illumination of the moon's disc gives a somewhat exaggerated measure of the effect of our own atmosphere in extending the corona outwards. Accordingly, I am disposed to think the effect of the atmosphere (when clear) is a very subordinate one, since in 1869 the light upon the moon's disc was only very trifling compared with that even a whole degree from the sun. This atmospheric light would also be polarised radially. Its spectrum would be mainly that of the chromosphere, prominences, and "leucosphere" combined, a discontinuous bright line spectrum.

4th. There must be a large subjective element, for two even skilled observers, standing side by side, describe phenomena differing in very essential points.

5th. I am somewhat inclined to think with Oudemans (see his paper published in NATURE Nov. 10) that possibly *cosmical dust* between us and the moon may play an important part. Assuming a light cloud of such matter, one or two hundred thousand miles above the earth's surface and of great thickness, it becomes easy to account for the straight dark streaks, the varying form (if it does vary), and many other puzzling phenomena of the corona-phenomena which can hardly be produced by portions of our own atmosphere deeply immersed in the lunar shadow, but which, I own, seem to me now less aurora-like and less certainly solar than they did a year ago. I do not see how optical tests by polariscope and spectroscope could discriminate between the effects of such a cloud and those of our own atmosphere.

C. A. YOUNG

POPULAR NAMES OF BRITISH PLANTS

The Popular Names of British Plants. By R. C. A. Prior, M.D., F.L.S. Second Edition. (Williams and Norgate.)

THERE are many botanists who know little of the English names of Plants; and there are many who know these intimately, yet are not botanists. Both classes will welcome this comprehensive volume: and those who possess neither a philosophical nor a popular knowledge of the subject, will yet find abundant interest in a book, which is the work of an accurate scholar and philologist, as well as of a scientific botanist.

Most interesting, and perhaps least expected, is the light which these names throw upon the history of early civilisation. Many of them date from a period antecedent to the European settlement of the Aryan race, and enlighten us as to the habits of our remote ancestors some thousand years ago. We discover from them that the men who continuously advanced through many countries, from the confines of India to the British Islands, were no race of savages, but a comparatively civilised community: that they understood letters; that they had a knowledge of the useful metals; that they possessed the principal domestic animals; that they cultivated the oak, the beech, the birch, the hawthorn, the apple; grew wheat, barley, oats, rye, beans; built timber houses and thatched them; hedged their fields and fenced their gardens.

In a later class of names, which betray the intercourse of our forefathers with Roman cultivation and Grecian poetry, many a strange piece of myth or history lies embalmed. Thus the *Centaury* is the plant with which the Centaur Chiron healed the wound received from the poisoned arrow of Hercules. The beautiful and rare wild *Pæony* recalls Pæon, the physician-god of Homer, who healed the bellowing Ares when smarting from the spear of Diomed. The *Juno's Rose*, or tall white lily, preserves a story curiously transformed in later times from the Pagan to the Roman Catholic Mythology. The *Treacle-mustard* (Erysimum cheiranthoides) is the "Theriacum,' invented as an antidote by the Emperor Nero's physician and reappearing long afterwards as an ingredient in the Orvietan, or Venice Treacle. Mediæval tales and legends in abundance find illustration from the same source. The *Carline Thistle* is the herb which miraculously healed a pestilence that attacked the army of Karl the Great. The sea-loving *Samphire* is corrupted from Saint Pierre, the fisherman Apostle. The *Flower-de-Luce*, or Iris, was the device of Louis VII. The *Filbert* commemorates the horticultural skill of king Phillibert. The *Herb Robert* cures a disease named after Duke Robert of Normandy, The *Margarette*, or Daisy, owes its name to St. Margaret of Cortona, known to the readers of Mrs. Jameson.

More than one curious superstition is embalmed in well-known names. The *Celandine* is an altered form of the Greek "Chelidon," a swallow, because with its yellow juice the swallows were supposed to restore sight to their blinded young ones. The *Hawk-weed* records a like belief respecting the Hawk. The *Fumitory* (fume-terre) was thought to be produced without seed by vapours rising from the earth.

Many names, as *Wound-wort*, *Tetter-wort*, *Pile-wort*, *Nipple-wort*, have survived the ancient faith in their medical efficacy. And here comes in the strange principle of nomenclature, known as the "Doctrine of Signatures." Where the external appearance of a plant resembled any disease, or any part of the human body liable to a disease, such resemblance was taken as an indication of its especial healing virtue. The spotted leaves of the *Lung-wort* must be a specific, it was thought, for tubercular disease of the lungs; the scaly pappus of *Scabious* for cutaneous eruptions; the hard stony seeds of the *Gromwell* for stone in the bladder; the throat-like corolla of the *Throat-wort*, or Canterbury bell, for sore throats; the knotty tubers of *Scrophularia* for scrophulous glands The pretty *Toad-flax* of our walls and hedges owes its name to a strange mistake. Believed in early times to be a cure for a complaint known as "Buboes," it received the Latin name of "Bubonium." A confusion between Bubo and "Bufo," Latin for a Toad, gave birth to its present name; and stories were not long wanting that sick or wounded Toads had been seen to eat of it and to recover health.

Similar distortions occur in names not medically expressive. *Apricot*, connected ordinarily with the adjective "Apricus," sunny, is a corruption through the Spanish and Italian of "Præcoqua," early; having been looked upon as an early peach. *Sweet William* is transformed from "Æillet," a little eye; *Pink* is shortened from the Low German "Pinksten," Whitsuntide, a name due to the season of its blossoming. *Gilliflower* comes through *Giroflée* from "Caryophyllus," a clove. *Carnation* is from "Coronation," its flowers being used in chaplets. *Marigold* is Marsh-gold; *Cowslip* is "Hose-flap," applied originally to the large flannelly leaf of the Mullein; *Dame's Violet* from "Damascene" Violet; the blunder being kept up, as is often the case, in the Latin "Hesperis matronalis." *Hip* is the same word with *Jujube*; *Hawthorn* is "Hedge-thorn;" *London Pride* is named not from the smoky metropolis in which it thrives, but from a Mr. London, who introduced it. *Snap-dragon* is "Snout-dragon." *Daffodil*, more properly *Daffadowndilly*, is a combination of "Sapharoun" or "Saffron-lily" with "Asphodelus," the old English "Affodilly." With the taste for alliteration often shown in popular names, the Sapharoun-lily, blending with Affodilly, became by a mutual compromise Daffadowndilly, whence Daffodilly and Daffodil. *Peach* (Persicus), *Damson* (Damascenus), *Shalot* (Ascalonicus), and *Spinach* (Hispanicus), retain in their names dim memories of the lands of their birth. But the most curious instance of blundering is the *Jerusalem Artichoke*. It is a sun-flower, not an artichoke; but its tubers resemble the artichoke in flavour. From its Italian name "Girasole," turn-to-the-sun, came "Jerusalem;" and by a further quibble the soup which is made from it is called "Palestine soup."

Of the miscellaneous names many are equally interesting. The *Laburnum*, closing its petals at night like a tired labourer; the *Campion*, which crowned the champion of the tournament; the *Lady's Bedstraw*, recalling the days when mattresses were not, and when this fragrant Galium was used for ladies' couches; the *Heartsease*, with its many amorous synonyms; the *Shamrock*, concerning whose botanical identity no two Irishmen can agree; the *Lavender*, or Washerwoman, scenting freshly washed linen; the *Ozier*, from the oozy beds in which it grows, are a few out of countless specimens. The philologist may delight to trace the root of *Apple* from the Zend and Sanscrit "Ap" to the Latin "Pa" in "Padus," "Po" in "Pomum" and "Poto." Those familiar with Hans Andersen will read with fresh enjoyment the tale of the "Wild Swan," when they learn that the *Nettles*, which the Princess had to weave into shirts, are derived from the verb "ne," to spin or sew. On the other hand, some pretty time-honoured traditions are ruthlessly swept away. The *Narcissus* is referred, not to the enamoured Grecian youth, but to the Sanscrit "Nark" or Hell; and the protesting lover of the classics is reminded "that Proserpine was gathering Narcissi long before Narcissus was born." The *Fox-glove*, is not, as we had loved to think, like the *Troll-flower* and *Pixie-stool*, the "Folks'" or Fairies' "Glove"; but the Foxes' *Glew*, or Tintinnabulum, with its ring of bells, hung on an arched support. Worst of all, the charming story of the Forget-me-not, current in every European language, is a later legend framed to meet a name already extant. For the original use of the name, its first curious transference of allusion, its final attachment to a river-side flower, and the narrative of the drowning lover, which readily grew up to adorn it, we must refer our readers to Dr. Prior's book. They will be surprised to find a treatise on Botany *poetical*, and a Dictionary light reading. They will learn how much a technical subject may be enlivened by varied accomplishments in him who treats it; they will see that in the Science of Botany, as in every other Science, the widest culture brings about the most telling and effective teaching.

W. TUCKWELL.

OUR BOOK SHELF

A Ride through the disturbed Districts of New Zealand, together with some Account of the South Sea Islands. Being Selections from the Journals and Letters of Lieut. the Hon Herbert Meade, R.N., edited by his Brother. With Maps and Illustrations from the Author's Sketches. (Murray, 1870.)

THE title-page sufficiently describes this book, which is illustrated by some nice woodcuts, and several coloured lithographs of less merit. There is a good description of the Geysers of New Zealand, and of the state of the native insurrection in 1865; with some exciting narratives of attacks on Papuan cannibals in the New Hebrides. A good-sized house, built in a lofty tree and used as a fort, was seen in one of the Solomon Islands. One cannot but regret that the opportunities possessed by our officers on the Pacific Station for investigating the little-known natural history of the islands, are so seldom utilised. The author of this book often shoots, but hardly seems aware that his game could be of any other use than for food. The only natural history passage in the book is the following, dated Upolu, Samoan Islands:—"Saw a very rare bird, the Dodunculus, native name, which is peculiar to this island. It has the feet of a pigeon, beak of a hawk," &c., &c. Dodunculus! native name!! A. R. W.

Metals, their Properties and Treatment By C. L. Bloxam, Professor of Chemistry in King's College, London; Professor of Chemistry in the Royal Military Academy, and in the Department of Artillery Studies, Woolwich. Pp. 296. (London: Longmans, Green and Co., 1870.)

THIS is one of the text-books of science which are being edited by Mr. T. M. Goodeve and published by Messrs. Longmans. The series is intended to supply a want that has long been felt of exact and complete works on mechanical and physical science for the use of schools, and for the self-instruction of working men. A difficulty must have been experienced by many who are engaged in teaching science, when asked to recommend a small and inexpensive text-book, which may at the same time be so simply and clearly written as to be useful to those who have not had a scientific education, and who have not the advantage of being able to attend long courses of lectures. Many popular books on scientific subjects have been written, but they are not unfrequently somewhat inaccurate; difficult questions being often omitted, or, what is worse, treated in a superficial manner which is likely to mislead the student, inducing him to believe that these questions are very simple, and deluding him with the notion that he knows all about them. He is thus frequently disappointed at a subsequent period by finding that on studying the subject more minutely, it is much more complex than he at first imagined, and that many of the simple ideas which he had carefully fixed in his mind have to be discarded, and new ones acquired.

The book opens with an introductory section on the properties and treatment of metals, containing many useful tables, such as specific gravities, fusing points, conductivity, &c. The more common metals used in the arts are alone discussed, so as not to introduce unnecessary complication. The remaining sections of the book treat of iron and steel, copper, tin, zinc, lead, silver, gold, mercury, platinum, palladium, antimony, bismuth, aluminium, magnesium, and cadmium. The last six being far less important than the others, are very shortly described, and only occupy twelve pages.

Each section commences with a description of the ores of the metal under consideration, their composition being given, and also the per-centage of metal present. This is followed by the methods of treating the ores in order to extract the metal, chemical reactions being written in words without formulæ, so hat no preliminary knowledge is necessary. The mechanical treatment of the reduced metal is then detailed, and its useful applications in the pure condition or in the form of alloys. The book is profusely illustrated with good woodcuts, and is written in an extremely interesting manner which cannot fail to attract the attention of the student. This, together with the trustworthiness of its contents and its low price, will render the treatise extremely useful for scientific instruction. If the remaining text-books of the series possess all the advantages which are presented by this one, the thanks of teachers and students of science will be due to the editor and publishers for their undertaking.

Odd Showers: or, Explanations of the Rain of Insects, Fishes, and Lizards; Soot, Sand, and Ashes; Red Rain and Snow; Meteoric Stones; and other Bodies. By Carribber. (London: Kerby and Son, 1870.)

THIS little book is stated on the title-page to be "intended chiefly for young persons;" but others will, doubtless, gain information from it, as to the causes of the sudden appearance of swarms of insects and other animals, and showers of rain tinged with various colours, with respect to which so many popular errors are afloat. The writer derives his experience from a long residence in Canada, and one explanation of so-called "showers of blood" is new to us, that it is caused by the exudation of a crimson fluid by various chrysalides when passing into the imago state. The writer states that, on one occasion, twenty-eight chrysalides of *Vanessa antiopa*, the Camberwell Beauty, which he had preserved in a small room, underwent transformation in a single day in July; when the walls and floor were bespattered with a bright crimson-coloured substance resembling blood, as to give the appearance of a regular shower of the fluid.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Natural Science at Cambridge

IN NATURE for January 12, 1871 (p. 209), there appeared an article headed "Natural Science at Cambridge," which has the air of having been promulgated *permissu* (if not *auctoritate*) *superiorum.* It is extremely gratifying to read the list of exhibitions and scholarships founded or proposed to be founded in certain Colleges in this University, but the concluding sentence of the article has struck me as having been penned by one with whom the wish was father to the thought. It is said that "most of the Colleges are understood to be willing to award Fellowships for merit in Natural Science equivalent to that for which they are in the habit of giving them for Classics and Mathematics." Now this is entirely at variance with my own opinion on the subject, formed on a somewhat wide acquaintance with members of various colleges; and I would beg of the writer to be good enough to inform the public through your columns, first, how many Fellowships have solely and actually been awarded for merit in Natural Science, and, secondly, which of the sixteen colleges, besides Trinity, have absolutely declared that a Fellowship shall be the reward of great proficiency in Natural Science. I need not say how glad I shall be to find that my opinion is erroneous. M. A.

Cambridge, Jan. 28

Yellow

IT was not from any experiments of my own, but on the authority of Helmholtz, that I asserted the identity of brown with a dark yellow or orange. He found that the pure red and yellow of the spectrum gave the various shades of brown when seen by the side of more brilliantly lighted white surfaces. (Physiologische Optik, p. 281.) There is therefore nothing in the nature of the colour to exclude complete saturation, although it may well happen that most of the browns we ordinarily see fall somewhat short of it.

In NATURE of Jan. 26, Mr. Munro calls attention to the great brilliancy and saturation of many natural yellows as accounting for the difficulty of resolving them into their components. It is, no doubt, quite true that a full yellow could not be compounded of such reds and greens as we come across in daily life, but it is equally certain that a drab or dilute yellow could be; and yet no one recognises the fact by his unaided senses, or thinks it anything but strange and unlikely when told of it. And after all, can it properly be said that natural yellows are more saturated than other colours? That they approach more nearly the corresponding tints in the spectrum is admitted; but is that test a fair one? It seems to me that the homogeneous yellow itself must be considered as dilute when brought into comparison with the nearly primary red and green.

I have another difficulty in accepting Mr. Munro's explanation. A suitable mixture of any red, green, or blue will give a neutral grey. All four come within our every-day experience; but such a result seemed to Goethe, soon after Newton proved it, a paradox of paradoxes, and I believe to unsophisticated minds it seems so still.

Mr. Munro has ingeniously shown from the colour equations that there is no more primary blue in my blue disc than about $2\frac{1}{2}$ as much as in the red *plus* $1\frac{1}{5}$ as much as in the green—a conclusion which seems somewhat startling. In choosing the coloured papers and cards for the discs, I had great difficulty in finding a green that was even tolerably good, and the one that I finally used reflected large quantities of blue light. I had some thought of trying a green silk disc, which was of a much better colour, but feared errors depending on the different character of the surface.

It is not hard to see a reason for the comparative scarcity of good greens. To obtain a good red orange or yellow by means of absorption, all that is necessary is to cut away the spectrum above a certain point; for a good blue, the rays standing below a given one in refrangibility must be got rid of; but in order to isolate a green in anything like purity, the absorbing agent must hit off *two* points of the spectrum, removing all below one point and all above the other. The result is, that while nearly saturated yellows and reds abound—the scarlet of the geranium is almost perfect—hardly a good green is to be met with. The best I know is a mixture, prepared by adding bichromate of potash to a strong solution of sulphate of copper. The addition of a little chloride of chromium to remove the yellow more effectually is perhaps an improvement. If Mr. Munro would care to see the colours which I used for the discs, I should be very happy to send him samples.

Terling Place, Witham, Jan. 29 J. W. STRUTT

Comets' Tails

MAY there not be a connection between the colour of the sky and the tails of comets?

Suppose a comet to be surrounded with a wide-spread transparent atmosphere, holding in suspension matter as finely divided and as invisible as the sky-matter of our earth; and suppose the more condensed, but still transparent nucleus to act as a lens, throwing a beam of light upon the otherwise invisible mist of that atmosphere, could not most of the phenomena of those puzzling tails be thus accounted for?

Leicester, Jan. 28 FREDERICK T. MOTT

Ocean Currents

MR. LAUGHTON's letter in the latest number of NATURE shows that the suggestion of a probable influence of differences of atmospheric pressure on ocean currents has not been stated with sufficient clearness.

That a hydrostatic equilibrium would exist under a permanently unequal distribution of the pressures over the ocean, as Mr. Laughton argues, is not to be doubted. But is there any such permanent arrangement?

Granting that the high-pressure area of the trade-winds is nearly constant, varying little from day to day, and only expanding and contracting its limits gradually in summer and winter, the pressures to northward of this over the Atlantic are by no means so steady. Though they give on the average of the year, or even of the month, a *lower* pressure, yet the daily observations show that the pressures over wide areas may vary considerably, rising at one time to equal that of the Trade-wind patch, at another falling very much below it. Quoting from the daily barometric curves registered in the Quarterly Report of the Meteorological Office (for April—June, 1869), "On the 13th of May in that year, the seven stations whose weather is there recorded (Valencia, Armagh, Glasgow, Aberdeen, Falmouth, Stonyhurst, and Kew) show simultaneously a barometric pressure of 30·1 inches." On the 6th of May these stations had severally reported a pressure of only 28·9 inches. A difference of pressure equal to that of 16 inches of water had taken place over the entire extent of the British Isles, and probably over a much greater space.

The system of synchronous observation is not yet sufficiently extended to enable any positive statement of the general direction of the movement of low pressure areas across the North Atlantic to be made; but there is a strong probability that such depressions travel in a direction continuing the path taken by the hurricanes of the West Indies, since the majority of barometric hollows reach the British Isles from west or south-west. Just as the waters are forced to rise into the central low pressure of a hurricane (to the extent of several feet) and to follow its path, so, in a less degree, does it seem probable that the movement of extended and less violent depressions may influence the ocean currents.

The rate of progress of such depressions appears to be in an inverse ratio to their extent and depth. A West Indian hurricane moves onward at a rate of only fifteen miles an hour (Buchan's Meteorology, p. 269), and the highest speed of European storms, according to the same authority, is forty-five miles an hour; but that minor depressions may travel with much greater velocity is shown by reference to the quarterly report before noticed, where is the record of a hollow of from three to four tenths of an inch, which passed over the British Isles on the 24th of February, 1869, at the rate of ninety miles an hour.*

Either in causing a considerable change in the level, or in rapidly moving over it, from near an area of constantly high pressure, through a region where the average pressure diminishes, such depressions must surely influence the surface of the ocean, and either aid or retard its currents. That a difference in level of four inches in 1,800 miles can scarcely under any circumstances give rise to a current of twenty miles a day (the word hour in Mr. Laughton's letter is probably a typographical error) is also clear, but the temporary difference in level may be much greater than this within a much shorter distance. The average rate of the north equatorial current, moreover, in the Trade-wind region, is shown by the pilot charts of the Admiralty to be only from ten to twelve miles a day, and it is only claimed for difference of atmospheric pressure that it has some small share in aiding the formation of the current in question.

KEITH JOHNSTON, JUN.

Insulation of St. Michael's Mount, Cornwall—When did it occur?

THIS is a very interesting question, and the reader will be enabled to judge for himself, presently, whether there are not sufficient historical facts on record to enable us to answer—"In the eleventh century."

Domesday Book, date 1086, in the part relating to "Cornvalge" (*i.e.* Cornwall), at p. 2, has the following, which I have translated from the abbreviated Latin—"*The Land of St. Michael.*—Keiwal holds the church of St. Michael. Brismar was holding it in the reign of King Edward. There are two hides which never paid the Danish tax [nunquam geldaverunt]. The land is 8 carucates. There is 1 carucate with 1 villan, and two bordarii, and 10 acres of pasture. Value 20 shillings. Of these 2 hides, Earl Moriton took away 1 hide, value 20 shillings." And accordingly, at p. 11 of Domesday Book, there appear in the descriptive list of the many estates of Earl Moriton, corresponding particulars of the 1 hide which he had taken away.

Now, in the first place, Domesday Book gives no reason whatever for believing that, at its date, St. Michael's Mount was an *Island*, neither does Magna Britannia, vol. 1, p. 309, where the Mount is called *Mychel-stop*, or Michael's place. And in every case, while "annoting" those holding possessions in "Cornvalge," there is an entire absence in Domesday Book of any mention of island or islands on any of the coasts of Cornwall, just as if there had been then no islands on those coasts of sufficient extent to be worthy of mention. On the other hand, it is the custom in Domesday Book *when any place is an island, to call it so.* For example, in vol. i, folio 75, "Dorsete" [Dorset], we have—"The land of the King. The King holds the island which is called Porland [*i.e.* Portland]. King Edward held it in his life." And again, same vol., fol. 396—"Hanteschire [*i.e.* Hampshire]. These lands below written lie in the isle of Wit" [*i.e.* Wight].

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* The Meteorological Report shows that in January, 1869, 12 depressions reached the British Isles from W.-ward; in February 10, also from W.; in March 8 (5 from W.); in April 7 (5 from W.), in May 8 (7 from W.); and in June 2 from W.

In short, the Mount could not have been an island in 1086, because it then contained at least eight times as much land as it does at present, probably connecting it with the mainland, from which it is even now only one-third mile distant. The truth, Sir Henry Ellis says, seems to be that a hide, a yardland, a knight's fee, &c., contained no certain number of acres, but varied in different places at different times. He says there are "four virgates in each hide, and thirty acres to make a virgate." The elementary acre was 40 perches by 4 perches, as now. And, accordingly, the 8 carucates would amount to the respective numbers of acres mentioned in the last column of the following table at the respective times:—

Periods		Acres in a Carucate	No of Carucates	Total Acres
Carucate temp.	Richard I.	60	8	480
do.	do.	100	8	800
do.	Edward I.	180	8	1440
do.	32 Edward III (Oxon)	112	8	896
do.	Middleton	150	8	1200

The hide is generally supposed to have been 120 acres. Now, taking the smallest of these measures, we shall have for the two hides 240 acres. The present area of the Mount, however, is only 30 acres, so that there are 210 acres missing. How can we account for them except by supposing that the Mount extended further, perhaps in every direction? The hide of land taken by Earl Moriton was given to Mont S. Michel,* on the French coast where Normandy joins Britanny, in 1085.

I have before me "Two of the Saxon Chronicles parallel," edited and commented on by the Rev. John Earle, formerly Professor of Anglo-Saxon, Oxford, from which the two following remarkable passages are extracted for the respective years 1014 and 1099.

"1,014. And on this year, on Saint Michael's-Mass-Even, came that great sea-flood through widely this land, and ran so far up as never before not did, and submerged many towns, and mankind innumerable number," p. 151.

"1,099. This year also, on St. Martin-Mass-Day, sprang up that exceeding sea-flood, and so much to harm did, as no man not remembered that it ever afore did, and was that same day a new moon," p. 235.

Nobody seems to contend that the coasts of Cornwall have simply been abraded and cut back by the action of the sea during the Christian period. Nor does anyone appear to doubt that there has been a *subsidence.* Now there are 210 acres of land missing since 1086, and in 1099, thirteen years *after*, we have a record of a catastrophe which would fully account for the loss! Supposing, whilst travelling in a foreign country, we were to come suddenly upon a city in ruins, when we had personally experienced shocks of earthquake a short time before. How could we doubt that the earthquake was the cause of the ruins, and how can we doubt that the remarkable event of 1099 caused the loss of land by subsidence and the Insulation of the Mount?

Sir Henry de la Beche (Report on Cornwall, &c., p. 417, *et seq.*) says "submarine forests are so common that it is difficult not to find traces of them in the district at the mouths of all the numerous valleys which open upon the sea, and are in any manner silted up." . . . . It is well known that abundance of roots and stumps of trees have been found *in situ* for some distance on the south of the Mount. And no one seems to doubt that "the Mount was formerly five or six miles from the sea, and enclosed with a very thick wood," and that it was called in the Cornish language "the hoar rock in the wood."

I have before me the very interesting Prize Essay by Mr. John E. Thomas, F.G.S., written for the National Eisteddfod, held at Chester, 1866, upon the "Encroachment of the Sea between the River Mersey and the Bristol Channel."† Prof. Ramsay, who had the adjudication of the prize, says, "The result is so good, that I think this part of the Essay well worthy of the prize of 10*l.* and a medal." Mr. Thomas does not look further back than the year 520 for his sinkings in Cardigan Bay (p. 13). A perusal of the whole tract of twenty-four pages will well repay gentlemen who take an interest in this particular department of geology.

Jersey, Jan. 23 R. A. PEACOCK

Measurement of Mass

PROF. EVERETT's first letter contains the statement "Deschanel, in accordance with what has been till recent years an almost universal custom, employs a variable unit of force, and as depending upon this, a variable unit of mass, so that the number denoting the mass of one and the same body is diminished as the body is carried from the equator to the poles, and would increase up to infinity if the body fell to the centre of the earth."

I wished to point out that in making the standard pound a unit of force, by defining the place where it is to be used, we do not adopt a unit of mass which is variable, since if we take three times as much matter as gravitates with the unit force, we shall obtain the same mass at whatever point of the earth's surface the comparison is made. I cannot see that the adoption of this method necessitates any filing or loading of weights to suit change of latitude, since we invariably employ an ordinary balance and not a spring balance to effect our weighings.

Whenever it does become necessary to compare gravitating forces, we are obliged to fall back upon the use of the pendulum, whether we adopt the old system of standards or the new.

As a philosophical theory I am perfectly ready to admit that the standard pound is most appropriately considered as a standard of mass, but the employment of this standard in a text-book for the use of beginners seems calculated to lead to confusion.

If we refuse to commit ourselves to the absurdity of comparing the quantity of one kind of matter with the quantity of another kind of matter, I hardly see how *mass* is to be defined except by means of weight, and without, for the moment at least, employing weights as measures of force.

The assumption of a hypothetical force of gravity not dependent on latitude seems to stand on the same footing as the employment of a *mean solar day*; it is convenient, leads to no confusion, and is not unphilosophical. W. M. W.

Mount Etna

IT may interest the observers who have lately been in Sicily to hear that since their departure there has been a sad falling off in the appearance of Etna. The grand wreath of steam that used to roll out of the crater at such stately leisure that you could hardly detect any movement without close attention, suddenly ceased about three days ago, and left nothing more than a tiny wisp of smoke, rather suggestive of a cottage chimney than a volcano. I call it smoke because the colour became decidedly darker than it used to be, and the manner of its dissipation is different. Formerly, after issuing from the crater it used to assume true cloud forms, and lie about the mountain exactly like clouds: now, it diffuses itself as a thin veil over the sky; sometimes being traceable in a streak as far as the coast of Calabria. Its volume is perhaps a thousandth part of what it was last week. It issues in a distinctly spiral form, the wreath oscillating apparently from side to side of the crater; and sometimes there are little puffs of extra size, whilst at others the wreath is nearly sundered.

The date of the change cannot be very precisely given, although I have watched the mountain at all hours of the day for a week past, in hopes of getting a correct outline of it for pictorial purposes. The clouds only cleared off completely yesterday; but I observed that a change in the wreath had taken place as early as Friday the 13th January. The weather up to the 15th has been outrageously squally and rainy, but is now superb. Last night there was a magnificent display of zodiacal light, considerably brighter than we saw it at Augusta before the Eclipse, and distinctly traceable up to the zenith; the apex of the cone reaching to within about 10° of the Pleiades. It was brightest about 7 P.M., but was still visible at 10 P.M., when clouds shut it out.

Taormina, Sicily, Jan. 17 JOHN BRETT

Note on Chromosphere Lines

A VERY small but very bright prominence on the N.W. limb of the sun, observed at Xeres Dec. 21st, gave in addition to the ordinary protuberance bright lines the following—one below C at 656 Kirchhoff's scale precisely corresponding to an iron line in the solar spectrum—also 3, at 1601, 1605, and 1607, the reversal of a well-known group of *Chromium* lines. The latter, I believe, are new in prominence spectra.

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* Penny Cyclop., Art. CORNWALL.

† Published by Messrs. Spon, Charing Cross, price 1*s.*

C. A. YOUNG

Eozoön Canadense

I HAVE just observed that in your number for December 22 a correspondent revives some of the old but often refuted objections to the organic nature of Eozoon. As the mail closes in a few hours, and I have lectures in those hours, I cannot reply by this opportunity; but shall crave a small share of your space next week to show that the objections stated are unfounded; and to state what is now being done here in further illustration of this ancient and veritable fossil.

J. W. DAWSON

M'Gill College, Montreal, Jan. 16

THE battle for the *Eozoön Canadense* may be left to Messrs. Carpenter, Jones, King, Rowney, and other eminent microscopists, but perhaps an outsider may be pardoned if he asks some anomalies to be explained.

In the Ophytes of Bennabeola the mountain group in Connemara, or rather Yar-Connaught, Mr. Sandford proved the existence of the *Eozoon Canadense*, and his opinion was backed up by Mr. R. Jones and also, if I remember rightly, by Dr. Carpenter. There are acres upon acres of limestone in that country of the same age, and some of them on the same geological horizon as the Ophytes, Ophicalcytes, Ophimagnesytes and Ophidolomytes; yet, in no plaec, except where Ophyte or one of its varieties exist, has the Eozoonal structure been found. Furthermore, when the West Galway Ophytes are followed in depth they graduate into a Schistose-dolomyte that may be micaceous, felsitic, or quartzitic, and contains more or less calcyte; yet in these dolomytes there is no trace of the Eozoonal structure.

These rocks of Yar-Connaught are said to be of Lower Silurian (Cambro-Silurian) age, by Sir R. I. Murchison, Prof. Harkness, and other eminent geologists. In other parts of the world will be found square miles upon square miles of rocks, of the same geological age, often having inliers of limestones, yet in them there is no *Eozoon Canadense*, it only being found in a peculiar rock (pseudomorph-dolomyte) in this small tract of Lower Silurian rocks, in Yar-Connaught.

Yar-Connaught, Jan. 23 G. H. KINAHAN

IF my previous letter, as alleged by Dr. Carpenter, exhibits a complete misapprehension of the state of our knowledge of the above fossil, I cannot plead in extenuation a want of familiarity with the arguments he again brings forward in support of the organic theory. Had he, instead of explaining away imaginary difficulties, addressed himself to those that really exist, his reply would have possessed greater value. Let us examine how my objections have been met.

Firstly, then, Dr. Carpenter cannot affirm that any specimen of Eozoon has been obtained from unaltered rocks. He can go no further than to say that his best specimens are from rocks that have undergone the *least* metamorphic change. Thus it appears after all, that it is only a question of *degree* in metamorphism; and when we consider that Logan, Dawson, Sterry Hunt, and himself, in their original papers, constantly alluded to these Eozoonal rocks as crystalline, highly crystalline, of serpentine marble, &c., we are enabled to judge of the value of the diminutives "little" and "least," now used when it becomes necessary to the argument to soften down these expressions. Sir W. Logan, who is an authority on the subject, says: *—"Any organic remains which may have been entombed in these limestones would, if they retained their calcareous character, be almost certainly obliterated by crystallisation, and it would only be by the replacement of the original carbonate of lime by a different mineral substance, or by an infiltration of such a substance into all the pores and spaces in and about the fossil, that its form would be preserved." It would be strange indeed if, during the millions of years since the deposition of the Laurentian limestones, they had undergone no change, and notwithstanding Sterry Hunt's depositional views, the consensus of opinion is in favour of serpentine itself being a product of alteration.

Had Dr. Carpenter pointed out where serpentine pyroxene or loganite had been found in unaltered rocks, instead of dwelling upon the internal casts of foraminifera distinguishing the Greensand formation, his remarks would have been more relevant to the subject. These casts, it is well known, are in glauconite, a hydrous silicate of protoxide of iron and potash. Whether or not the silicates replacing the sarcode bodies of the foraminifera dredged up by Capt. Spratt in the Ægean, are the result of precipitation from sea water, caused by the decomposition of the sarcodic substance, is quite immaterial to the argument; but if, as is assumed, the chambers of Eozoön were filled in the same manner with serpentine, and this chemical reaction was necessary to its precipitation, how are we to account for the serpentine investing huge blocks of pyroxene, and the solid bands of the same mineral intercalated in the limestone? If, therefore, I admit the possible infiltration of certain silicates into the *body* of Eozoon—did such an animal ever exist—it is no help to those who favour the organic hypothesis. I have, however, neither affirmed nor denied such a possibility, as it is entirely outside of my line of argument.

As regards *hydrothermal* action, which it appears is objected to if called in to aid my theories, I may say it is a matter of indifference what the *agency* be so long as the *alteration* is proved.

It would take up too much of your space for me to go into the details of the "canal system," "nummuline layer," "chamber casts," "Stolon passages," "pseudopodial tubules;" and such is unnecessary, as Profs. King and Rowney have pretty well exhausted the subject, and, to my mind, have conclusively proved the existence of identical forms of purely mineral origin. If, as is alleged, the canal system always crosses the cleavage planes, and is never between them, such would appear to be correlative mineral phenomena, and tells against the organic hypothesis. I object, however, to a question of such wide bearing being settled solely on the authority of Dr. Carpenter as a microscopist. If others are wrong, let him demonstrate the fact, which his great experience will more readily enable him to accomplish.

If I have misconstrued the following passage into an admission which he now repudiates, I am ready to make ample apology; perhaps, however, he will explain to what the term "elsewhere" refers. After combating the notion that the *nummuline* layer can be precisely parallel in a purely mineral production, he says he is "prepared to maintain the organic origin of Eozoon on the broad basis of cumulative evidence afforded by the combination in every single mass of an assemblage of features which can only be *separately* paralleled elsewhere."*

Such is Dr. Carpenter's unbounded faith in Eozoon—though every hypothesis attempting to bring it into the category of organic beings is beset with difficulties—that he would not be surprised to find it existing now in the deep-sea bottom. There is, he says, no *a priori* improbability in such an event happening, and indeed there is not, for the persistence of types is one of the most remarkable of zoological facts. But as the area in which Eozoon is to be found is enlarged, and the duration of its time lengthened, our difficulties increase. If the infilling material of the chamber casts is due to substitution during decomposition, or to direct deposition as suggested by Sterry Hunt, there is no possible reason why we should not find Eozoon in some of the immense masses of unaltered limestone which still exist. I repeat that it has never yet been found in such rocks, but always in those that have been metamorphosed. If again serpentine is not a product of alteration, why do we not find it in unaltered rocks? The inference is obvious, they are correlative phenomena, and therefore Dr. Carpenter must pardon me if I decline at present to adopt his views. Still I am open to conviction, and will freely admit my error when, after some of his deep-sea dredgings, he brings home the modern Eozoon fossilised with a silicate, and when, in addition, it is discovered in an unaltered limestone fossilised with serpentine pyroxene or loganite.

T. MELLARD READE

Blundellsands, Liverpool, Jan. 9

The Eclipse Expedition

How about the Eclipse Expedition, which, I presume, you helped to sanction? I informed the public that it would prove a complete swindle, and so it has turned out. As long as such professional liars as the Astronomical Society are allowed to gull the nation, what chance is there of arriving at the truth?

JOHN HAMPDEN

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* "Geological Journal," No. 81, p. 48.

* Geological Journal, vol. xxii., p. 22[illegible]

Scientific Nomenclature

THE Kakapo or Night Kaka of New Zealand, *Strigops habroptilus*, described in NATURE at p. 190 as the *ground* parrot, is called the owl-parrot by Mr. Wood in the current number of the "Student." So long as both names are given there is no confusion, but it is otherwise if a full description is omitted; and I have to suggest that it is very desirable to adopt a uniform usage upon all occasions.

It is curious to notice the analogy between the words *psittakos* of ancient Greece and the *Kaka* of aboriginal New Zealand; as the Greek word has been traced to a Sanscrit origin it would seem that the New Zealand word must have originated since the Aryan descent upon India. A. H.

NATURAL SCIENCE AT CAMBRIDGE

THE following Lectures in Natural Sciences will be delivered at Trinity, St. John's and Sidney Sussex Colleges during the Lent Term, 1871. On Electricity (for the Natural Sciences Tripos,) by Mr. Trotter, Trinity College, in lecture room No. 11, on Tuesdays, Thursdays, Saturdays, at 10, commencing February 4. On Electricity and Magnetism (for the special examination for the B.A. degree), by Mr. Trotter, Trinity College, in lecture room No. 11, on Mondays, Wednesdays, Fridays, at 10, commencing Wednesday, February 1. On Chemistry, by Mr. Main, St. John's College, on Tuesdays, Thursdays, Saturdays, at 12, in St. John's College, Laboratory, commencing Tuesday, January 31. Instruction in Practical Chemistry will also be given. On Geology, by Mr. Bonney, St. John's College: (1) Palæontology, on Wednesdays and Fridays, at 9, commencing Wednesday, February 1; (2) Lyell's Principles of Geology, on Tuesdays and Thursdays, at 9, commencing Tuesday, January 31; (3) Elementary Lectures on Tuesdays and Thursdays at 11, commencing Tuesday, January 31. On Structural and Morphological Botany, by Mr. Hicks, Sidney College, in the College Laboratory, on Mondays, Wednesdays, Fridays, at 10, commencing Wednesday, February 1. On Physiology, by the Trinity Prælector of Physiology (Dr. M. Foster), at the New Museums, on Wednesdays, Thursdays, Fridays, at 11, beginning Wednesday, February 1. The Physiological Laboratory will be open for pratical instruction in Physiology daily.

It may be remembered that a year ago we pointed out some defects in the prospectus issued for the intercollegiate teaching of Natural Science by Trinity and St. John's Colleges, Cambridge. We are glad to find that, as will be found from the above statement, these have been rectified, and that by the appointment by Trinity College of Dr. Michael Foster as Prælector of Physiology, and by combining with Sidney College, and so availing themselves of the services of Mr. Hicks of that College, who obtained the first place in the Natural Sciences Tripos, as lecturer on Structural and Morphological Botany, the staff has been greatly strengthened, and the prospect of thorough teaching proportionately increased. The lectures are open to members of the other colleges upon payment of a small fee.

*OCEANIC VERTEBRATES**

SO far as concerns their distribution, animals may be divided into two classes, the tenants of the land and fresh waters, and the inhabitants of the ocean. In the one case their boundaries depend upon the form and extent of continents past and present; on the other, upon the corresponding limits of the ocean.

Little enough is as yet known with certainty about the general distribution of terrestrial animals; about those of the ocean we are still more ignorant. It is, therefore, with great pleasure that we have received Prof. Giglioli's notes on the vertebrated animals which were met with during the voyage of the Italian frigate *Magenta* round the world. The scientific command of this expedition was originally entrusted to Prof. Philippi of Turin. Upon his lamented death at Hong Kong, the author of the present treatise, we believe, succeeded to the post, and is now busily engaged in working out the results obtained by the expedition in every branch of natural history. The present memoir, although founded on observations made during the voyage of the *Magenta*, seems to be only incidentally connected therewith, and to have been prepared with reference to a competition for the Chair of Zoology and Comparative Anatomy at the Royal Institute of Practical Studies in Florence.

Professor Giglioli commences his remarks by treating of the oceanic fishes met with during his voyages. Although it is quite true, according to the popular idea, that the sea is full of fishes, it must be recollected that those that inhabit the mid-ocean are quite distinct from those that swarm round the coasts, and are not nearly so numerous. At the same time, many of them are remarkable for their brilliant colour, and are otherwise of special interest. It is difficult, says Professor Giglioli, to describe the beauty of the *Coryphæna hippuris* when first taken from the water: a thousand different tints of deep azure and golden yellow sparkle over its body, which, however, fade upon death with surprising celerity. Other oceanic fishes are the large *Thersytes*, various species of Tunny, the well-known Pilot fish (*Naucrates*), and the *Echeneis*, concerning which such marvellous tales are told by ancient writers. But, perhaps, the most attractive of all the group to the oceanic traveller are the flying fishes (*Exocetus*). Of this genus six species were met with during the voyage of the *Magenta*, each appearing to have a peculiar district of the ocean assigned to its range.

Of the class of reptiles which Professor Giglioli next speaks of, two orders only have oceanic representatives—namely, the Ophidia and Testudinata. Of the sea-snakes three species were met with belonging to the genera *Hydrophis* and *Pelamis*. This peculiar family of serpents was formerly supposed to be confined to the Indian Ocean; but it has of late years been discovered to extend its range over the Pacific, even up to the Gulf of Panama. Of the marine Turtles likewise three species, all well known, were observed.

The class of birds, which follows third in Prof. Giglioli's memoir, is much better represented on the so-called "desolate" ocean. Members of four large families of this class frequent the seas traversed by the *Magenta*, which were chiefly those belonging to the southern hemisphere. These are the Penguins (*Spheniscidæ*), the Petrels (*Procellariidæ*), the Gulls (*Lavidæ*), and the Pelicans (*Pelicanidæ*). A fifth great oceanic family, the Awks (*Alcidæ*), replaces the penguins in the Arctic Seas, and was not met with by Prof. Giglioli. The most abundant of all oceanic birds are, of course, the petrels and albatrosses, of the family Procellariidæ, many of which pass by far the greater part of their lives in mid-ocean. Upwards of forty species of this group are enumerated as having been encountered during the circumnavigation of the *Magenta*, amongst which are several supposed to be new to science, and which are provided with new names accordingly.

The mammals of the ocean, which the present memoir lastly treats of, belong to three very different orders: the Cetaceans, Seals, and Sirenians. Of these the first alone pass their whole existence in the salt sea. All the marine Carnivores, so far as we know, habitually resort to land, or at all events to ice, which in polar regions serves the same purpose, and of the few existing members of the Sirenia, one at least is rather an inhabitant of fresh water than of salt. Prof. Giglioli's observations are chiefly confined to the Cetaceans, of which thirteen or fourteen species were met with in various parts of the ocean traversed by the *Magenta*.

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* Note intorno alla distribuzione, della Fauna Vertebrata nell'oceano, presse durante un viaggio intorno al Globo, 1865—68, dal Professore Enrico Hillyer Giglioli. Firenze, 1870, 8vo 96 pp.

At the end of his memoir, Professor Giglioli gives a kind of journal of his voyage, containing the approximate latitude and longitude of the *Magenta* upon each day of her circumnavigation, and the various species of vertebrates observed or obtained, in parallel columns. Further assistance in tracing the distribution of oceanic life is afforded by the concluding chart, in which the track of the vessel is exactly delineated, and the names of the principal animals met with on each spot are likewise given.

It will be thus seen that even an oceanic voyage round the world, without counting the foreign lands touched upon, affords ample opportunities for the student of nature who has the use of his hands and eyes. In sending a single frigate on such a voyage, the poverty-stricken government of Italy does not hesitate to put on board a band of scientific observers. Does Mr. Childers do the same, when he sends his flying squadron round the world composed of the largest and most expensive ships which wealthy England can produce? We are ashamed to say he does not. Any application, even, for a free passage for a naturalist on such an occasion, would receive the stereotyped refusal, and the answer that "my lords" had no funds to devote to such purposes, and no space to spare.

P. L. S.

EARTHQUAKES AT FIUME DURING THE YEAR 1870

THE following list of earthquakes at Fiume during the year 1870 is sent by a correspondent at that town, to whom it was furnished by Prof. E. Stahlberger, of the Naval Academy, together with an extract from the Journal of Meteorological Observation kept at that Institution, condensing the remarks, &c., of the original, and omitting such details as are of mere local interest, as well as descriptions of the apparatus used in marking direction.

The year 1870 is not to be taken as a fair specimen of the frequency of earthquakes in this place. It was decidedly an exceptional year, both in this respect and with regard to the weather, which was unusually changeable throughout the whole twelve months, and during the autumn and December was marked by an abnormal amount of rain. About the time of the December full-moon a large halo of broad bands was noticed by our correspondent and others at about 8 P.M. On the 20th of May, at about 9 P.M., a very faint Aurora Borealis was visible.

No.	Day.	Time.	Duration.	Direction.	Remarks.
1	Feb. 28	0.22 P.M.	About 4 sec.	Apparently N.N.E. to S.S.W.	This beginning of the series of earthquakes came so unexpectedly that no apparatus was in readiness for marking the exact direction. The shock was violent; the oscillations succeeded each other with great rapidity.
2	Mar. 1	8.57 P.M.	5 sec	N.N.W. to S.S.E.	An alarmingly violent shock, the most violent that has occurred here for many years. Its commencement was sudden; it was accompanied by hollow, roaring sounds; the oscillations extremely rapid. Of all the shocks during the year, this was the most remarkable. It produced disastrous effects on the village of Clana, situate inland 2½ hours distant from Fiume. Out of the 150 houses there, 40 were rendered uninhabitable, and the rest were more or less injured. The walls mostly fell outwards, and no lives were lost.—(N.B. Not many walls fell, but very many were cracked the whole way down, and the houses left in so unsafe a condition that they were uninhabitable. I saw the village myself some time after the catastrophe, but before the repairs and rebuilding were made.—A. M. SMITH.)
3	Mar. 2	About 1.15 A.M.	....	N.W. to S.E. or N. to S.	Slighter as to violence.
4	Mar. 4	2.45 A.M.	5 sec.	N.W. to S.E.	Oscillations very rapid. On the preceding evening, at 7.5, a slight vibration of the earth was perceptible, lasting, with interruptions, two minutes. The same phenomenon was again observed at 11.14, about three hours therefore before the actual earthquake
5	April 28	3.25 A.M.	....	......	Two distinct shocks, separated by an interval of two or three seconds.
6	May 9	4.16 A.M.	....	......	Vibrations or tremblings were observed on April 29, at 7.30 A.M. and 2.28 P.M., and on May 4, at 2.30 A.M.
7	May 10	2.51 A.M.			
8	do.	9.19 A.M.			
9	do.	4 5 P.M.			
10	do.	5 56 P.M.	....	Vertical	This was one of the more violent shocks, and was also felt at sea.
11	May 11	1.30 A.M.	....	N.W. to S.E.	Slight.
12	do.	2.15 A.M.	....	do.	do.
13	do.*	2.50 A.M.	....	Vertical	Violent. It consisted of two distinct movements, separated by an interval of a few seconds, and was preceded by a thunder-like sound.
14	do.	4.15 A.M.	....	do.	Violent.
15	do.	4.30 A.M.	....	N.W. to S.E.	Slight.
16	do.	5 0 A.M.	....	do.	do.
17	do.	9 15 A.M.	....	do.	do.
18	do.	9 50 A.M.	....	do.	do.
19	do.	11 38 A.M.	....	do.	do.
20	do.	2.18 P.M.	....	do.	do.
21	do	During the night; time not specified	....	do.	do.
22	May 13	9 5 A.M.	....	......	do.
23	do.	11.19 P.M.	....	......	do.
24	May 14	0.50 A.M.	....	......	do.
25	May 16	10 (?) P.M.	....	......	A slight shock, preceded by a hollow groaning noise some short time previously.
26	do.	10.25 P.M.	....	......	Of short duration, but of some violence, consisting of two distinct shocks.
27	May 18	10.57 P.M.	....	......	Two slight shocks, following close on one another.
28	May 19	9.26 A.M.	....	......	Slight.
29	May 21	1.5 A.M.	....	......	Slight, preceded by a rolling noise of long duration, at 1.45 P.M. vibration,
30	May 23	8.25 P.M.	....	......	Slight.
31	do.	About 10.45 P.M.	....	......	do.
32	June 2	0.27 A.M.	3 sec.	......	Somewhat violent.
33	Sept. 25	5'.43 A.M.	4 sec.	......	Slight, with loud thunder-noise.
34	Oct. 13	4.30 P.M.	....	......	Slight.
35	Oct. 17	8.0 P.M.	....	......	A double shock, with loud noise.
36	Dec. 16	1.50 A.M.	....	N.E.	Moderately violent.

* On this day, between 2.50 and 9.15 A.M., there were 27 insignificant movements not specified in the list.—A. M. S.

THE GENESIS OF SPECIES[*]

IT is a remarkable illustration of the apparently fitful manner in which our knowledge of Nature increases, that the event which has probably been more fruitful than any other during the present century in inducing practical advances in the study of Natural History, was the promulgation of a pure theory, the publication, namely, by Mr. Darwin and Mr. Wallace, of the doctrine of the Origin of Species by means of Natural Selection. We say a pure theory, because the genesis of a new species is a phenomenon which never has yet, and probably never will, come consciously under the cognizance of man. We see forms of animal and vegetable life die out before our eyes, but their birth is not within our ken. As Mr. Darwin has pointed out, even should a new species suddenly arise, we have no means of recognising it as such. As a matter of fact, new plants and animals are constantly being discovered in all parts of the globe. Even in our own small and well-searched island, the additions within the last twenty years of more or less conspicuous flowering plants to our native flora are not inconsiderable; but no naturalist suggests any other interpretation of this, than that either they have been overlooked before, have been recently introduced from other countries, or that the seeds have been buried for ages in the soil. None the less, however, does it seem possible, or even probable, that we may eventually arrive at a correct solution of the problem by a rigorous induction from known facts.

So recently as the date of the publication of the first edition of the "Origin of Species," in 1859, Mr. Darwin wrote, "The great majority of naturalists believe that species are immutable productions, and have been separately created;" and the statement has been repeated in subsequent editions. We think, however, that it is impossible, at the present time, to sustain the correctness of this assertion. A writer in the "Botanische Zeitung"[†] has recently shown that there is some reason to believe that Linnæus himself, in later years, considerably modified the rigidity of his adherence to the doctrine which he laid down so decisively in his earlier writings:—"*Species tot numeramus quot diversæ formæ in principio sunt creatæ.*"[‡] During the eighteenth and the first half of the present century, however, it was only a few naturalists of more than ordinarily keen powers of reasoning and prodigious knowledge of Nature—Lamarck, Buffon, Geoffroy St. Hilaire, Owen (in his earlier writings), and some others—who were bold enough to enunciate the theory that species have been created by a process of evolution from earlier closely-allied forms of life. Since the publication of the "Origin of Species," we may say that almost the whole body of the younger naturalists of this country and of Germany—Von Baer, Huxley, Spencer, and Haeckel leading the way after Darwin and Wallace—have given in their adhesion to the doctrine of Evolution. It is only within the last twelvemonth that the evolutionists can claim so great an accession to their strength as that distinguished systematist, the President of the Linnean Society.[*]

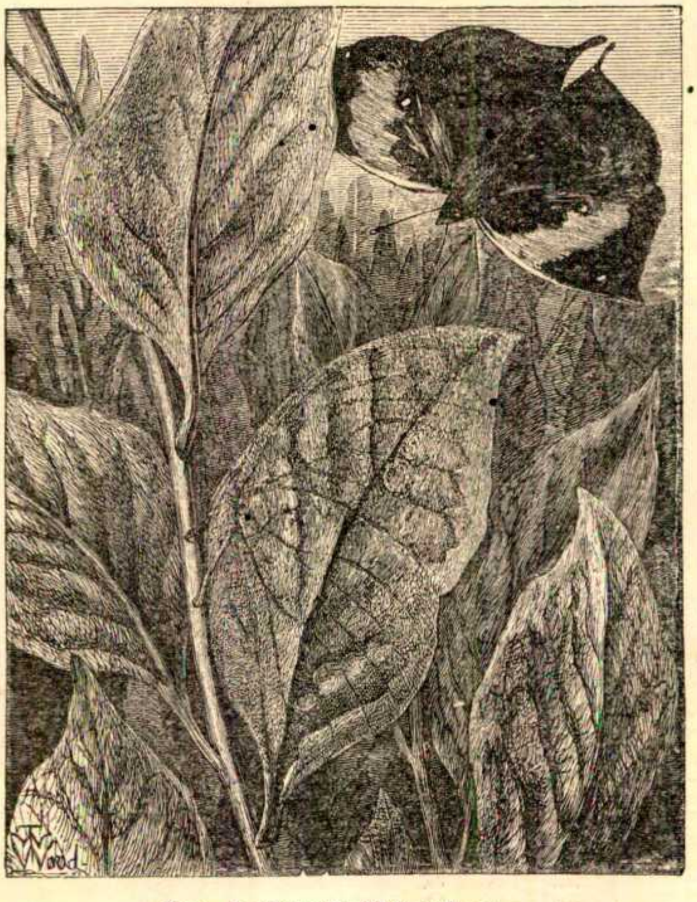

FIG. 1.—Leaf Butterfly in Flight and Repose.
The lowest apparent leaf on the stem is in reality the under side of the wing of the same butterfly which is represented in the upper part of the picture.

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[*] "On the Genesis of Species." By St. George Mivart, F.R.S. (London: Macmillan and Co., 1871.)

[†] Botanische Zeitung, Sept. 9, 1870.

[‡] Linn. Phil. Bot. Aphor. 157, p. 99.

[*] Bentham, "On the Species of Cassia," a paper read before the Linnean Society in 1870, but not yet published.

Along with this theory, these two writers introduced another, of the *modus operandi* by which this evolution is mainly or entirely effected, that of a process of Natural Selection from spontaneous variations. This doctrine was supported by an enormous array of facts, and by a brilliancy of argument which caused it at first to be as eagerly and generally adopted as the other. During the last few years, however, it may be said that a reaction has been setting in in an opposite direction, and attention has been widely called to difficulties in the way of the full adoption of the theory of Natural Selection, at first overlooked, the force of some of which has been admitted, with his usual candour, by Mr. Darwin himself. Some of these objections were pointed out by Mr. J. J. Murphy in his elaborate essays, entitled "Habit and Intelligence," published in 1869; on others Mr. Mivart dwells in the volume now before us, the most recent contribution to this department of literature.

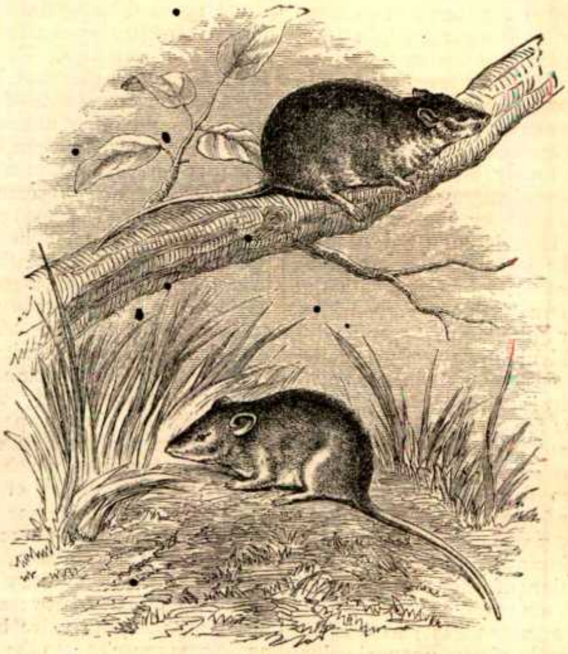

FIG. 2.—Upper Figure—Antechinus minutissimus (*implacental*). Lower Figure—Mus delicatulus (*placental*).

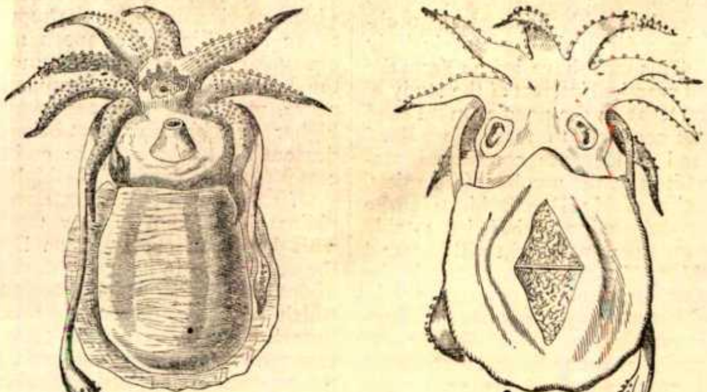

FIG. 3.—Cuttle-fish. Ventral and Dorsal Aspect.

A theory may be true, and yet may not be adequate. To take an illustration from the realm of mind. The believer in the doctrine of innate ideas will admit that the greater number of our conceptions are the result of habit, imitation, and training; he believes, however, that there are others which cannot thus be adequately accounted for, and which are innate and independent of subsequent education. In the same manner Mr. Mivart and those who think with him freely admit the potency of Natural Selection to have produced the greater number of the specific forms and organic structures we see around us; for the production of others they believe that it can be proved to be inadequate, and that we must look to some other innate principle for their formation. Mr. Mivart supports his arguments with so much cogency of reasoning, so great a knowledge of anatomical structure, and so complete an acknowledgment of the strength of his opponents' position, that they cannot be disregarded by any one interested in the subject. His objections are the more deserving of careful consideration, inasmuch as he states that he was himself by no means disposed originally to dissent from the theory of Natural Selection, if only its difficulties could be solved, but that he has found each successive year that deeper consideration and more careful examination have more and more brought home to him the inadequacy of Mr. Darwin's theory to account for the preservation and intensification of incipient specific and generic characters. It behoves, therefore, every Darwinian to satisfy himself that either Mr. Mivart's premisses or his line of argument is unsound.

The objections brought forward by the author are summed up as follows:—(1.) That Natural Selection is incompetent to account for the incipient stages of useful structures. (2.) That it does not harmonise with the co-existence of closely similar structures of diverse origin. (3.) That there are grounds for thinking that specific differences may be developed suddenly instead of gradually. (4.) That the opinion that species have definite though different limits to their variability is still tenable. (5.) That certain fossil transitional forms are absent which might have been expected to be present. (6.) That some facts of geological distribution supplement other difficulties. (7.) That the objection drawn from the physiological difference between "species" and "races" still exists unrefuted. (8.) That there are many remarkable phenomena in organic forms upon which Natural Selection throws no light whatever, but the explanations of which, if they could be attained, might throw light upon specific origination. If these objections are not new, they are at least sustained by new arguments. They are evidently of very unequal value. The third is very difficult of proof or disproof. The fifth may be true in our present state of knowledge, but would be very unsafe by itself as the basis of an argument. The first, second, and eighth are of greatest value, and are those which Mr. Mivart has most closely worked out.

Hitherto the attention of those scientific naturalists who have concerned themselves with the intricate problems of organic life, has been directed almost exclusively to the animal kingdom. This may have arisen from the greater attractiveness and practical interest of the study of zoology, or from the fact that in the popular mind (and we fear the error is not confined to mere "popular" writers) natural history and zoology are considered convertible terms. Be this as it may, the number of botanists, with the illustrious exception of Mr. Darwin himself, who look on their science in a philosophic spirit, is lamentably small. We believe, however, that more light will be found to be thrown on the problem of the genesis of species by a consideration of the phenomena of the vegetable than of the animal kingdom. Plants have less power of adapting themselves to new conditions, or of finding for themselves more congenial surroundings, than have animals. Their locality and their food are, as it were, prescribed for them by the circumstances of their birth; here, therefore, we might expect to find the rule of the survival of the fittest to reign supreme. We believe, however, it would be very difficult to substantiate any instances of species of plants being supplanted by other closely allied species, similar to those well-authenticated in the case of the rat and the cockroach. Plants when first artificially introduced into a new country undoubtedly frequently spread with extraordinary rapidity, to the destruction of weeds belonging to native races; but this is evidently not the mode in which species have supplanted one another in a state of pure nature.

Under Mr. Mivart's first head, he deals with the subject of Mimicry, contending that Natural Selection is incompetent to account either for the first or last stages of such wonderful instances of protective resemblance as that represented in our illustration. As this subject has been so recently discussed in these columns, we need not dwell upon it further than to remark, that we think the author could have supported his case with arguments of even greater force, had he extended his observations to the vegetable kingdom. The only object which it has been conjectured can be gained by a plant imitating a different species or a foreign structure, is to attract insects to assist in the distribution of its pollen. The most remarkable instances of the imitation by plants of foreign objects is in the case of the curious resemblance of the flowers of certain orchids to insects and other animals. One of the most singular of these is the well-known bee-orchis. But, as Mr. Mivart remarks, Mr. Darwin, in a course of observations extending over a series of years, has never seen a bee alight on this orchis. The most noteworthy resemblances again of plants *inter se* unconnected with organic affinity, are not in the flowers, where they might be useful, but in the leaves, or in the whole stem and foliage. It is difficult to conjecture any advantage that is gained by the close resemblance between an African Euphorbia and a South American Cactus, the imitation being carried out in the most extraordinary manner throughout the vegetative organs, the flowers being, of course, totally unlike.

But besides these superficial resemblances, there are also analogies of organic structure in different classes of the animal kingdom, which Mr. Mivart holds to be equally opposed to the theory of Natural Selection. He refers especially to the existence of the higher organs of sense, as the eye, in at least three distinct and independent lines of descent, the Mollusca, the Annulosa, and the Vertebrata, an objection already pointed out by Mr. Murphy; to the resemblance between the shells of certain Mollusca and Crustacea, the valve being moved in each case by analogous muscles; to the analogy between the different families of Marsupials and the different orders of Placental Mammals; and to numerous other instances. These might be supplemented in the vegetable kingdom by the similarity in the mode of opening of the anthers in Berberidaceæ and Lauraceæ, or the extraordinary resemblance of certain Conifers to flowerless plants. The wood-cuts which we give illustrate the remarkable resemblance between an ordinary European mouse and an Australian marsupial (Fig. 2); the structure of the cuttle-fish with the brain, cartilaginous cranium, and complex auditory nerve, presenting so many similarities to those of the higher Vertebrata, and yet belonging to a different line of descent (Fig. 3); and the curious bird's-head-like processes found in some of the higher Polyzoa (Fig. 4).

Of exceptional structures, none is more interesting in a philosophical point of view than the neck of the giraffe. This has been explained on Darwinian principles from the occurrence in its native country of occasional periods of drought, during which those giraffes only have survived which had the power of reaching somewhat higher branches of the trees; and this peculiarity, being advantageous, was propagated, and continually augmented during each period of drought by the process of Natural Selection. To this Mr. Mivart objects, firstly, that if this explanation is correct, many other African animals, which have no greater power of endurance or of migration than the giraffe, ought to have elongated necks; and secondly, that in the intervals between the droughts the long neck would be a positive disadvantage, as requiring a greatly increased size and strength of muscles to support it, and would, consequently, be lost before the next drought set in. To take another instance of the commencement of an organic structure which is universal in all the higher classes of animals; there is scarcely anything more inexplicable than the separation of the sexes, if we suppose animals with distinct sexes to have originated by the process of Natural Selection from those simple forms which propagate by cell-multiplication or spontaneous fission.

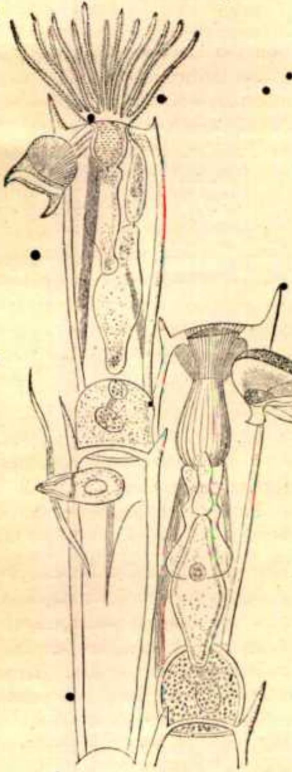

FIG. 4.—A Polyzoon with Bird's-head Processes.

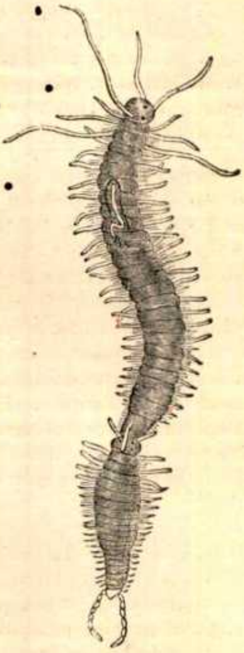

FIG. 5.—An Annelid dividing spontaneously.
(A new head having been formed towards the hinder end of the body of the parent.)

We have not space to enumerate all our author's arguments, for which we must refer our readers to the volume itself, wherein even those who differ from his conclusions will find a mine of physiological information and ingenious speculation. Nor can we do more than allude to the theological portion of the work, wherein he ably defends the doctrine of Evolution against both the *odium theologicum* and the *odium antitheologicum.*

The present state of the argument we take to be this :—The theory of Natural Selection, in the hands of Mr. Darwin and Mr. Wallace, afforded a simple, a beautiful, and a valid solution of the origin of a large number of the organic phenomena by which we are surrounded ; by many disciples of Mr. Darwin it has been assumed, perhaps too rashly, as adequate to account for the entire evolution of all the existing forms of animal and vegetable life from one or a few primordial germs. To this idea, so seductive in its very simplicity, a number of more or less cogent objections have now been urged. It is possible that on still closer examination, these objections will be found to break down ; but in the meantime we must suspend our judgment ; and in order to save defeat, the next move must be made by the advocates of Natural Selection, a *primâ facie* case against them having at all events been made out. Mr. Mivart has no counter theory to propose, beyond a belief that there exists in all organic life an innate power analogous to intelligence, which controls their actions as reason does those of men. Should the inquiries which are now being energetically pursued on every side result in our acquiring more accurate knowledge of such a force, it will be safe to predict that to it will then be ascribed a more easy and natural solution of many phenomena which we are now forced to attribute to Natural Selection.

ALFRED W. BENNETT

NOTES

THE Royal Commission on Scientific Instruction and the Advancement of Science is about to resume its sittings, and meetings will be held at 6, Old Palace Yard, Westminster, on Thursday, February 9 ; Friday, February 10 ; Monday, February 13 ; Tuesday, February 14, at 11.30 A.M.

IN the recent changes which have taken place in the Royal Mint by the deaths of Professors Graham and Miller, Dr. Stenhouse, F.R.S., lost his position as Non-resident Assayer to the establishment. Several of our leading chemists, including Sir Benjamin Brodie, and Professors Williamson, Frankland, and Odling thought that this was an opportunity for recognising the services which Dr. Stenhouse had rendered to chemistry by his numerous researches, and they requested Dr. Lyon Playfair, M.P., to bring his claims before the Premier. We have now the gratification to announce that the representation has been successful, and that Her Majesty has directed that a pension on the Civil List of 100*l.* should be given to Dr. Stenhouse "in consideration of his scientific attainments."

THE shelling of Paris has been disastrous to M. Desnoyers, the librarian of the Museum, who lost his son by it. The celebrated Abbé Moigno, editor of *Les Mondes*, was slightly hurt by the breaking of a glass, from an explosive shell. The private apartment of M. Milne-Edwards was visited by a shell which has done much harm to the valuable furniture. Another shell penetrated into the Gallery of Zoology and caused much damage to the glass-work ; and again another into the Gallery of Mineralogy, in the very place where M. Daubrée performs his experiments on artificial meteors ; but happily it had exhausted its force, and did but little harm. Many instances of this description have occurred, where shells were sent to their utmost range, and did not explode under such circumstances. The reverse has however been observed, for one shell falling in the Seine with great force, the water offered resistance like a solid body, and explosion took place : the effect was curious. A shell fell in the courtyard of the Collège de France when M. Levavasseur was lecturing on political economy, and the young professor was not disturbed by the projectile, portions of which broke through the windows. He continued lecturing, saying, "Those who fear such things have only to leave the place."

THE addition to the building for the Museum of Comparative Zoology at Cambridge, at an expense of upwards of 60,000 dollars, is rapidly going on. Prof. Agassiz has returned to Cambridge with restored health, and with new plans for the enlargement of his museum. Prof. O. C. Marsh, of Yale College, has just returned with his party from the Rocky Mountains.

THE *Indépendance Belge*, in its number for January 9, publishes an extract from the *Comptes-rendu de l'Académie des Sciences*, in which M. Elie de Beaumont describes the experiments used in directing the course of the balloon which fell at Rheims. In accordance with what was said in one of our recent numbers, M. Elie de Beaumont, who had witnessed the ascent, explained to his colleagues that the wind was bearing to the East, but that owing to the deviation the balloon would reach Switzerland! He was, however, most unfortunate. The captain of the directing balloon arrived recently at Lille. Experiments directed by M. Mangon have proved that a balloon 90,000 cubic metres measurement requires forty-two foot-pounds to move with a rapidity of a yard per second, which is a very important result for future experiments. The mass of gas and balloon is about three tons.

MR. J. HOPKINSON, the Senior Wrangler of 1871, is a native of Manchester. He was educated at Owens College; and at Easter 1867 obtained the Senior Mathematical Scholarship at Trinity, after open competition. In 1868 he also obtained the Sheepshanks Astronomical Scholarship. In 1867 he was awarded the exhibition in Chemistry and Natural Philosophy, at the first examination for the B.Sc. degree at the London University; and at the second examination for the same degree in the following year he carried off the Scholarship in Mathematics and Natural Philosophy. Mr. Jas. W. L. Glaisher, the Second Wrangler, is the son of Mr. Jas. Glaisher, F.R.S., director of the Meteorological Department at the Greenwich Observatory. He received his education at St. Paul's School, where he gained the first mathematical prizes in 1865, 1866, and 1867. On leaving school he entered Trinity College with a Senior Campden Exhibition. He is the author of a paper read before the Royal Society in March 1870, on the numerical value of the sine-integral, cosine-integral, and exponetical-integral.

THE Delegate Government of Bordeaux has established a Scientific Commission, presided over by M. Marié Davy, the meteorologist, and having amongst its members the Professor of Analysis at the Polytechnic School of Paris, M. Silberman, Head of the Physical Laboratory at the Collège de France, and some local scientific celebrities.

M. LEVERRIER is said to be hiding in a country place near Marseilles, assuming a false name, and living like a private man with one of his intimate friends.

M. THÉNARD, a member of the French Institute, who had retreated to his home on the Côte d'Or, was taken prisoner and sent to a fortress in Germany as a hostage. The President of the French Institute has protested against the arrest. M. Thénard is the son of the celebrated Professor of Chemistry, and belongs to the agricultural section. He is a landed gentleman of considerable property.

THE elephants and other animals of the same description at the *Jardin des Plantes* were sacrificed to the necessities of the war. Members of the French Institute were present on the spot in order to witness the effects of the shots on the huge brutes, and some parts of the body were set apart for careful dissection. M. Milne-Edwards has taken advantage of the circumstance to prepare a paper, which must have been already presented to the French Institute, and which will create a sensation in the scientific world.

DURING last session the American Congress made an appropriation of 50,000 dollars for Arctic exploration, with the promise that the scientific operations of the expedition were to be prescribed by the National Academy of Sciences. Captain Hall was appointed by the President of the United States to command the expedition in question, and a commission of the National Academy, recommended by Professor Henry, is to act in concert with him, and to prepare a manual of scientific inquiry for the use of the expedition, "which will, undoubtedly," says the *American Naturalist*, "interest a large circle of readers when published."

MR. A. HYATT has been appointed Professor of Palæontology at the Massachusetts Institute of Technology. Mr. E. S. Morse has been chosen Professor of Comparative Anatomy and Zoology at Bowdoin College, and has been appointed lecturer in the same branch at the Maine Agricultural College. Dr. A. S. Packard, jun., editor of the *American Naturalist*, is to lecture on Economic Entomology at the same institution. Mr. B. H. Emerson has recently been elected Professor of Geology at Amherst College, the chair filled for so many years by Dr. Edward Hitchcock, sen.

THE *American Naturalist* states that Dr. Hagen has recently returned from Europe, having purchased, through funds supplied by a lady in Boston, for the Cambridge Museum, a Parisian collection of weevils of great extent and value. He has also brought over his own unrivalled collection of Neuroptera. Its presence in the United States is most fortunate for this department of entomology. The same journal gives us the following additional items of entomological intelligence:—Congress is about to print an entomological report by Townend Glover, the entomologist of the Agricultural Department. It will form an exceedingly useful work, and will deserve the widest circulation. Mr. P. R. Uhler, of Baltimore, has ready for publication by the Boston Society of Natural History, descriptions of the Hemiptera of the Harris entomological collection. Gradually the unpublished results of the labours of Dr. T. W. Harris are being given to the public.

MR. J. A. M'NIEL, who has made two expeditions to Central America, is now in Philadelphia preparing for a third archæological excursion to Nicaragua.

IN the number of the *Quarterly Journal of Education* for January is a short article on school books, in which occur the following remarks, which we commend to the notice of those interested:—"The vast majority of elementary works are written specially as aids to assist the student in cramming a certain specified subject, so that he shall be able to pass some particular examination. Now every examination differs to a certain extent from any other examination, and text-books are therefore required to point out and elucidate these differences. For example, one is told that it is almost imperative to use Buckmaster's Chemistry in preparing for the science examinations, whilst Miller's, Williamson's, Gill's, or Barff's works upon the subject, although admitted by competent authorities to be the best of their kind, are kept in the background. On the other hand, in preparing for the London University examinations, it is better to use the latter works. . . . Frequently, again, an examiner writes and publishes a text-book, and, of course, he has a predilection for the peculiarities of his own offspring; and, by so framing his questions that a knowledge of *his* book is necessary, he increases its sale, and thereby the balance at his banker's." Without subscribing to the inuendo contained in the last sentence, we think the *Journal* here points out a weak place in our examination-system. Great complaints are made—and these do not come from rejected candidates—that the mode in which questions are sometimes put at these examinations renders them impossible to be answered clearly by any student who has not been trained in the particular school of the examiner, though he may possess a competent knowledge of the subject. This is a great evil, and ought to be looked into.

We have received the first four numbers of what seems likely to be a very useful series of publications issued by authority of the University of New Haven, Connecticut, and termed "The University Series." Two of these are reprints of the well-known English treatises:—Prof. Huxley "On the Physical Basis of Life," and Dr. Jas. H. Stirling "As Regards Protoplasm, in reference to Prof. Huxley's Essay." The remaining two comprise Prof. Cope's essay "On the Hypothesis of Evolution, Physical and Metaphysical;" and Prof. G. A. Barker of Yale College "On the Correlation of Vital and Physical Forces."

A RECENT number of *L'Illustration Horticole* contains an interesting paper on the Botanic Gardens of Kew, by M. André, prefaced by some details regarding similar establishments in Europe. From this it appears that the first was established at Padua in 1545, followed by that of Pisa; those of Leyden and Leipzig date respectively 1577 and 1579. The Montpellier garden was founded in 1593, that of Giessen in 1605, of Strasburg in 1620, of Altorf in 1625, and of Jena in 1629. The Jardin des Plantes was established in 1626, and the Upsal Garden in 1627; that of Madrid dates from 1763, and that of Coimbra from 1773. At the end of the eighteenth century, according to Gesner, more than 1,600 kindred establishments existed in Europe. England comes late in the list, the Oxford Garden not having been founded until 1632, and long remaining the only one in the kingdom.

We learn from the *Scottish Naturalist* that the work of fitting up cabinets for the reception of the Natural History collection in the Paisley Museum is rapidly approaching completion; and that the opening is expected to take place shortly. The geological and botanical specimens are mostly, if not all, British, and will form a valuable reference collection for students. The large and valuable reference library will contain a choice selection of scientific works.

The revenue cruiser *Moccasin*, according to the *Technologist*, has been supplied with a new marine drag, that is, one of those substitutes for an anchor which will, it is said, effectually prevent a ship's drifting even in the heaviest weather. The success of experiments made and reported to the Treasury department has caused a contract to be made with a New York firm for the construction of several of these marine drags; and it is the intention to supply all vessels in the Revenue service with them. To vessels off a lee shore, in stormy weather, the marine drag must be a most desirable protection against shipwreck.

Since the American publishers abandoned the reprinting of the *Chemical News*, our American friends, says the New York *Technologist*, "have no resource but to subscribe for the original London edition. We do not regret this. What they lose in cost they more than gain in neatness and accuracy, and also in the fact that they do not have to wait a month for their news."

Mr. Andrew Murray has published in the *Field* some remarks upon eight samples of honey which have been forwarded by M. de Solsky, of the Agronomic Museum of St. Petersburg, to the South Kensington Museum. The honey was produced by bees fed in districts where there was a great preponderance of the following plants:—*Reseda odorata* (mignonette), *Tilia parvifolia*, *Dracocephalum moldavicum*, *Carduus nutans*, *Fagopyrum esculentum* (buckwheat), *Epilobium angustifolium*, *Echium vulgare*, each plant being represented by one of the samples. The eighth consisted of honey flavoured by the herbs of the Steppes of Central Russia, and this was the best of all in taste. Next came that from the mignonette, then that of the lime-tree; the buckwheat honey being beyond question the worst.

One of the most interesting and important trees of Sumatra is the Camphor-tree, *Dryobalanops camphora*. This camphor attracted the attention of the earliest voyagers, and was then, as it is now, an important article of commerce with China and Japan, the people of those countries attributing to it extraordinary virtues and paying a high price for it. The tree grows to a height of 100 or 130 feet, and forms a trunk 7 to 10 feet in diameter. The quantity of camphor contained in the trunks is very unequal, the young trees appear to contain little or none. It is said that, on an average, about nine trees are required to produce 100 lbs. weight of crystallised camphor. It is obtained by cutting down the tree and dividing the wood into small pieces, in the divisions of which the camphor is found. It differs in the form of its crystals from the camphor of commerce, is harder, more brittle, and does not so readily condense. Great quantities are used by the Bataks for the preservation of the corpses of their chiefs. The trees are spread over a portion only of Sumatra and Borneo, and generally occur in localities into which commerce and civilisation have as yet but little penetrated. Notwithstanding the continued destruction of the trees, for the sake of procuring the camphor, no means are taken for the future preservation of the species. This camphor is seldom seen in this country, except in museums. The Chinese eagerly buy it in preference to the ordinary camphor—their own produce—which they send in such large quantities into the European markets.

The habit of branching among palms, though constant only in the Doum palm (*Hyphæne thebaica*) of Egypt, is not uncommon in other genera, and notably amongst Palmyras (*Borassus flabelliformis*) and Cocoa Nuts, (*Cocos nucifera*), figures and descriptions of which have been published in the Linnean Society's Transactions. According to a correspondent in Ceylon, branching Cocoa Nuts, Palmyras, and Arecas, are to be seen there in plenty, besides other curious freaks of the Cocoa Nut, such, for instance, as the growth of two or three trees from one nut. In the Racket Court at Colombo, we are told that five trees are now flourishing (or at least were at the time the letter was written) which have proceeded from the same nut. Originally there were eight in all, but three have died, probably by the nourishment being drawn away from them for the sustenance of the other five.

The use of the fruits of *Taughinia venenifera* in Madagascar as an ardent poison is of great antiquity, and is one of the still remaining superstitious customs ardently believed in by savage nations. The system of administering the poison has been often told, but the following account from a private letter of an eye-witness differs in so many points from those we have before read that we give it entire. The fruit was taken, bruised, and boiled whole. A fowl was boiled, and the broth set aside. Three pieces of the skin of the fowl were cut and put into the broth; a cupfull of poison was first administered, followed by another of the broth containing the three pieces of skin. If vomiting did not speedily set in, the poison soon killed, but if it did, it was kept up by constant exhibition of the broth and warm water until the three pieces of skin were ejected. Should the skins obstinately remain, it was held as evidence of guilt, and another dose of the poison was administered.

Some discussion has taken place in Indian circles on the authenticated, but unprecedented, fact of a tigress having been shot near Octacamund, by Colonel Christie, while she was forty feet from the ground in a tree. Many observers state that a tiger when in danger, and at the foot of a tree, does not take to a tree, and in the inundations only gets on to the lower branches. A leopard does take to a tree.

HEAT SPECTRA

I NOTICE in NATURE, vol. i., p. 28, an account of some very important researches on Heat Spectra, made by the late Professor Magnus; and I am gratified to think that some observations which I made on the subject in the years 1858 and 1859 were confirmed by this eminent German philosopher.

In a paper read before the Royal Society of Edinburgh in 1858, I showed that rock-salt absorbs heat radiated from rock-salt in larger quantity and more powerfully than other kinds of heat; and also that the amount of absorption of rock-salt for heat increases with the thickness of the absorbing plate. These are the fourth and fifth results of Professor Magnus. His next result is very interesting, namely, that the high diathermancy of rock-salt does not depend on its small absorptive power for the different kinds of heat, but on the fact that it only radiates, and consequently only absorbs, heat of one kind; while almost all other bodies at the temperature of 150° F., emit heat which contains only a small fraction or none of those rays which are given out by rock-salt.

Certain experiments which I made in 1859 lead me to think that Professor Magnus was quite justified in his conclusion that the heat radiated and absorbed by rock-salt is a peculiar kind of heat. These experiments, which are described in the Transactions of the Royal Society of Edinburgh, are as follows. I tested the quality of the heat radiated by rock-salt at 212° F. by transmitting it through three different screens:—

α A screen of mica.
β One of mica split by heat.
γ One of glass.

It was found that a mica screen, which passed about 31 per cent. of ordinary lampblack heat, passed only 18 per cent. of rock-salt heat; or if we call the proportion of black heat passed by the mica 100, that of rock-salt heat will be 58. Again, it was found that while 20 per cent. of lampblack heat passed through a screen of split mica, the proportion of rock-salt heat transmitted through the same screen was only 15½ per cent. These numbers are to one another as 100 to 76.

Lastly, with respect to a glass screen, calling the proportion of lampblack heat which passed 100, that of the rock-salt heat which passed the same screen was 57.

On these results I remark as follows:—It is already well known that rays of great refrangibility, or small wave length, pass through glass or mica more readily than those of an opposite character. The difficulty with which rock-salt heat penetrates these substances as compared with ordinary heat, might therefore lead us to infer that the wave length of this heat is greater than that of ordinary lampblack heat.

If we now look to the relative transmission of the two descriptions of heat through mica split by heat, we see that the facility of transmission is yet in favour of ordinary heat, but not so strikingly as with a screen of common mica. This will be seen from the following table:—

Nature of Screen	Transmission of Ordinary Heat at 212°	Transmission of Rock-salt Heat at 212°
Mica	100	58
Mica split by heat	100	76

Compare this with the following table, deduced from the results given by Professor Forbes:—

Nature of Screen	Transmission of Heat from Blackened Brass at 700° F.	Transmission of Black Heat at 212° F.
Mica ·015 inch thick	100	52
Mica split by heat	100	64

From a comparison of these two tables, it will be seen that, as tested by the two substances, mica and mica split by heat, rock-salt heat at 212° F. bears to ordinary heat of that temperature a relation similar to that which ordinary heat at 212° bears to heat at 700°; that is to say, that just as heat of 212° has a greater wave-length than heat of 700°, so rock-salt heat at 212° has a greater wave-length than ordinary heat at that temperature. And the *surface stoppage* produced by splitting the mica, telling most powerfully upon heat of high temperature, or small wave-length, while the *stoppage by substance* is in the opposite direction, we see how the one effect tends, to a certain extent, to neutralise the other, rendering the proportions of different kinds of heat passed by split mica more nearly alike than those passed by ordinary mica. In connection with these remarks I may state that neither the radiation nor the absorption of a plate of rock-salt is sensibly influenced by roughening its surface with emery paper.

All these experiments concur in showing that heat from rock-salt possesses very great wave-length, and probably heat from a thin plate of this substance, at a low temperature, may be found to possess a greater wave-length than any other description of heat which can be exhibited.

The observations of Professor Magnus with respect to the nature of the heat from potassium chloride are very interesting; unfortunately I did not make any experiments on this substance, but I did on some others in the shape of powder.

I found that the comparative radiation at 212° was as follows:—

From lamp black	100
Alum in powder	100
White sugar	98·7
Sulphate of potash	88·1
Nitrate of potash	86·7
Table salt	83·1

The experiments of Prof. Magnus on the reflection of heat are also of the very greatest importance, and they strengthen the evidence (already overwhelming) in favour of that view which regards light and heat as varieties of the same agent differing in nothing except wave-length.

BALFOUR STEWART

ON THE METHOD OF ASSAYING SILVER AS CONDUCTED IN THE INDIAN MINT*

BY DR. H. E. BUSTEED, OFFG. ASSAY MASTER

THE method of assaying silver, as now in use in H.M.'s Indian Mints, is one peculiar to them; it was introduced into the Calcutta Mint about the year 1850, and thence extended in course of time to those of Bombay and Madras.

It has been favourably reported on and described more or less in detail as an official duty by various assay officers to local Mint authorities in India; but beyond this it would appear that no attempt has been made towards giving publicity to the practical working of the process, or to making generally known the laboratory details of this method of assay.

It has been suggested to the writer that some such attempt now would be not only interesting but useful, as after twenty years' experience of it, the assay offices in the Indian Mint must be in a position to assign its true value to a method which has been used for the assay of an immense importation and coinage of silver bullion. To render it more generally intelligible, and to show wherein the process about to be explained contrasted with those in more general use, Dr. Busteed very briefly adverted to the principles on which those processes depend for their results, omitting details and technicalities. In modern acceptance, the principal duty of an assayer is to ascertain the proportion of the precious metals present in any sample of mixed metal submitted to him for examination, so that from the result of his investigation the proper value may be assigned by calculation to the mass which the sample is supposed to represent.

This the assayer effects by separation of the precious metals from the coarser ones. The most ancient means of effecting this was by the method of *cupellation*. He explained the principle of this method, what skill and experience it required on the part of the operator, and how it still fell short of accuracy in its results.

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* From the Proceedings of the Asiatic Society of Bengal.

Its shortcomings led to the invention of another process by Gay-Lussac, known as the volumetric, or humid, method, which is much more accurate, and is now practised very generally on the Continent. Its principles were briefly glanced at. Its introduction, however, into the Indian Mints was not considered desirable by their assay officer for certain reasons, a few of which were given. The method of cupellation, therefore, being not accurate enough for the purposes of buying and selling bullion, and that by the French process being considered not well suited to Indian Mints, it became necessary to look out for and introduce into the Mints of this country a process more likely to answer all the ends in view.

This object was attained by the adaptation and introduction of the process now in use, viz. the "Chloride process of assaying silver.' Hitherto it had never been resorted to, except on a very small scale. Assayers appear to have shrunk from the manifest difficulties of manipulation in collecting, drying, and weighing the precipitated chloride of silver. The credit is due to Mr. James Dodd, a former Assay Master of the Calcutta Mint, of having so simplified, modified, and systematised the details of this method, as to render its application to the assaying of silver on a large scale easy and accurate. The principles, and an outline of the details of the process were then given, an understanding of some of the chief appliances and steps in the manipulations being assisted to by suitable photographs. The system of weights in use, and the quantity of the sample taken for assay, were also explained, as well as the points wherein this system might fairly be considered better suited to a Mint in India than the other methods.

In conclusion Dr. B. alluded to the vast amount of silver bullion which this process enabled the assay officers of the Indian Mints to deal with confidently and accurately during the past fifteen years. In one year alone, that of 1865-66, the importation of silver bullion reached to the immense amount of over fourteen millions sterling, so putting to a crucial test the system of assay used for its valuation.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 12.—"On Fluoride of Silver," Part II., by George Gore, F.R.S. An exhaustive account of the behaviour of argentic fluoride in vessels of platinum, carbon, and various fluorides in contact with chlorine, bromine, and iodine at various temperatures. When argentic fluoride is completely decomposed by chlorine in platinum vessels at a red heat, the reaction agrees with the following equation :—

$$4\,\mathrm{Ag\,F} + 4\,\mathrm{Cl} + \mathrm{Pt} = 4\,\mathrm{A\,G\,Cl},\ \mathrm{Pt\,F_4}.$$

Vessels of cryolite and of fluor-spar were found incable of retaining argentic fluoride in a melted state. Other vessels were also made by melting and casting various mixtures of earthy fluorides at a high temperature; and although forming beautiful products, probably capable of technical uses, they were not capable of retaining silver fluoride in a state of fusion. Numerous vessels were also made of seventeen different fluorides by moulding them in the state of clay and baking them at suitable temperatures; these also were found incapable of holding melted fluoride of silver. Argentic fluoride was only superficially decomposed by chlorine at 60° Fahr. during thirty-eight days. When heated to 230° Fahr. during fifteen days in a platinum vessel in chlorine, it was very little decomposed. Chloride of silver heated to fusion in a platinum vessel in chlorine corroded the vessel and formed a platinum-salt, as when fluoride of silver was employed. An aqueous solution of argentic fluoride agitated with chlorine evolved heat and set free oxygen, in accordance with the following equation :—

$$8\,\mathrm{Ag\,F} + 8\,\mathrm{Cl} + \mathrm{H_2O} = 5\,\mathrm{Ag\,Cl} + 3\,\mathrm{Ag\,Cl\,O} + 8\,\mathrm{H\,F} + \mathrm{O},$$

or

$$7\,\mathrm{Ag\,Cl} + \mathrm{Ag\,Cl\,O_3} + 8\,\mathrm{H\,F} + \mathrm{O}.$$

Dry hydrochloric acid gas completely decomposed argentic fluoride in a melted state, but only acted upon it superficially at 60° Fahr. A saturated aqueous solution of argentic fluoride was not precipitated by chloric acid. Perfectly anhydrous fluoride of silver was only superficially decomposed by contact with bromine in a platinum vessel during thirty-six days at 60° Fahr., or during two days at 200° Fahr. At a low red heat in vessels of platinum argentic fluoride was completely decomposed by a current of bromine vapour, a portion of its fluorine being expelled and a portion corroding the platinum and forming an insoluble compound of fluoride of platinum and bromide of silver. In carbon boats at the same temperature the whole of the silver-salt was converted into bromide, the boat being corroded and the fluorine escaping in chemical union with the carbon. The action of bromine on an aqueous solution of argentic fluoride was similar to the action of chlorine. A solution of argentic fluoride yielded copious precipitates both with hydrobromic and bromic acids. Under the influence of a temperature of 200° to 600° Fahr. in closed platinum vessels, iodine very slowly and incompletely decomposes argentic fluoride without corroding the vessels, and produces a feeble compound of argentic iodide, fluorine, and iodine, from which the two latter substances are expelled at a red heat. At a red heat in platinum vessels, iodine produces argentic iodide, and in the presence of free argentic fluoride corrodes the vessels in consequence of formation of platinic fluoride; iodine and fluorine pass away together during the reaction. In vessels of carbon at the same temperature, argentic iodide is formed, the vessels are corroded, and a gaseous compound of fluorine and carbon is produced. By treating an aqueous solution of argentic fluoride with iodine, similar results are produced as with bromine and chlorine; a similar solution yields copious precipitates both with hydriodic and iodic acids. A mode of analysis of iodine is also fully described in the paper. A known weight of iodine was dissolved in absolute alcohol, a strong solution of argentic nitrate of known strength added to it in proportions at a time with stirring until the colour of iodine exactly disappeared. The mixture was evaporated, the free nitric acid expelled by careful heat, and the residue weighed. The residue was then heated to fusion to convert the iodate of silver into iodide, and again weighed. Two experiments of this kind yielded accurate results, and the process was easy and expeditious.

January 19.—"On the Structure and Development of the Skull of the Common Frog (*Rana temporaria*)," by W. Kitchen Parker, F.R.S. At the close of my last paper, "On the Skull of the Common Fowl," I spoke of bringing before the Royal Society another, treating of that of the osseous fish. I was working at the early conditions of the salmon's skull at the time. I was, however, led to devote my attention to another and more instructive type early in the following year; for it was then (January 1869) that Professor Huxley was engaged in preparing his very important paper "On the Representatives of the Malleus and Incus in the other Vertebrata" (see Zool. Proc. May 27, 1869). In repeating some of his observations for my own instruction, it occurred to me to renew some researches I had been making from time to time on the frog and toad. The results were so interesting to us both, that it was agreed for me to work exhaustively at the development of the frog's skull before finishing the paper on that of the salmon. On this account Professor Huxley mentions in his paper (*op. cit.* p. 406) that he leaves the Amphibia out of his demonstration, and that they are to be worked out by me. The amount of metamorphosis demonstrable in the chick whilst enclosed in the egg, suggested a much more definite series of changes in a low, slow-growing Amphibian type. I think that this has been fully borne out by what is shown in the present paper.

The first of the ten stages into which I have artificially divided my subject is the unhatched embryo, whilst its head and tail project only moderately beyond the yelk-mass. Another stage is obtained by taking young tadpoles on about the third day after they have escaped from their glairy envelope; a few days elapse between the second and third stages, but a much longer time between the third and fourth, for the fourth stage is the perfect tadpole, before the limbs appear and whilst it is essentially a fish with mixed Chimæroid and Myxinoid characters. Then the metamorphosing tadpole is followed until it is a complete and nimble frog, two stages of which are examined, and then old individuals are worked out, which give the culminating characters of the highest type of Amphibian.

The early stages were worked out principally from specimens hardened in a solution of chromic acid; and the rich umber-brown colour of these preparations made them especially fit for examination by reflected light.

Without going further into detail as to the mode of working my subject out, and without any lengthened account of the results obtained, I may state that the following conclusions have been arrived at, namely, that the skull of the adult is highly compound, being composed of :—

1st. Its own proper membranous sac;

2nd. Of a posterior part which is a continuation, in an unsegmented form, of the vertebral column;

3rd. Of laminæ which grow upwards from the first pair of facial arches, and which enclose the fore part of the membranous sac, just as the "investing-mass" of the cranial part of the notochord invests the hinder part;

3rd. The ear-sacs and the olfactory labyrinth become inextricably combined with the outer case of the brain.

And 5th. The subcutaneous tissue of the scalp becomes ossified in certain definite patches; these are the cranial roof-bones. Around the mouth there are cartilages like those of the Lamprey and the Chimera; but these yield in interest to the proper facial bars, which are as follows, namely:—

First pair the "trabeculæ."
Second pair the mandibular arch.
Third pair the hyoid arch.
And fourth to seven pairs; these are the branchials.

These are all originally separate pairs of cartilaginous rods; and from these are developed all the complex structures of the mouth, palate, face, and throat. The pterygo-palatine arcade is merely a secondary connecting bar developed, after some time, between the first and second arches.

Meckel's cartilage arises as a segmentary bud from the lower part of the second; and the "stylo-cerato-hyal," as a similar secondary segment, from the third arch.

By far the greater part of the cranium (its anterior two-thirds) is developed by out-growing laminæ from the trabeculæ, which after a time become fused with the posterior or vertebral part of the skull.

When the tadpole is becoming a frog, the hyoid arch undergoes a truly wonderful amount of metarmophosis.

The upper part, answering to the hyomandibular of the fish (not to the whole of it, but to its upper half), becomes the "incus," and a detached segment becomes the "orbiculare," which wedges itself between the incus and the "stapes." The stapes is a "bung" cut out of the "ear-sac." The stylo-cerato-hyal is set free, rises higher and higher, and then articulates with the "opisthotic" region of the ear-sac; in the toad it coalesces therewith, as in the mammal. The lower part of the hyomandibular coalesces with the back of the pier of the mandibular arch; and the "symplectic" of the osseous fish appears whilst the tadpole is acquiring its limbs and its lungs, and then melts back again into the arch in front; it is represented, however, in the Bull-frog, but not in the common species, by a distinct bone.

This very rough and imperfect abstract must serve at present to indicate what has been seen and worked out in this most instructive vertebrate.

"Modification of Wheatstone's Bridge to find the Resistance of a Galvanometer Coil from a Single Deflection of its own Needle." By Professor Sir W. Thomson, F.R.S. In any useful arrangement in which a galvanometer and a galvanic element or battery are connected through whatever trains or network of conductors, let the galvanometer and battery be interchanged. Another arrangement is obtained which will probably be useful for a very different, although reciprocally related, object. Hence, as soon as I learned from Mr. Mance his admirable method of measuring the internal resistance of a galvanic element (that described in the first of his two preceding papers), it occurred to me that the reciprocal arrangement would afford a means of finding the resistance of a galvanometer coil, from a single deflection of its own needle by a galvanic element of unknown resistance. The resulting method proves to be of such extreme simplicity that it would be incredible that it had not occurred to anyone before, were it not that I fail to find any trace of it published in books or papers, and that personal inquiries of the best informed electricians of this country have shown that in this country at least it is a novelty. It consists simply in making the galvanometer coil one of the four conductors of a Wheatstone's bridge, and adjusting, as usual, to get the zero of current when the bridge contact is made; with only this change of plans, that the test of zero is not by a galvanometer in the bridge itself showing no deflection; but by the galvanometer, the resistance of whose coil is to be measured, showing an unchanged deflection. Neither diagram nor further explanation is necessary to make this understood to anyone who knows Wheatstone's bridge.

Chemical Society, January 19.—Professor Odling, F.R.S., Vice-President, in the chair. The following gentlemen were elected fellows:—R. Bannister, H. T. Brown, J. Moss, R. J. Moss, E. Pott[illegible] The following papers were read:—"On the action of sulphuric acid on the natural alkaloids," by Henry E. Armstrong. On heating narcotine with sulphuric acid, which had previously been diluted by its own volume of water over the water-bath, and subsequent addition of ammonia to the mixture, a body is obtained which shows at once the properties of dimethylnarcotine, the base which Matthiessen and Wright had obtained from narcotine by means of hydrochloric acid. The reaction takes place, therefore, according to the equation—

$$C_{22}\,H_{23}\,N\,O_7 + H_2\,S\,O_4 = C_{21}\,H_{21}\,N\,O_7 + C\,H_3\,H\,S\,O_4.$$

From this result the author concludes it becomes evident that Gerhardt and Laurent's view, who regarded this body as an amide, must be abandoned. On treating codeine in a similar manner, and dissolving the base obtained in hydrochloric acid, a crystalline hydrochlorate was obtained. An analysis of the product showed it to be hydrochlorate of codeine. The first action of sulphuric acid results, therefore, in the production of an isomeric codeine. By the further action of sulphuric acid, 1 molecule of water is removed from 2 of codeine—then 1 $H_2\,O$ from 1 codeine, and finally, apomorphine seems to be formed. On this last point, however, further evidence has yet to be awaited. "On the origin of nitrates in potable waters," by Ch. Ekin.—The author found nitric acid in the water of a spring which is very remote from any agency that could impart to it decaying animal matter. On closer examination, he found that the water in question had passed through a fossiliferous stratum. This observation necessitates a modification of the "previous sewage contamination theory."—"On an alkaloid from Cinchona bark, hitherto undescribed," by Dr. Howard. This new alkaloid was obtained from the mother liquors of quinine salts. It is a yellowish oil which cannot be sufficiently purified for analytical purposes. The formula of its platino-chloride corresponds with the formula assigned by Gerhardt to the anhydrous platino-chloride of quinine. Mr. Macleod exhibited an ingenious little contrivance by means of which eudiometer tubes which have lost the outer portion of their discharging wires may yet be made use of.

Mathematical Society, January 12.—Mr. W. Spottiswoode, F.R.S., President, in the chair. The Rev. J. Wolstenholme, M.A., Christ's College, and Mr. R. B. Hayward, M.A., late Fellow of St. John's College, Cambridge, were proposed for election. There was a large attendance of members to hear Prof. Peirce, of Harvard, give an account of the methods made use of in his "Linear Associative Algebra." The President conveyed to the author the thanks of the society for his interesting communication. Other papers communicated were "On Systems of Tangents to place Cubic and Quartic Curves," by Mr. J. J. Walker, M.A., and "On the Order and Singularities of the parallel of an Algebraical Curve," by Mr. S. Roberts, M.A. In the course of the evening Mr. Roberts stated the following construction as being mechanically more convenient than one discussed in a former paper "On the Pedals of Conics:"—In a plane, if a limited straight line, whose length is equal to the distance between the centre of two equal circles, moves with an extremity on each, the locus of any point rigidly connected with the line will consist of a circle, and a bi-circular quartic with a third node.

Entomological Society, January 23.—At the Annual Meeting this day held, Mr. A. R. Wallace, President, in the chair, the following gentlemen are elected to form the Council for 1871.—Messrs. Butler, Dunning, Fry, Grut, Higgins, M'Lachlan, Major Parry, Pascoe, E. Saunders, Stainton, S. Stevens, A. R. Wallace, and Professor Westwood. The following officers for 1871 were subsequently elected:—President, Mr. A. R. Wallace; Treasurer, Mr. S. Stevens; Secretaries, Messrs. M'Lachlan and Grut. Librarian, Mr. Janson. An Address was read by the President, which will be published *in extenso* in the Society's Proceedings.

Ethnological Society, January 24.—Professor Huxley, President, in the chair. The Rev. Dr. Steere read a paper "On the Tribes and Languages of East Africa." The author, who had resided for several years at Zanzibar, described in detail the Swahilis, a mixed race, half negro and half Arab, belonging to the Shafi school of Mahommedans. Many examples of their folk-lore were introduced, and a detailed description was given of the Swahili language. A comparison was instituted between the Swahili, Shambala, Yao, and Nyamwezi languages, which all belong to the great Bântu family. Mr. Hyde Clarke spoke upon this communication.—A paper was read "On the weapons and implements used by the Kaffir Tribes and Bushmen of South Africa," by Dr. Carl L. Griesbach. The author described the primitive method of iron-smelting practised by the Kaffirs, and alluded to the knowledge of certain mixed metals, such as brass, possessed by some of the northern tribes. A description was also given of the native method of gold-washing, carried on in some of the tributaries to the Zambesi. The degraded state of the Bushmen was referred to, and it was remarked that although they are ignorant of iron-working, they yet possess some artistic taste. Among the South African implements attention was directed to the musical instruments, which the author considered to have been derived from the Arabs. Dr. Theophilus Hahn made some philological remarks upon this paper, and gave some illustrations of the Hottentot clicks. The President announced that this was the last meeting of the Ethnological Society as a distinct body, and read the terms of union whereby an amalgamation had been effected between the Ethnological and Anthropological Societies of London, under the common designation of "The Anthropological Institute of Great Britain and Ireland."

EDINBURGH

Royal Physical Society, January 25.—Mr. C. W. Peach, president, in the chair. The secretary exhibited a beautiful specimen of the snowy owl *Strix nyctea*, shot near Baltasound, Shetland, on the 24th of December. It was a female, and the remains of a dunin and a jack snipe were both found in its stomach. The facial disc and legs of the bird were pure white, the rest of the bird was whitish, barred all over with rich brown. Another specimen was seen in the same locality, probably the male bird. A curious specimen of a young rook was exhibited. It was of a uniform dull brown or ash colour, instead of the usual black colour, the bill being lightest coloured, and it was feathered down to the base of the bill.—Mr. G. F. Barbour, of Bonskeid, exhibited to the society a fine specimen of the spotted rail, *Crea porzana*. It was shot on the lands of Preston, near Linlithgow, in a bog on the hillside.—Note on the Nesting of the Kingfisher, *Alcedo ispida*, by Prof. Duns The opening to the nest was twenty-two inches below the surface of the bank, and a little more than four feet above the water. The entrance to the nest was by a rounded passage, two inches wide at the front, increasing a little in size as it reached the oval chamber, or nest proper, and being only twenty-one inches long. The chamber was four inches broad, six long, and four high. After looking at the notices of the kingfisher's nest in the literature of ornithology, I find that the specimen before us sheds light on the following moot points:—1. The passage did not slope upwards, but was horizontal, the bottom being about half an inch below the bottom of the nest. "Instinct," says Montagu, "has taught them to have the entrance to their habitation ascending, by which means the filthy matter runs off." The matter referred to is the thin, watery fœces of the young birds, which soon becomes fetid. In this case the end indicated would be partially gained by the greater thickness of the small bones laid down in the passage than in the nest—the passage being thus brought to the level of the nest, and an imperfect kind of drainage supplied, by which, for a time, the watery fouling would be taken from the surface. 2. The hole was not the old hole of a water rat. 3. There were no traces of withered leaves or grass or feathers in the nest. The bottom was covered with the bones of minnows. The nest proper was perfectly dry, though the passage, especially at the edge of the nest, was wet and fetid. As the bones when disgorged must have been wet, it would appear that the pellets must have been scattered by the birds and left to dry before the eggs were dropped. 4. It is evident from this specimen that the bones of small fishes are as truly the lining of the nest as feathers are of the nests of many other birds—"Note on the Plaice," *Platessa Vulgaris*, by Prof. Duns.—"Note on *Lithodes Maia*, fem.," by Prof. Duns. The specimen was taken at Elie, Fifeshire, in December last. It is a female. When received it was loaded with spawn, attached to branching tubes, situated beneath the abdominal plates; the size and arrangement of the abdominal plates, the presence and state of the ova, and the light shed by this specimen on the spawning time, of which Bell and others say they know nothing, deserve to be noted. *Lithodes Maia*, though occurring in the Firth, is no doubt one of our rarer crabs.—"Note on *Galathea strigosa*," by Prof. Duns, New College.—Mr. C. W. Peach exhibited *Antholithes* and its fruit (*Cardiocarpon*) with specimens of *Halonia*, *Flabellaria*, and other fossil plants, from the Coalfield near Falkirk.—C. W. Peach exhibited a large collection of fossil plants from the coal at the Cleuch No. 1 pit, and the brickwork near Falkirk, last summer. Amongst them was a series of *Antholithes Pitcairniæ*, some with its fruit, *Cardiocarpon*, attached; this being the first instance of the kind at present known. He stated that *Calamites*, associated with magnificent fronds of *Flabellaria Corassifolia*, were abundant. *Lepidodendron*, *Halonia*, *Ulodendron*, &c. &c., were much rarer; altogether, they showed that the flora of the coal period of Scotland was varied and of great beauty. He added that they were more interesting from the fact that several of them were generically and specifically identical with plants described in his "Acadian Geology," by Principal Dawson, found in the coalfields of Canada and America, even to the minute shells of *Spirorbis* still adhering to the fronds of *Flabellaria*.—"Notice of the Discovery of a new locality, near Edinburgh, of the Lower Carboniferous rocks, having fossils equivalent to the Burdiehouse and Wardie Series," by Mr. D. Grieve. Mr. Grieve read a notice of a new fossiliferous deposit discovered by him in certain shales and sandstones at Lochend, near Edinburgh, and which are situated on the east side of the loch. Mr. Grieve was led to make a search in this quarter from an indication given by Mr. Geikie, ten years ago, in his "Geology of the Neighbourhood of Edinburgh," that a continuity of the shales on the north side of the Calton Hill would likely be found between that place and Lochend, and which indication he had now verified. Mr. Grieve described the shales as belonging to the Lower Carboniferous formation, and as being equivalents of the sandstones and shales of Burdiehouse, Wardie, and Granton. He obtained *Calamites* of larger size and better marked than those found in the other localities stated as being abundant in the sandstone; *Lepidodendra*—a *Lepidophyllum*, *Sphenopteris*, &c. Of fishes he had obtained a beautiful specimen of the genus *Palæoniscus*, also scales, teeth, and spines, besides coprolites, which are abundant; also numerous specimens of a small crustacean, identical with *Cypris Scoto Burdigalense*, or of an allied species.

GLASGOW

Geological Society, January 5.—Mr. E. A. Wunsch, V.P., in the chair. *Carboniferous Fossils.*—Mr. James Thomson read a paper on the occurrence of *Cœlacanthus lepturus* at Newarthill, and *Palæoniscus Wardii* at Possil. He briefly described the scales, fin-rays, and head-plates of *Cœlacanthus* which had been found in a detached form in the neighbouring coal measures, and which the examination of a nearly entire specimen from the Staffordshire coal-field had now enabled him to identify. It occurs in the upper members of the Carboniferous system in Scotland, in a shale overlying the ironstone of the Airdie coal-field. Both with regard to this and the other ichthyolite—the *Palæoniscus*—before them, he remarked that he had had these forms for years in his cabinet, unnamed; and it was only recently that *Palæoniscus Wardii* had been described, and named specifically after its discoverer in the Staffordshire coal-field, Mr. John Ward of Longtown. It is found in the Possil black-band ironstone, which is between four and five hundred fathoms below the position in which it occurs in Staffordshire, thus not only adding another form to the fauna of our Scottish coal-fields, but adding also to our knowledge of its range in time. The lower beds of the Ayrshire coal-field had also yielded some specimens of this fossil. Mr. Thomson then exhibited specimens of *Rhizodopsis sauroides*, *Amphycentrum granulosum*, and *Platysomus parvulus*, from the Staffordshire coal-field, observing that the scales of *Rhizodopsis* had been found in our Scottish coalbeds, but as yet no complete specimen of the fossil had thence been obtained. Mr. Thomson also exhibited specimens of *Oldhamia* from Bray Head, near Dublin. He described minutely the position of the beds in which these fossils are found, and complained that geological references are frequently so vague as to be of little real service to one going over the ground for himself. Two species of this fossil had been discovered, *O. antiqua* and *O. radiata*; and they were generally believed to have been zoophytes allied to the *Sertularia*. Their precise nature, however, is still matter of discussion. They possessed a special interest as being, with the exception now of *Eozoön Canadense*, the oldest distinct traces that had been found of life on the globe. Mr. Thomson further called attention to the wide unconformability presented by the Mountain Limestone near Dublin, resting, as it does, upon the Cambrian rocks on the north, and upon the granite on the south, side of the bay.

PHILADELPHIA

Academy of Natural Sciences, October 11, 1870.—Dr. Ruschenberger, president, in the chair. Mr. Thomas Meehan said he had noticed a singular habit in the common "Stink bug" of gardens, *Reduvius novenarius*, Say, which might lead to some important physiological discoveries by those more closely devoted to entomological studies. Wondering what made some abrasion on the bark of a *Pinus cembra* on his grounds, he was attracted by a female insect of this species near it; and noticed that on the thigh of the middle leg the usual grey colour was of a polished black. Supposing that possibly the insect may have had something to do with the injury to the bark, through which the turpentine was oozing, he waited a few minutes to re-assure the insect—usually timid under observation—that there was no danger. It then went to work to take the turpentine with the heel of the tarsus of the fore leg, and place it on the thigh of the second leg. It took several dozen "heelsful," winding it round the gathering ball on the leg, as one would wind a ball of string. After it had collected together a ball of turpentine about the size of a pin's head, it gently wiped it off with the femora of the hind leg, and applied it to the anus, where it was very rapidly absorbed. It then walked very leisurely to the top of the nearest branch, when it flew away. This was in the end of September. He saw no more of these insects till a week afterwards, when he cut off a small branch on which was another female, and carried it to the pine tree, applying the branch to the stem so that the insect could walk on to it, without much suspicion of human agency in the matter. As soon as it got to the turpentine, it went through the same operation as the other one, taking two doses of it before it walked away; which it did leisurely, and with much apparent satisfaction. Up to this time he had not been able to find a male, so as to ascertain if it also had any similar use for turpentine.

Oct. 25.—"On the Stipules of Magnolia and Liriodendron," by Thomas Meehan. An examination of the stipules of *Magnolia* affords some highly interesting facts; most, or perhaps all of which are known to leading botanists, but which do not appear to be as generally known as they deserve to be; and which facts may have a more intimate bearing on many of the questions connected with the laws of development than is suspected. On the upper point of the scar next the leaf blade are two small articulation points, where the membranaceous stipules finally parted from the leaf. Examining a leaf before these stipules have fallen, the main veins forming the skeleton of the stipules are found connecting with these articuli, and spreading out, diverge downward toward the base of the leaf. I suppose no one of experience in living plants doubts the possibility of the adhesion of some parts and the separation of others, so as to make new parts or organs. If such is desired, I would refer to the *adhesion* of the carpellary leaves by their backs in the capsules of *Staphylia trifolia*; and for *separation* to the pinnate leaf often formed out of an entire blade in *Fraxinus excelsior, heterophylla*, and many other plants with entire leaves which often have pinnate ones amongst them. The author stated his opinion that the stipules of *Magnolia* are not formed like the stipules of most plants, which are perhaps leaf portions which have never been well developed, but rather are the tolerably well developed side pinnules of a trifoliate or deeply auricled leaf, which in an early stage had adnated with the petiole, and by their edges, and thus formed the stipular sheath we see. This ternate division of the leaf is a marked character in Ranunculaceæ, and with this exposition of a ternate type in Magnoliaceæ, its claim to a place in the Ranal alliance, strong as it always has been acknowledged to be, is still more strengthened. It is impossible to suppose that a so closely allied genus as *Liriodendron* should be founded on a different type from *Magnolia*. We shall see that only very slight causes, which we can well understand, have made some of the chief foliar distinctions, and the few which we cannot prove from actual facts, can be made almost certainties from parallel observations. The identity of type will in this way be manifest. There seems to be every evidence short of an actual witnessing of the fact, that the petiole in *Liriodendron* became adnate with the stem, and in this way the two lateral sections (stipules) were brought in contact with the stem with which they united. This would bring them nearer the sources of nutrition, and enable them to assume a more leaf-like and permanent character than if on the petiole. They become rather primary than secondary leaf organs, and this is just what we see them to be. Thus we may assume that *Magnolia* has typically a ternate leaf structure; that the stipules are the two lateral lobes which, by a peculiar process of adnation, became stipular sheaths after having been partially organised as leaf blade; and that *Liriodendron* differs from *Magnolia* only in possessing a greater power of adnation.

BOOKS RECEIVED

ENGLISH.—A Dictionary of Science: G. F. Rodwell, new edition (E. Moxon and Co).—The Earth, vols. 1 and 2: E. Réclus (Chapman and Hall).—Dr Bevan on the Honey-bee, new and enlarged edition: W. A. Munn (J. Van Voorst)

AMERICAN.—Theoretical Chemistry, part 1: G. F. Barker, M.D. (C. C. Hatfield, New Haven.)

DIARY

THURSDAY, FEBRUARY 2.

ROYAL SOCIETY, at 8 30—On Linear Differential Equations: W. H L Russell, F.R.S.—Measurement of Specific Inductive Capacity of Dielectrics. J. C. Gibson and T. Barclay.—On the Uniform Flow of a Liquid: Rev Canon Mosley, F.R S
SOCIETY OF ANTIQUARIES, at 8 30—On Charters relating to Robertsbridge: C S. Perceval, LL.D., Dir S.A.
LINNEAN SOCIETY, at 8.—Natural History of Deep-Sea Soundings between Galle and Java: Capt. Chimmo, R.N.
CHEMICAL SOCIETY, at 8.—On the Development of Fungi in Potable Water: Dr Frankland.
LONDON INSTITUTION, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S Barff.
ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

FRIDAY, FEBRUARY 3

GEOLOGISTS' ASSOCIATION, at 7.30—Anniversary Meeting.
ROYAL INSTITUTION, at 9.—Polarisation of Light: W. Spottiswoode, F.R.S
ARCHÆOLOGICAL INSTITUTION, at 4.

SATURDAY, FEBRUARY 4

ROYAL INSTITUTION, at 3—Laws of Life revealed in History: Rev. W. H. Channing.

SUNDAY, FEBRUARY 5.

SUNDAY LECTURE SOCIETY, at 3.30.—The Origin, Migrations, and Development of Remarkable Parasites: Dr. Cobbold, F.R S.

MONDAY, FEBRUARY 6.

ENTOMOLOGICAL SOCIETY, at 7—On the Early Development of the Sexual Organs in Insects, and its bearing on the Origin of Species: Mr. Lowne
VICTORIA INSTITUTE, at 8.—Evidence of the Egyptian Monuments to the Sojourn of Israel in Egypt: Rev. B. W. Savile.
LONDON INSTITUTION, at 4.—On the First Principles of Biology: Prof. Huxley. (Educational Course)
ROYAL INSTITUTION, at 2—General Monthly Meeting.

TUESDAY, FEBRUARY 7.

ZOOLOGICAL SOCIETY, at 9.—Notes on some points in the Osteology of *Rhea Americana* and *Rhea Darwinii*: Dr. R O. Cunningham.—On the Arctic collection of Birds presented by Mr. John Barrow to the University Museum, Oxford: J. E. Harting.
ROYAL INSTITUTION, at 3.—Nutrition: Dr. Foster.

WEDNESDAY, FEBRUARY 8.

GEOLOGICAL SOCIETY, at 8.
ROYAL MICROSCOPICAL SOCIETY, at 8.—Anniversary Meeting. Election of Officers and Council.
SOCIETY OF ARTS, at 8.—On Ornamentation considered as High Art: Dr. C. Dresser.
ARCHÆOLOGICAL ASSOCIATION, at 8

THURSDAY, FEBRUARY 9

ROYAL SOCIETY, at 8 30.
SOCIETY OF ANTIQUARIES, at 8.30.
LONDON MATHEMATICAL SOCIETY, at 8.
LONDON INSTITUTION, at 7.30—On the Action, Nature, and Detection of Poisons: F. S. Barff, M A., F.C.S.
ROYAL INSTITUTION, at 3—Davy's Discoveries: Dr. Odling.

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THURSDAY, FEBRUARY 9, 1871

THE POWER OF NUMERICAL DISCRIMINATION

It is well known that the mind is unable through the eye to estimate any large number of objects without counting them successively. A small number, for instance three or four, it can certainly comprehend and count by an instantaneous and apparently single act of mental attention. The limits of this power have been the subject of speculation or experiment among psychologists, and Sir William Hamilton thus sums up almost the whole of what is known about it:—

"Supposing that the mind is not limited to the simultaneous consideration of a single object, a question arises, How many objects can it embrace at once? . . . I find this problem stated and differently answered by different philosophers, and apparently without a knowledge of each other. By Charles Bonnet, the mind is allowed to have a distinct notion of six objects at once; by Abraham Tucker the number is limited to four; while Destutt Tracy again amplifies it to six. The opinion of the first and last of these philosophers appears to me correct. You can easily make the experiment for yourselves, but you must beware of grouping the objects into classes. If you throw a handful of marbles on the floor, you will find it difficult to view at once more than six, or seven at most, without confusion; but if you group them into twos, or threes, or fives, you can comprehend as many groups as you can units, because the mind considers these groups only as units; it views them as wholes, and throws their parts out of consideration. You may perform the experiment also by an act of imagination." (Lectures, vol. i. pp. 253-4)

This subject seemed to me worthy of more systematic investigation, and it is one of the very few points in psychology which can, as far as we yet see, be submitted to experiment. I have not found it possible to decide conclusively in the manner Hamilton suggests, whether 4 or 5 or 6 is the limit, nor do imaginative acts of experiment seem likely to advance exact knowledge. Probably the limit is not really a definite one, and it is almost sure to vary somewhat in different individuals.

I have investigated the power in my own case in the following manner. A round paper box $4\frac{1}{2}$ inches in diameter, lined with white paper, and with the edges cut down so as to stand only $\frac{1}{4}$ inch high, was placed in the middle of a black tray. A quantity of uniform black beans was then obtained, and a number of them being taken up casually were thrown towards the box so that a wholly uncertain number fell into it. At the very moment when the beans came to rest, their number was estimated without the least hesitation, and then recorded together with the real number obtained by deliberate counting. The whole value of the experiment turns upon the rapidity of the estimation, for if we can really count five or six by a single mental act, we ought to be able to do it unerringly at the first momentary glance.

Excluding a few trials which were consciously bad, and some in which the number of beans was more than 15, I made altogether 1,027 trials, and the following table contains the complete results:—

Estimated Numbers.	Actual Numbers.												
	3	4	5	6	7	8	9	10	11	12	13	14	15
3	23												
4		65											
5			102	7									
6			4	120	18								
7			1	20	113	30	2						
8					25	76	24	6	1				
9						28	76	37	11	1			
10						1	18	46	19	4			
11							2	16	26	17	7	2	
12								2	12	19	11	3	2
13										3	6	3	1
14										1	1	4	6
15											1	2	2
Totals..	23	65	107	147	156	135	122	107	69	45	26	14	11

The above table gives the number of trials in which each real number was correctly or incorrectly guessed; thus in 120 cases 6 was correctly guessed; in 7 cases it was mistaken for 5, and in 20 for 7. So far as my trials went, there was absolute freedom from error in the numbers 3 and 4, as might have been expected; but I was surprised to find that several times I fell into error as regards 5, which was wrongly guessed in 5 per cent. of the cases. Abraham Tucker thus appears more correct as to my power than the other philosophers.

But in reality the question is not to be so surely decided by the trial of the few first numbers, as by endeavouring to obtain some general law pervading the whole series of trials. Calculating the average error of estimation in the case of each number, without regard to the direction of the error, we get the following numbers:—

3	4	5	6	7	8	9	10	11	12	13	14	15
0	0	·06	·18	·27	·44	·41	·65	·81	·73	1·08	1·21	1·27

These numbers vary pretty regularly in an apparently linear manner, except that in the case of the numbers 9 and 12, the result is too small. The error is simply proportional to the excess of the real number over $4\frac{1}{2}$, or obeys a law expressed in the formula (n being the real number)—

$$\text{error} = m \times (n - 4\tfrac{1}{2})$$

When we calculate the constant m for each number it comes as follows:—

5	6	7	8	9	10	11	12	13	14	15
·112	·122	·110	·127	·091	·117	·125	·098	·127	·128	·121

These numbers are sufficiently equal to enable us to take the average 0·116 as a good result, and the formula then becomes—

$$\text{error} = 0{\cdot}116 \times (n - 4\tfrac{1}{2})$$

or approximately—

$$\text{error} = \frac{n}{9} - \frac{1}{2}$$

This is a purely empirical law, the meaning or value of which I cannot undertake to explain. The most curious point is that it seems to confirm my previous conclusion that my own power of estimating the number *five is not perfect*. The limit of complete accuracy, if there were one, would be neither at 4 nor 5, but half-way between them; but this is a result as puzzling as one of the uninterpretable symbols in mathematics, just, for instance, like the factorial of a fractional number. But I give it for what it may be worth.

When we take into account the direction of the errors, the results are as follows:—

5	6	7	8	9	10	11	12	13	14	15
+·06	+·09	+·05	0·0	−·05	−·27	−·46	−·51	−·85	−·93	−1·27

Thus there is a clear tendency to over-estimate small numbers and to under-estimate large ones. There is an evident inclination towards those medium numbers which most frequently recurred: how far this discredits the experiments I cannot undertake to say, but it is an instance of that inevitable bias in mental experiments against which it is impossible to take complete precautions.

My conclusion that the number five is beyond the limit of perfect discrimination, by some persons at least, is strongly supported by the principles of rhythm. All the kinds of time employed by musicians depend upon a division of the bar into two or three equal parts, or into multiples of these. Music has, indeed, been composed with the bar divided into five equal parts, but no musicians have yet been found capable of performing it (Rees' Cyclopædia, RHYTHM). Short runs, indeed, consisting of five or even seven equal notes, are not unfrequently employed by the best musicians, but it is to be doubted whether the ear can grasp them surely. I presume it is beyond doubt that 6, 8, 9, or more equal notes in a bar are always broken up by the hearer, if not by the performer, into periods of 2, 3, or 4. Quinary music, even if it could be executed, would be ill appreciated by the hearers, and, though all the powers of the human mind may be expected to progress in the course of ages, quinary rhythm belongs to the music of the distant future.

W. STANLEY JEVONS

BURMEISTER'S FAUNA ARGENTINA

FEW districts of the world are so rich in well-preserved remains of an extinct fauna of remarkable and interesting character as the neighbourhood of the city of Buenos Ayres. The immense alluvial plain of the Argentine Republic is the burial-place of the Megatherium, the Mylodon, the Glyptodon, the Macrauchenia, the Toxodon, and many other strange forms of ancient life, whose bones are ever and anon restored to light by the crumbling away of the soft banks of the great rivers which flow into the estuary of the Plata. So abundant, indeed, are they that, as remarked years ago by Darwin, any line whatever drawn across the Pampas would probably cross the skeleton of some extinct animal.

Collections of these fossils have at various times been sent to several European museums, and much information has been published upon the nature of the animals to which they belonged, but these observations have been generally made upon imperfect or fragmentary materials. The fortunate circumstance of the able and energetic German naturalist, Dr. Hermann Burmeister, formerly Professor of Zoology in the University of Halle, having taken up his residence at Buenos Ayres, and having been appointed Director of the Public Museum of that city, has been the occasion of a systematic and elaborate elucidation of the ancient fauna of this important district.

This has been brought about mainly by the publication of a richly illustrated serial quarto work, entitled "Anales del Museo Publico de Buenos Aires," the special object of which is to describe and figure the new or little-known objects preserved in that establishment. This work, which was commenced in 1864, appears at irregular intervals, but has already reached its sixth number, the first five of which constitute the first volume, the sixth, which is just published, being the commencement of a second volume. Dr. Burmeister is the sole author, and we have no hesitation in saying that it promises to be one of the most important contributions yet made to the knowledge of Mammalian zoology, for to this class is the publication mainly restricted. The parts already before us contain not only far more complete descriptions and detailed figures than have hitherto been given, of many of the extinct forms mentioned above, but it has also several admirable anatomical memoirs on rare or little-known living forms, especially of the Cetacea which occur in the estuary of the great river Plata, and in the adjoining part of the Atlantic Ocean, a field of research hitherto almost unexplored.

As this work, being written in the Spanish language, is not so well known in this country as it deserves to be, we propose to lay before our readers a summary of the contents of the volume already completed, from which they will be able to judge of the richness and variety of the material which has been at the author's disposal, and of the excellent use that has been made of it in his experienced hands.

After a history of the foundation and progress of the public museum of Buenos Ayres, and a general essay on palæontology, a detailed description is given of the skeleton of *Macrauchenia patachonica*. The first discovered remains of this very remarkable animal, which is about the size of a camel, were found by Mr. Darwin in 1834 at Port St. Julian on the Patagonian coast, and presented by him to the Museum of the Royal College of Surgeons of London, where they are now preserved. They were described by Professor Owen in the appendix to the "Voyage of the *Beagle*" (1840). Since that time but little addition was made to our knowledge of the species (although some bones of a smaller animal of the same genus, discovered by Mr. D. Forbes in Bolivian copper mines, have been described by Professor Huxley), until the lamented Bravard commenced the description, in a work to be entitled "Fauna fósil del Plata," of a comparatively perfect skeleton, which was contained in the museum of Buenos Ayres; but as he did not recognise its identity with Owen's Macrauchenia, he gave it the name of *Opisthorhinus falconeri*. The premature death of Bravard in the earthquake which destroyed the greater part of the town of Mendoza, prevented the publication of this work; but three of the plates, which had already been executed, containing figures of the skull with nearly complete dentition, and many of the vertebræ and limb bones, form the first three plates of the present work. To these, Dr. Burmeister has added another containing views of the pelvis and some more vertebræ, and an elaborate description of the whole of the known bones, finally concluding that the zoological position of the genus is among the imparidigitate or perissodactyle Ungulata, between the Horse and the Tapir.

After some remarks on the humming-birds described by Azara, a preliminary notice is next given on the different species of Glyptodon, or gigantic extinct Armadillo, in the museum. Three species are distinguished as well established, viz., *G. spinicaudus*, *G. clavipes*, and *G. tuberculatus*; a fourth, *G. pumilio*, smaller than the other, is founded on a portion of a lower jaw. Some general observations on the osteology of the genus are added.

The next portion of the work is called "Fauna Argentina, Part I., Fossil Mammals." It commences by a geological description of the fossiliferous region, and then follows a list of the fossil mammals of the "diluvian" deposits, with some remarks upon each. This list comprises, of Carnivora, *Machairodus neogæus*, of which the museum possesses a nearly complete skeleton discovered in 1844 near Lujan, about fifty miles west of Buenos Ayres. A short account of the osteological character of this interesting specimen is given, and a full and illustrated description is promised in one of the future numbers of the work. We have also *Felis longifrons*, *Canis protalopex*, *Canis avus*, *Mephitis primeva*, *Ursus bonærensis*. With regard to Marsupialia, it is singular that no remains of this group have hitherto been found in the diluvial deposits of the region in question, although, as is well known, they are not infrequent in the Brazilian caves explored by Lund. The list of fossil rodents includes *Myopotamus bonærensis*, *M. antiquus*, *Ctenomys bonærensis*, *Lagostomus angustidens*, and *Cavia breviplicata*. In the Edentata, the district is of course especially rich. Notices are given of *Megatherium americanum*, *Mylodon giganteus*, *M. gracilis*, *M. robustus*, *M. darwinii*, and a plate is devoted to the illustration of details of the osteology of these two genera, especially the hitherto little known sternum, sternal ribs, and hyoid bones. Then follow *Scelidotherium leptocephalum*, *S. cuvieri*, *Megalonyx meridionalis*, and *M. jeffersoni*. The genus *Glyptodon*, now divided into several sections, is represented by the following species: *G.* (*Panochthus*) *clavicaudatus*, *G.* (*P.*) *tuberculatus*, *G. clavipes*, *G.* (*Hoplophorus*) *asper*, *G.* (*H.*) *elongatus*, *G.* (*H.*) *lævis*, with several species but incompletely known. Numerous details are given of the osteology of these animals, with three plates of illustrations, one containing a view of a complete skeleton of *G. asper*. Under the head of Pachydermata, the teeth of two extinct species of horse, *E. curvidens* and *E. devillei* are described and figured, further notes are added on *Macrauchenia*, together with a complete view of the skeleton, and a restored outline of the animal with a slender elongated pendant proboscis, and some valuable details are given on the genus *Toxodon*, for the first knowledge of which, as in the case of *Macrauchenia*, we are indebted to Darwin's collection, described by Owen. Three species are now distinguished, viz., *T. burmeisteri*, *T. owenii*, and *T. darwinii*, appropriately commemorating the three distinguished naturalists by whose labours the history and affinities of this singular form have been made known. Three plates are devoted to the illustration of this genus. The list concludes with *Mastodon humboldtii*, to the osteology and dentition of which one plate is assigned.

The next and concluding section of the volume is entitled "Fauna Argentina, Part II., Mammifera pinnata." It is devoted to an account of the marine mammalia of the republic. Of the Pinniped Carnivora, *Otaria jubata* and *O. falklandica* are mentioned. The Manati is stated not to occur in the Argentine rivers. The Cetacea are represented by *Pontoporia blainvillii*, *Delphinus microps*, *D. obscurus*, *D. cymodoce*, *Lagenorhynchus ceruleo-albus*, *Orca magellanica*, *Phocæna spinipinnis*, *Globiocephalus grayii*, *Epiodon australe*, *Balænoptera bonæriensis*, *E. patachonica*, *Sibbaldius antarcticus*, and *Megaptera burmeisteri*.

A very detailed description is given of the external characters and anatomy of a newly discovered species of Ziphius, named by Dr. Burmeister *Epiodon australe*, which is not only valuable as being one of the most complete and fully illustrated accounts we possess of the structure of any Cetacean, but especially as the members of the particular group to which this one belongs are all exceedingly rare, or, at all events, have a remarkable habit of keeping out of the way of naturalists, and, consequently, are less known than almost any other section of the Mammalia. Descriptions, more or less detailed, are also given of the following new species:—*Globiocephalus grayii*, and *Orca magellanica*, both founded on characters of the skulls, which are figured. The former has much larger and thicker teeth than any other members of the genus to which it is referred. *Phocæna spinipinnis* is characterised not only by the numerous and regularly placed horny tubercles on the anterior edge of the dorsal fin (which are also frequently found, though in a more rudimentary condition, in the European porpoise) but also by the peculiar form of that fin, the anterior edge being concave, and by the conformation of the skull.

Not less valuable than the anatomical description of *Epiodon australe* is the article which concludes the work, which is a full and excellently illustrated account of the external characters and anatomy of a very singular and aberrant form of dolphin, hitherto but imperfectly known, called *Pontoporia blainvillii*. This animal is one of the smallest of the Cetacea, being but five or five and a-half feet long when adult. It inhabits the estuary of the river Plata and the adjoining parts of the ocean, but it is not truly fluviatile, like the *Mia* of the Amazon and the *Platanista* of the Ganges, to which two forms it presents some structural affinities.

We trust that the brief outline which we have given of the contents of the first volume of these "Anales" will be sufficient to show that it is a book indispensable to every good scientific library, and, in conclusion, we wish to express our cordial hope that the inhabitants and government of Buenos Ayres will continue zealously to carry on the creditable work they are doing for science in keeping up and augmenting their valuable museum, and that Dr. Burmeister will long continue to be the exponent of its treasures.

W. H. FLOWER

RECENT PETROGRAPHICAL LITERATURE

I.

Lehrbuch der Mineralien und Felsartenkunde. Von Dr. F. Senft. Jena. (London: Williams and Norgate.)

THE future historian of Geology who shall describe the rise and progress of the science in England, will find material for one of his most curious and interesting chapters in tracing out the causes which checked the growth, and finally all but extinguished the very existence of petrographical study in this country. While in all that relates to stratigraphical geology, we have kept well ahead of other nations, and have been quite abreast of them in palæontology, we have allowed petrography, or the study of rock-species, to fall into disuse. For half a century we have been content to make shift with vague, incorrect names invented in the infancy of the science; our progress in this respect since the early days of M'Culloch, Boué, and Jamieson, having been simply *nil*. More especially is this true of our nomenclature of igneous rocks. While we have unravelled the complicated stratigraphical structure and relations of these rocks with unrivalled labour and detail, we have left aside the questions touching their mineralogical ingredients and chemical composition, and their classification as mineral compounds. English petrography does not exist; what we have in its stead is an indefinite obsolete grouping of rocks patched up with occasional borrowings from the Continent. And yet, strange to say, it is in England that the most important step in modern petrography has originated. Sorby's application of the microscope to the study of rocks has opened a new era in the science, and our good friend Sorby himself is regarded as a kind of demi-god in the eyes of our German brethren of the hammer. But even his wand, though it has raised up a new army of zealous petrographers on the Continent, has, as yet, failed to quicken the dry bones of English petrography Mr. David Forbes is our *spes altera Romæ*. Our waiting eyes have been turned to his laboratory in York Place, Portman Square, for years past. His materials are vast, his enthusiasm great, and his intention fixed, to retrieve the honour of English petrography. May his shadow never be less until long after his wishes and our hopes are fully realised!

They manage things petrographical very differently in Germany. There the study of rocks is introduced into the curriculum of schools and universities. It is treated of in many excellent text-books. It is eagerly pursued by zealous investigators from Berlin to Vienna. The great paper of Mr. Sorby, published here thirteen years ago, has done much to quicken this research by showing that the older methods were in many respects untrustworthy. These methods were based primarily upon chemical analysis. But such analysis, while it reveals the ultimate chemical constitution of the rock, may not explain its mineralogical composition. The various stages of the metamorphism of the component minerals are thereby often lost sight of. Hence two rocks, having by analysis approximately the same chemical composition, may differ materially from each other in mineralogical composition. It is here that, as Sorby showed, the microscope comes in to our aid, and shows what the different mineral ingredients of the rock are, how far they have respectively undergone alteration, how they are built into each other so as to form the rock-mass, and under what conditions they may originally have been formed. This important addition to the methods of research has so powerfully affected petrography, that this branch of science must be regarded as at present in a transition state. Many of the groups of rocks in the nomenclature now in vogue in Germany will require reconsideration. More especially is revisal needed in those based upon subdivisions of the triclinic felspars. Petrographers are now coming to see that, in a vast number of cases, it is not possible to discriminate the particular species of felspar in a rock, further than as belonging to the othoclase or plagioclase division. In this separation the microscope becomes of essential importance.

A small pile of German petrographical literature has accumulated on our table, and we propose in this and a subsequent paper to notice the more important works. The first volume that comes to hand is another publication of that most voluminous writer, Dr. Ferdinand Senft, Professor of Natural Science, Eisenach. He seems to issue a goodly octavo every year, though possibly the past year's political events may have interrupted his labours for 1870. The present work is entitled "A Text-book of Mineralogy and Petrography," and contains some 700 pages. One would have thought that the Doctor had hardly left himself room for such a book as this, when we remember not only his former special treatises on the subject, but his text-book of "Forstlicher Naturkunde," one of the volumes of which is devoted to geognosy, soils, and chemistry. And yet the book differs materially from any of his former works, and, if we mistake not, is likely to be at least quite as useful. It is not designed to be an elaborate methodical text-book, but one in which the teacher and pupil will find all the material they require for a successful and methodical study of minerals and rocks, and also one which will prove sufficient for the student in his early inquiries, even without the help of a master. The author has had peculiar advantages for the compilation of such a book. In an interesting preface he tells us that for a quarter of a century he has been engaged in teaching these subjects to the two higher classes in a school, and he details the method of instruction which his experience has found to be successful. He had used the best mineralogical treatises as text-books in his classes, but had always found them too difficult for use in schools. Accordingly in 1860 he brought out a little "School Text-book of Mineralogy and Geognosy," which, having been out of print since 1866, he has remodelled and enlarged into the present work.

The general plan of the book is like that of the ordinary German text-books, only the first or mineralogical division occupies about five times more space than that devoted to petrography. It is of the latter that we have at present to speak. Retaining the usual grouping of crystalline and fragmental rocks, the writer gives a clear and succinct account of each subdivision and species. His plan for the specific details somewhat resembles that in his earlier work on the Classification of Rocks, but with some improvements. Under each species of rock, a clear but brief description of its leading features is given in large type, then follows an equally concise account of its varieties, transitions, mode of weathering, geological occurrence, and geographical distribution. The notes on the weathering of the different rocks, and the general remarks on that subject in the introductory part of the petrographical section, go some way to supply a want which every beginner soon discovers to exist in other manuals. The same commendation may be given to the descriptions of the various kinds of *débris* and soil formed by the decay of rocks.

As a school-book, the present volume seems likely to prove useful. The arrangement into short subdivisions, each clearly marked in the mode of printing, and treating each rock in the same method, will facilitate the progress of a class, and give precision to the inquiries of a beginner. Were there only any general taste for such pursuits in our own country, we might hope to see the book translated and adapted for use in our colleges and schools. At the same time, we are bound to notice what appears a very serious defect in the volume. The author has ignored recent microscopical research, and instead of giving that method a distinct and prominent place in his account of the investigation of rocks, he contents himself with the old "dry way" and "wet way" of analysis. In so doing, he tacitly confesses himself to be behind his time. His compilation, useful as it is, will, we hope, ere long be superseded by another, when petrography has had time to compose itself again into something like clearness and symmetry. In the mean time, the student who wishes to go more fully into the matter, will still find Zirkel's "Lehrbuch" his best guide, though even that valuable manual is fast getting out of date; owing to the great progress which the last few years have witnessed in this branch of geology. That progress has been largely shared in by Zirkel himself, as will be shown in a subsequent paper.

ARCH. GEIKIE

OUR BOOK SHELF

Der Zoologische Garten. Zeitschrift für Beobachtung, Pflege und Zucht der Thiere. Herausgegeben von Dr. F. C. Noll. XI. Jahrgang, 1870. (Frankfort a. M.) (London: Williams and Norgate.)

THE "Zoological Garden" is, as its name imports, a periodical especially devoted to all that is connected with the maintenance of animals in what are commonly called "Zoological Gardens." Having been founded some ten years ago by the Zoological Society of Frankfort-on-the-Main, it more especially relates to the affairs and condition of the small but well-arranged garden belonging to that Society, which is situated in the environs of that free and ancient city. It may appear somewhat surprising that a journal devoted to a subject of apparently so limited an extent can achieve sufficient circulation to command success. But the number of zoological gardens, aquarium houses, and similar establishments in Germany, has considerably increased of late years, and their institution in nearly all the principal cities of the Fatherland has been very favourably received, so that it is easy to understand that a considerable amount of popular interest in these subjects has been excited. Hamburg, Cologne, Dresden, Berlin, Hanover, and Munich, have all flourishing establishments of this description, and although the Zoological Garden founded some years ago in Vienna has come to an untimely end, yet in every part of what is now the new Empire of Germany the prospects of such institutions seem to be extremely favourable. But our "Zoological Garden" by no means entirely confines its attention to animals in captivity. It likewise contains many excellent articles relating to the habits of birds and beasts in a state of nature, so as to embrace many of the well-known attractions of a popular magazine of Natural History. Occasionally also more strictly scientific articles, such as that of Professor Pagenstecher on the Anatomy of the Cape Hunting Dog (*Lycaon pictus*) in the numbers for July and August last year, are given, so that the result is a zoological miscellany of a very various character. The woodcut illustrations are, it is true, perhaps not always in the highest style of art, but we have seen many inferior in English popular works of Natural History, and they have generally the merit of being tolerably correct. To such of our readers therefore as are growing weary of the "Zoologist," and cannot appreciate the learning of the "Annals," we recommend a trial of the "Zoological Garden," it being pre-supposed, of course, that they understand the language in which it is written (which in these days is a matter of course !). The subscription-price is very moderate, amounting only to about 8*s*. per annum for the twelve numbers, and the journal is regularly forwarded through the post to this country.

P. L. S.

The Marvels of the Heavens. By Camille Flammarion. From the French, by Mrs. Norman Lockyer. With 48 Illustrations. (London: R. Bentley, 1870.)

THE French certainly have the art, which we have not, of putting science in an attractive form to the popular mind—attractive, and yet not at the expense of scientific accuracy. Good service is, therefore, done by the translation into easy and graceful English of works like this by M. Flammarion. From the very commencement he carries the reader with him by his enthusiasm. Instead of starting with a bare statement of facts—that the Sun is the centre of the solar system, that it is so many hundred thousand miles in diameter, and has this, that, and the other planet revolving round it at such and such distances, he takes his reader out with him, as it were, to behold the heavens on a starry night; explains how it is that we see the sun only during a portion of the twenty-four hours; and speaks of the arrangement of the stars in clusters and nebulæ. Then he descends from the stars as a whole to a particular one, the Sun, and proceeds to describe in detail the solar system. And, throughout, the subject is treated with a graceful fancy and a wealth of illustration which make it very charming. Old Greek myths and fables of the astrologers, quotations from Byron and Lamartine, from Bryant and Victor Hugo, anecdotes of the value of astronomical knowledge, are brought in to point the moral and adorn the tale, and never appear to come amiss, or to be beside the mark. We must say a word about the illustrations, which are extremely good. We have never seen anything that so well recalls to our mind the appearance of the heavens through a powerful glass as Fig. 21, a part of the constellation of the Swan, as seen through the telescope; on the opposite page is placed, by way of contrast, the same seen by the naked eye. Author, translator, and artist have combined to produce a book which ought to be in the hands of every one who desires an introduction to "The Marvels of the Heavens."

B.

Geology. By Prof. John Morris, F.G.S., and Prof. T. Rupert Jones, F.G.S. First series. Heads of Lectures on Geology and Mineralogy, in several courses from 1866 to 1870, at the Staff and Cadet Colleges, Sandhurst, by T. Rupert Jones, F.G.S. (London: Van Voorst, 1870.)

THIS book can hardly be called a Manual of Geology; it is rather the *avant-coureur* to the book which is to be written presently. It is a series of outlines for a course or courses of lectures, furnishing in a brief and concise form the heads or texts for any number of geological discourses which a Science teacher may be called upon to give: or the student may take it as his guide to the main lines and branches of geological study, along which he may have to pursue his readings in preparing himself, either alone or with the assistance of a "coach," to pass his "B.Sc.," or other examination at any one of the Universities. Now-a-days, when a man has to coach up so many different subjects in so short a time, it is obvious that, the more handy and concise a book is, the more useful will it be in helping to the desired end. One thing more seems to us to be needed in order to render this book of practical utility to the *uninitiated*; it is, to give, under each head, references to the authors (with chapter and verse) whom the student or teacher should consult, to gather more fully what is here only hinted at, often in but six words or less. To those who are already read up in Geology and Palæontology, the book is a most useful form of "Remembrancer," containing besides numberless facts — the key-notes to whole discourses on the earth and its past history.

H. W.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

The Cretaceous Period

In the number of NATURE for January 19 appeared a letter from Prof Wyville Thomson, in which he attempts to justify the opinion which has been concisely summed up in the words "we are still living in the Cretaceous epoch," a statement emphatically contested in the lately published "Students' Elements of Geology," by Sir Charles Lyell.

Prof. Thomson begins by supposing that the term Cretaceous epoch is considered by geologists as an undefined period of time which may very well be elastic enough to reach up to the present day; and, in this misapprehension of the exact meaning of geological terms, betrays the source of the whole misunderstanding. This accounts for an unnecessary digression about axes of elevation and raising the floor of the Atlantic to the surface; as if the possibility of part of that ocean having remained as such since the chalk period had ever been denied. It is known with certainty that a large area of Cretaceous sea existed where the Atlantic does not extend, but beyond our observations on dry land it is perfectly useless to speculate how far the present distribution of land and water coincides with that which obtained when *Belemnitella mucronata* was a living animal. Moreover the possibility of perfectly continuous and conformable series of deposits from the chalk period upwards being present somewhere at the bottom of the sea, has not received an atom of confirmation in the late deep-sea dredgings, and if true, would be no reason for laying aside a very serviceable classification based on other than purely stratigraphical considerations.

In speaking of organic remains, Prof. Thomson persists in misunderstanding the reasons on which the title "Cretaceous" has been founded, and so clearly defined in the controverted paragraph of the Student's Elements, p. 263. He cannot gainsay the fact that so many genera of Cephalopoda have completely disappeared, and, indeed, grants only a generic and specific resemblance between certain chalk fossils and recent deep-sea forms; which is obviously no adequate reason for the assumption that we now live in the Cretaceous epoch It seems also to have been forgotten that our knowledge of this period is not confined to chalk strata, but is derived also from the examination of sand and clay formations in England, as well as those on the Continent and America, which are recognised as Cretaceous from their containing the same species of mollusca which characterise our chalk. Further, as the only organisms common to the Atlantic sand and the chalk are the Globigerina and other Bathybius, which are also common to the Indian and Pacific Oceans, any inferences drawn from their occurrence in the Atlantic are equally applicable to the two latter cases, and equally so to Eocene seas where Globigerinæ flourished along with Nummulites.

In fact the whole question is shortly this: calcareous mud has been dredged up from the bottom of the Atlantic, often from great depths. This mud resembles chalk so much as to leave no doubt but that the older deposit was produced under identically the same physical circumstances and organic conditions (as far as concern the matrix of Globigerinæ, Coccoliths, &c, but exclusive of all imbedded remains) as the newer formation. The outward resemblance of the two rocks, and the fact of the new deposit having been found so near where the old one accumulated, together with the recurrence of a form of Encrinite and a few Echinoderms, have imposed on Dr. Thomson, and led him to a conclusion which has no justification in facts Had this eminent naturalist been an equally experienced geologist, he would have seen that, with the recurrence of outward conditions, it was but natural to expect a return of some old forms of life, as is the real state of the case.

L. L.

A QUARTER of a century ago, when first I began to study geology, it appeared to me that a predominance was given to the more recent rocks, such as the Pleistocene, Miocene, Eocene, Cretaceous, and the like, to which they were not entitled, when ranked as periods alongside such great groups as the Carboniferous the Silurian, the Cambro-silurian, and the Cambrian. The more I have since daily learned confirmed me in this opinion, therefore it is with great interest that I have watched the discussion on the valuable suggestions of Drs. Thomson and Carpenter. Perhaps I may be allowed to join in the controversy, and to draw attention to some facts connected with the Carboniferous period, which seem to add weight to the arguments put forward by those gentlemen. Before proceeding further, I ought to state that in the Carboniferous period I include the so-called Old Red sandstone or Devonian period, on account of their being interpenetrated, dovetailed, and graduated one into the other in the S.W. of Ireland.

In the counties Limerick and Clare, well marked divisions occur in the Carboniferous period, of which the following is an epitome:—

11. Coal-bearing rocks.
10. Flag series.
9. Lower coal-measure shales.
8. Upper limestone.
7. Calp (shaly orgillaceous limestone, with shales and sometimes sandstones. Between the calp and the upper limestone a cherty zone may occur, but it is not constant).
6. Upper chert zone.
5. Amorphous limestone (Fenestrella beds).
4. Lower chert zone.
3. Lower shaly limestone.
2. Lower limestone shale.
1. Grit, &c. (Old Red sandstone, or Devonian period).

Certain fossils do range through all these divisions, while others will occur in all the similarly, or nearly similarly, circumstanced rocks; nevertheless, the major part of, if not all, these divisions have their groups of fossils. In some, a fossil first appears, in others it dies out, so that any one well acquainted with the rocks in the field can tell from the assembly of fossils to what division of the Carboniferous period the rock belongs.

The section of Limerick and Clare is only local, and will not be found in many places in Ireland. To the S.W. and S. in Cork and Kerry, some of the divisions die out, while others thicken, so that eventually in S.W. Cork and Kerry the limestones have entirely disappeared. To the N., N.E, and E. of Clare other changes take place. Moreover, they are not as gradual or regular as those to the S.W., and more especially if the rocks are followed towards the N.E. In north Galway the divisions in the limestone have disappeared, while interstratified with the limestone, in some place evidently high up (the representation of division No 8) will be found sandstone, and conglomerates in aspect exactly similar to the so-called Old Red sandstone, and when fossiliferous containing similar fossils. Farther N.E. in the county Leitrim, the divisions are very similar to those in Clare, while still farther N.E. the different divisions become mixed, and in the coal-fields of Ulster will be found rocks similar to the coal measures, the Old Red sandstone, and the Carboniferous limestone, all interstratified one with another.

Neither are these rocks without breaks In West Cork there is a continuous sequence either from the upper Silurian rocks, or rocks immediately above them to the coal measures; while in Kerry, there are in these rocks two well-marked breaks or unconformabilities, and in Limerick there may be another, as, at the only junction exposed, between the lower-coal-measure shales and the upper-limestone, the limestone appears to have been denuded prior to the shales having been deposited. By the above it is shown that the condition under which the Carboniferous rocks were deposited, in the area that is now Ireland, are very varied in character, and if Great Britain were also included, other marked changes could be pointed out; as, for instance, in South Staffordshire, where there seem to have been subaerial accumulations going on during a great part of the Carboniferous period, as suggested by the vast thickness of the coal. If, in such a mere speck as Ireland, these great changes took place in the Carboniferous period, what must have been the vast changes on the whole earth's surface?

To me there appears to be a certain analogy between "the Carboniferous period" and the "Cretaceous period of Thomson and Carpenter." Both are great limestone periods, and both will have their coal beds. But what is more remarkable, Carpenter suggests that the geologist of future ages will separate the rocks forming in the "cold area" from the rocks forming in the "warm area," and put them in separate formations on account of the great difference in their mineral constituents and their assembly of fossils, while the greater number of the field geologists of the present age make two periods of the rocks of the Carboniferous period (viz., Carboniferous period and Devonian period), although the rocks of both groups are found to inter-penetrate one another, to dovetail into each other, and gradually to blend one into the other, both in lithological character and fossil contents.

Will any palæontologist take it on himself to say that there is a greater difference between the fauna of the Atlantic chalk and the chalk of England, than there is between the lower and upper divisions of the rocks of the Carboniferous period? Of course the Atlantic chalk is not to be represented only by the low forms found in the deep-sea soundings, as they do not represent its entire fauna. Years ago the late Mr. Salter pointed out at Glengariff, Co. Cork, that more fossils occur at changes of strata than elsewhere; such as the uppermost limits of a series of argillaceous or arenaceous beds, or at the top of a bed, if grits and shales alternate. This I have since found to be a good general rule, more especially when subordinate beds appear in a group. In Limerick, as well as other places in Ireland, masses of limestone may be without fossils, or, at least, conspicuous fossils; but if subordinate beds appear, such as the cherty zones, the aspect of affairs immediately changes, and, as a general rule, the rocks immediately subjacent to such changes are almost entirely made up of fossils and their *débris*. Similar changes are not only possible, but also most probable, in the Atlantic chalk. However, they are not likely to be proved in our day. But as in the limestone, so in the Atlantic chalk, in such places the mass of the fossils belonging to the latter ought to be found.

In considering such a question as the present, I would suggest that such fragile accumulations as those of the Kainozoic epoch ought to be considered of only minor importance; as most of them would be denuded away as the land sank, while those that chanced to remain would only form very subordinate strata. Moreover, Edward Forbes long since suggested that both from palæontological and petrological considerations, it might be better if the division between the Mesozoic and Kainozoic epochs were obliterated. Furthermore it has to be borne in mind that while in new strata very minute breaks can be detected; in old strata, like the Carboniferous period, it would be nearly impossible; and most of the great advocates for the minute division of the newer rock would not allow them in the old, as they explain everything they cannot understand by a "fault."

Connemara, Jan. 29 G. HENRY KINAHAN

Eozoön Canadense

THE organic nature of Eozoön Canadense may, I trust, be regarded as established conclusively by the evidence which has been adduced by Dr. Carpenter, Dr. Hunt, and myself, and I think I am safe in saying that it is accepted by all or nearly all those best qualified to judge. Since, however, the doubts expressed by your correspondent, Mr. Reade, may be shared by many who have not had full opportunity to satisfy themselves on the subject, I think it may be useful once more to direct attention to the facts serving to answer the objections which he has stated, and which, on more full consideration of the questions involved, I trust he may abandon.

Your correspondent objects: First, That the supposed Liassic serpentine or ophiolite of Skye shows structures similar to those of Eozoön. In answer to this it is not necessary to have recourse to the supposition that creatures similar to Eozoon have continued to exist up to the Liassic age, since, as Dr. Hunt has shown,* there is reason to doubt the accuracy of the observations which refer this rock to the Lias; and, further, Profs. King and Rowney, in a recent paper on Eozoon in the Proceedings of the Royal Irish Academy,† have figured this supposed Eozoön, and have thus shown that the portions of it which they consider similar in structure to the Canadian specimens do not possess such structure. I would not, in any Canadian specimen, accept such appearances as those represented in their figure as the Eozoön. This objection is therefore wholly irrelevant.

He objects: Secondly, That Eozoon occurs only in metamorphic rocks, and usually mineralised by serpentine. To this I answer: (1) It unfortunately happens that Eozoon is a fossil of the Laurentian period, and that the rocks of this age are in a more or less metamorphic state in every part of the world where they are known. When we shall have found unaltered Laurentian rocks it will be time to inquire if this fossil occurs in them, and in what state of preservation. (2.) I have elsewhere shown that the chambers and canals of Eozoon are filled not only with serpentine but with other mineral substances, as Loganite, Pyroxene, and Calcite. There is thus, as Sir William Logan affirmed previous to the discovery of the minute microscopic structure, no connection between the forms of the supposed organism, and the mineral substances in connection with which they appear.

In the third place, in order to be enabled to make the assertion above referred to, your correspondent "disposes of" the Tudor specimen, which, as compared with the others examined, occurs in a comparatively unaltered sediment. With regard to this specimen, I affirm, and the published figures show: (1) that it presents the characteristic features of Eozoön, more especially resembling the specimens from the Calumet and from Perth; (2) that other specimens found in the same locality confirm its determination as Eozoon; (3) that the matrix containing the Tudor specimens is a coarse limestone not more metamorphic than many Silurian beds holding fossils. I have, however, to state that the recent explorations of Mr. Vennor, of the Geological Survey, seem to show that the beds which afforded the Tudor specimen, though unconformably underlying the Lower Silurian, overlie the highly metamorphic Lower Laurentian of the district, and, therefore, instead of being, as heretofore supposed, comparatively unaltered Lower Laurentian, they may prove to be even as late in age as the Cambrian. It is in these rocks that the worm-burrows which I observed some time ago occur.*

Fourthly, he alleges imitative forms which Profs. King and Rowney consider to be "identical with the thing itself." Now, imitative forms are not unknown to palæontologists. I have seen rill-marks figured as fossil leaves, and trails of worms and other mere markings, as fossil plants of various kinds; and many dendritic crystallisations are wonderfully like mosses and algæ. I have on my table at this moment a curious group of rounded concretions of black oxide of manganese in a coal-formation sandstone, which I received a few days ago from a very judicious collector, who believed that it was an undescribed fruit. But such things do not invalidate the evidence of real fossils. It is to be observed, however, that while it is extremely easy to assert that such imitative forms are identical with fossils, and even to make this appear plausible in descriptions and drawings, careful examination of actual specimens, with attention to chemical conditions and modes of occurrence, may be necessary in order to draw the proper lines of distinction. In the case of Eozoon, the imitative form has neither been shown to unite the general arrangement, microscopic structure, and mode of occurrence of the fossil, nor perfectly to resemble it in any one of these respects.† In so far as my own comparisons have extended, I am prepared to demonstrate the difference between all such crystalline, dendritic, and concretionary forms, and the Canadian Eozoon.

Your correspondent merely confines himself to general assertions and to starting difficulties. His authorities, Profs. King and Rowney, in the paper above referred to, have ventured on the more dangerous ground of constructive criticism, and have endeavoured to explain the way in which they suppose Eozoon to have been produced. In doing so they have been obliged to resort to an extravagant and complex theory of pseudomorphism, which I fancy most of the palæontologists will throw down in despair of comprehending it, and which I am sure any competent mineralogist or chemical geologist who studies it, will reject as much more trying to his faith than anything required to explain the occurrence and preservation of Eozoön as a fossil.

Lastly, your correspondent desires further investigations with reference to the questions involved in the organic character of Eozoön. It may satisfy him to be informed that Dr. Hunt and I have just sent to Dublin a reply to the objections of Profs. King and Rowney, in their paper above referred to; and that I have for some time been pursuing investigations of Primordial and Silurian fossils akin to Eozoön either in structure or mode of preservation. When these investigations are completed, I hope to show that Eozoön has several foraminiferal successors in the older palæozoic rocks of Canada, and that fossils of various kinds occur in those rocks infiltrated with mineral matters in a manner not dissimilar from that observed in the Laurentian Eozoon. J. W. DAWSON

McGill College, Montreal, Jan. 18

Natural Science at Cambridge

"M. A." will best satisfy himself as to the grounds for the sentence which appeared in NATURE for January 12, to which he demurs, respecting the willingness in Cambridge to award fellowships for merit in Natural Science, by making inquiries of the tutors of the several colleges. This I trust he will do, and if he takes the opportunity of impressing upon them the advantage of following the example set by Trinity of absolutely offering a fellowship or fellowships as the reward of great proficiency in Natural Science, he will be doing a great service, for that is unquestionably an important desideratum. At the same time, I would ask him to take the trouble to ascertain whether there have recently been in Cambridge any persons of great proficiency in Natural Science to whom fellowships have not been awarded. A close and fair examination of the matter will, if I mistake not, prove to him that the colleges have not been backward in this way in rewarding real merit in Natural Science; that there is in some colleges not only a willingness but an anxiety to do this; and that the arena is opening for Natural Science to enter the lists against Classics and Mathematics, with the prospect of a fair adjudication of fellowship prizes. "M.A." will do a further great service if he can turn his wide acquaintance with the members of various colleges to account by inducing the colleges to offer more Scholarships for proficiency in Natural Science, instead of limiting them so much, as is at present done, to students who have not commenced a University career. The stimulus thus afforded to the study of Natural Science by undergraduates would have the effect of producing a greater number of candidates deserving fellowships, and the more frequent award of fellowships to them.

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* Silliman's Journal, March 1870.

† Proc. R.I.A., July 1869.

* Journal of Geological Society, xxii. 608.

† Messrs. Rowney and King themselves virtually admit this.

THE WRITER
Feb. 4 OF THE ARTICLE IN QUESTION

Prismatic Structure in Ice

DURING the late frost and subsequent thaw, I have watched the ice as far as was in my power to see whether its demeanour bore out Mr. Langton's explanation of the prismatic structure (NATURE, vol. iii. p. 105) in reply to my communication (Id. vol i. p. 481). At the time when I received his letter, I felt unable to accept the compromise which he proposed, and the result of these last investigations has been to confirm my previous opinion. Let me, however, first explain away a slight misconception into which I have led him. In using the words "*severe* frost," I spoke as an Englishman, and used the epithet relatively, without thinking how it might be understood by one accustomed to a colder climate. All I meant was a frost severe enough to form ice more than an inch or so thick.

I will first give you the result of my observations, and then proceed to answer the questions which Mr. Langton proposes.

On December 26, while skating for some hours on a pool in Hagley Park, Rugeley, I examined the ice carefully, but could detect no trace of the prismatic structure. The air bubbles, generally more abundant towards the lower side of the ice, were irregularly dispersed, and not in any way arranged in vertical lines, and the ice had its usual fracture. On Tuesday, January 3, and the following day, I again skated on the same pool, and could not ascertain that any noteworthy change had taken place during my absence in such parts of the ice as were free from snow. Then came a thaw, on the first day of which I visited the pool; the sloppy state of the surface, owing to the melted snow, made examination difficult; but on breaking the ice, I detected occasional traces of the prismatic structure. On returning in the afternoon of January 7, I found that a slight frost during the morning had been sufficient to make the ice safe, though the surface was still wet in places. Now, however, there was a marked change in its appearance; in many spots the delicate surface-reticulation caused by the prismatic structure could be detected; small air-bubbles, as it seemed to me, were more numerous, and very many of them were arranged in vertical lines, which when produced, met the angles of the surface-polygons. In a few cases they were not in vertical but in sloping lines; here it was evident that from some accidental cause, the sides of the prisms had not been at right angles to the surface of the ice; this, however, was rare. Everything that I saw convinced me that the lines of bubbles as a rule were the consequence of the prismatic structure, not the cause of it. The quantity of snow and sleet that subsequently fell made it impossible again to examine the ice satisfactorily before I left the neighbourhood, though I visited the spot more than once. Yesterday and to-day I have been examining the ice on a pond in the Botanic Garden here, which is gradually melting away. It exhibits almost everywhere the delicate prismatic structure which I described last year. On withdrawing fragment after fragment, one to two inches thick, from different parts of the pond, I found the surface covered with a most delicate reticulation, and the edges crenulated, as though the whole were a model of a sheet of columnar basalt. The distance between opposite angles of the surface-polygons was generally from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch. Here and there the ice-slab was partially or wholly perforated, the surface-water having penetrated at the junction of a set of joints, and dissolved away more or less of the adjacent columns; in not a few cases these apertures were polygonal, whole columns having perished. There were, of course, many air-bubbles; but I could see nothing to lead me to suppose that they had caused the structure; thousands more would have been required than I could discover, for in many places where the structure was very perfectly exhibited there was hardly a bubble to be seen.

With regard to the first of Mr. Langton's questions, "Does ice contract on approaching 32° F.?" Jamin ("Cours de Physique," vol. ii. p. 108) states that it does; and though he does not mention either the amount of contraction or the point of minimum density, the former, from his illustrative diagram, appears not inconsiderable. (2.) "Do air-bubbles form from the first in vertical lines?" I should say not, as a rule. (3.) "Is there any indication, as the winter advances, of a re-arrangement of the bubbles, as that they run into each other, and get more and more ranged in vertical lines?" None that I can discover. (4.) "Is there any indication in the earlier stages of the ice that after a night's hard frost the cracks seen on its surface spread through its substance?" I have not observed any. The fifth question I have not at present the means of answering; but that does not materially affect the point in debate.

I may add that while visiting the Schafloch Glacière, during the past summer, I noticed that, near the entrance of the cave, the ice everywhere showed prismatic structure, but that near the extremity, where the temperature was at the time about 30° F., there was little, if any trace of it.

One word in conclusion to this long letter, on a point of practical importance. I am now convinced that this minute prismatic structure is the rule rather than the exception in a thaw, though it has hitherto been overlooked; and that it is the chief cause of the "rottenness" in ice. When it has been set up, a slab of ice 5 or 6 inches square, and full one inch thick, can be broken across as easily as if it were a cake of bread; and slabs little larger will snap in two when dropped flat on level turf from a height of less than eighteen inches. Almost every thaw brings its melancholy tale of persons drowned while skating on ice which, though thick, proves on trial to be "rotten." I have no doubt that in every one of these cases it had become prismatic. Therefore all who are about to venture on the ice, when a thaw is setting in, should look carefully for the signs of this structure, and if it is present keep their distance.

T. G. BONNEY

St. John's College, Cambridge, Jan. 17

Coming Home from Sicily

THE Eclipse Expedition has gone forth and returned, having on the whole been most successful in its observations. But never, perhaps, was science pursued under greater difficulties. The hot haste in which all the necessary preparations and arrangements had to be made and perfected before departure is already well-known; the combination of circumstances which impeded our locomotion on the outward journey to Sicily; the slight mishap on the Brenner, and the unfortunate loss of the beautiful *Psyche* have been noised abroad, but the return journey was not accomplished without mishaps, although nothing has been said about them. The stars in their courses fought against Sisera not more relentlessly than did the elements against us.

The Eclipse over, with all possible speed we packed up our instruments and started for Naples, being anxious to hasten homewards. We reached Messina on Monday, 26th December, with the full intention of sailing immediately. But a sirocco prevailed, which snapped the telegraph cable to Naples, and prevented our finding out anything about the movements of the steamer. Daily we expected the arrival of one from some quarter or other which might take us back, but we hoped against hope that each day would be our last in Sicily. It was not until late on Friday afternoon, Dec. 30th, that our hopes were answered, and we weighed anchor.

In due time we reached Naples (after a rough passage), and there our party divided. Between Naples and Rome the communication was open, but, on arriving at Rome, we found very evident traces of the recent inundations, which are said to express the indignation of Providence or the delight of Father Tiber at the downfall of the Pope's temporal power. The regular route from Rome to Florence *viâ* Foligno and Perugia was no longer available, as the flood had carried away most of the bridges on the railway, so we were compelled to take the other and very circuitous route *viâ* Civita Vecchia, Pisa, and Empoli. From Florence, after a delay of two days, during which large quantities of snow fell, we made an attempt to reach Bologna on January 9th, but our ill-luck still followed us, and we had to return whence we had come.

By the advice of some Italian friends, we decided to wait a few days at Florence before making a second attempt to cross the Apennines. Luckily we succeeded the next time in crossing the mountains and leaving Lombardy without any great inconvenience, for by this time the four feet of snow which had covered the country everywhere had been cleared from the rails. We experienced no further difficulty until we arrived at Brenner, where, after remonstrances on our part, we were turned out in a heavy snowstorm to find our way as best we could to the nearest hotel. The next morning we found that the cause of the delay was an avalanche, which, descending a very short time before our train came up, and carrying with it in its downward course trees and rocks, had effectually blocked up the line of rails. Vague rumours reached the hotel that no trains would pass for a week, that two battalions of Austrian soldiers were cutting a way through the snow, and that the avalanche was 200 yards in length, 80 feet in height, and extending across the gorge from side to side. We had no means of verifying these statements, as the telegraph wires were broken, and the officials were evidently as much in the dark as ourselves. Twenty-four hours after us the train bearing the Indian mails from Brindisi came up to the same spot, the passengers were treated as we had been, and ordered to turn out at midnight in happy ignorance of the cause of the delay. The mails were, however, sent up as near as possible to the obstruction, and thence carried over the tops of the mountains for a distance of eight miles, to a train which was waiting on the further side. The cold was intense at Brenner, and the depth of the snow confined us to the hotel. The weather, however, was only such as might be expected on the Alps, but we had good reason to fear from famine, as each successive train brought up regularly from Verona its freight of passengers, and discharged them all at Brenner, until the two hotels were full and overflowing. On the evening of the third day, as we sat at dinner, tidings came that the line was once more clear, and that a train would probably start that evening for Munich; so, thankful to quit the dull monotony of Brenner, we left by the night train, and after a delay of two days in passing through the disturbed parts of Germany, we arrived safely in England, having spent eight days *en route* from Florence. W. A. HARRIS

St. Michael's Mount

IN the last number of NATURE there appears a letter from Mr. R. A. Peacock, of Jersey, in which he attempts to prove that St. Michael's Mount, in Cornwall, was insulated in the eleventh century. To do this, he quotes the passage from Domesday relating to the lands held by the church of St. Michael, which he translates—"Keiwal holds the church of St. Michael," &c. Now this land, Mr. Peacock says, was 240 acres, but the area of the Mount is now only 30 acres, so that there are 210 acres missing, therefore it could not at that time have been an island, because, in the eleventh century, it contained at least eight times as much land as it does at present.

Unfortunately for this theory, the passage which Mr. Peacock translates "Keiwal holds the church of St. Michael" is really "The church of St. Michael holds Treiwal, or Treuthal," as it is called on p. 11, which is a manor in the parish of St. Hillary, Cornwall. H. MICHELL WHITLEY

Penarth, Truro, Feb. 6

The Zodiacal Light

IN NATURE for January 26, in the course of an interesting account of the Augusta Eclipse Expedition, by Prof. W. G. Adams, of King's College, there is a short allusion to the Zodiacal Light, which can hardly fail to be looked on by many as being, both on account of the author and the occasion, authoritative as well as important and instructive:—"At about $6^h\ 30^m$ on Monday evening (runs the article in question) we saw a brilliant display of the zodiacal light, consisting of brilliant pink streamers, stretching perpendicularly to the horizon, the planet Jupiter being just on the most brilliant streamers. Towards the north and round the horizon there were also streamers," &c.

Until assured by the author that the word "zodiacal" is not a misprint from something else, it is hardly worth while for me to point out in detail that the above description mentions almost everything which does *not* belong to the true zodiacal light, and nothing which does belong to or characterise it as hitherto known amongst astronomers. C. PIAZZI SMYTH

15, R. Terrace, Edinburgh, Feb. 1

The Reign of Law

THE following is an extract from a letter I lately received from a friend of mine who is on the Geological Survey of India:—

"The Duke of Argyll and his Council have determined that the leave-rules, which are good enough for the natives, are good enough for us, although they are not sufficiently good for educated Europeans, such as the Staff Corps and Civil Service, and this they have resolved on, in spite of the Indian Government. If we could only get fair leave and pension rules, the same as men of the same rank and education receive in the other services, I do not think I should be tempted to give up field-work."

May I be allowed to inquire whether this is a new illustration of "The Reign of Law?" TANTALUS

Misadventures in Conchology

MY experience seems to me curious; is it unique? I don't complain, I simply inquire.

1. As channel of communication for a foreign friend, I sent copies of his valuable conchological work to three public libraries in Great Britain. I know they were received. They have never been acknowledged.
2. I sent a complete set of shells, of a specially interesting field, to a foreign collection; they were delivered by a friend. They have never been acknowledged.
3. I sent (at his request) to a man of science a number of the rarer shells of a district. He acknowledged them through his clerk.
4. To a dealer at his urgent request and offer of exchanges, I sent a quantity (some hundreds) of shells. He sent me in return less than half what he had promised, selected from my list at his fancy.
5. To a scientific man in America, at his earnest request and offer of exchange, I sent a set of the shells of a district. No answer whatever.
6. I sent a unique specimen of a shell to a foreign conchologist for examination. I have never heard more of it.
7. At the earnest request of a dealer offering exchange, my brother sent a great number of Scotch glacial shells. His letter of request for desiderata was returned unopened.
8. A friend near me has more than once sent shells to German collectors, but has the same sad tale of packets sent, and no promised returns made.

Are these experiences exceptional, or is conchology fatal to conscience, or are *all* men liars? VALLE

ON THE NATURAL LAWS OF MUSCULAR EXERTION

II.

HAVING shown* that Mr. Jevons' first and third sets of experiments illustrate laws 1 and 2 of muscular action, it remains for me to apply the same laws to his second set of experiments, and to show that they also illustrate these laws.

Before doing so, a few words may be said on the subject of the *maximum* of useful effect. I have shown, from theory, that a *maximum* of useful effect is obtained in holding out weights horizontally in the hand, by using a weight which is 73 per cent. of the weight of the arm

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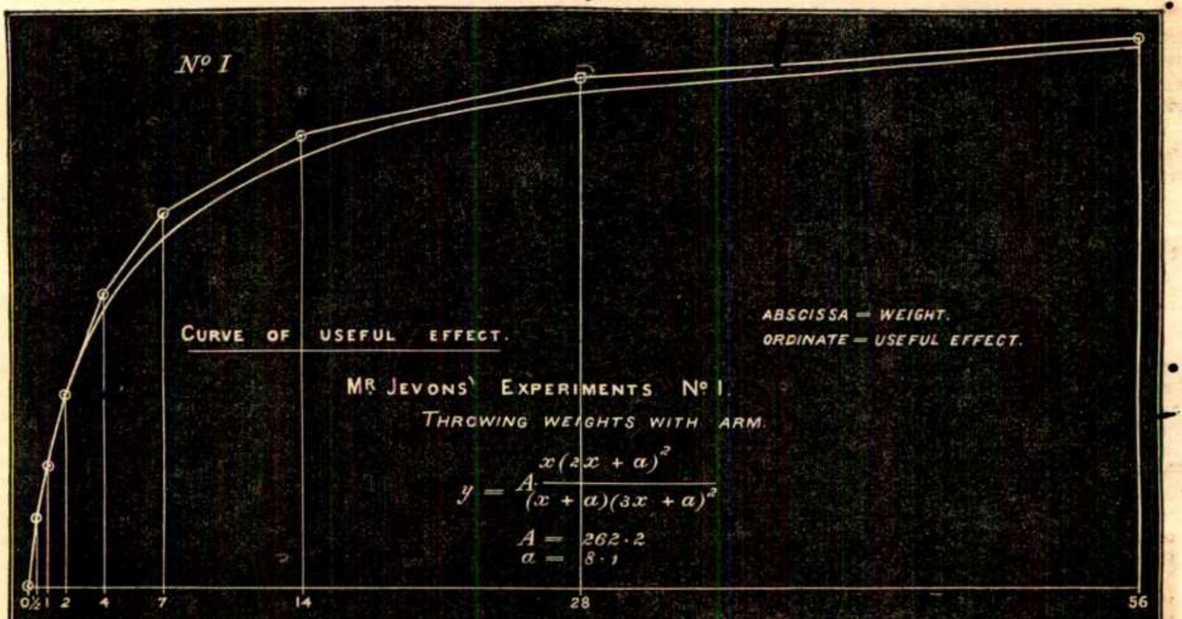
* See NATURE, vol. ii. p. 324.

Nº I



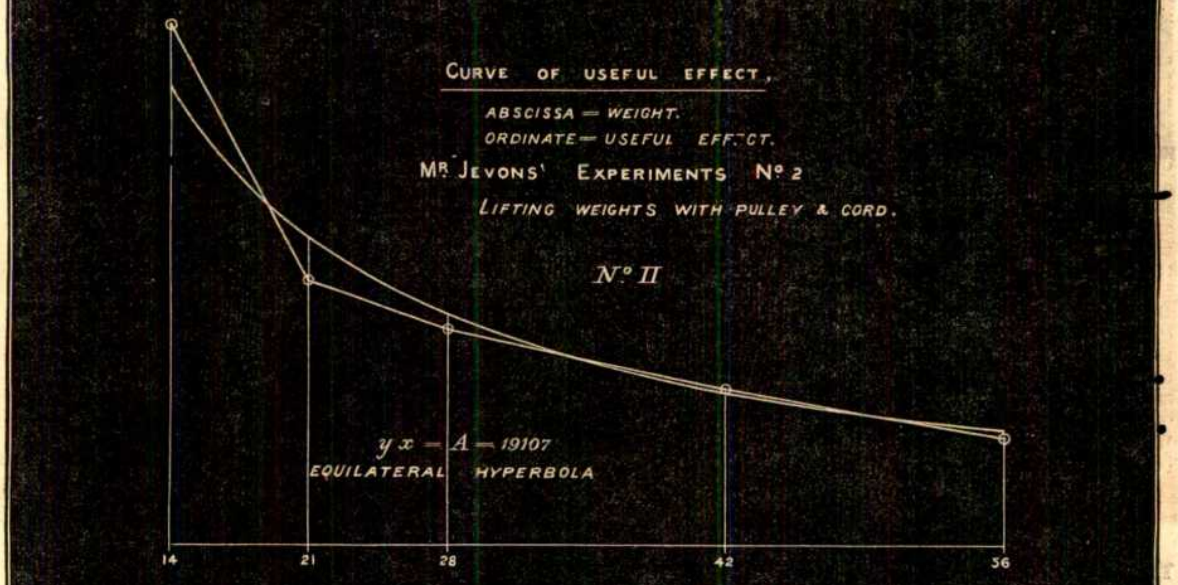
CURVE OF USEFUL EFFECT.

ABSCISSA = WEIGHT.
ORDINATE = USEFUL EFFECT.

MR. JEVONS' EXPERIMENTS Nº I.
THROWING WEIGHTS WITH ARM.

$$y = A\frac{x(2x+a)^2}{(x+a)(3x+a)^2}$$

$A = 262 \cdot 2$
$a = 8 \cdot 1$



CURVE OF USEFUL EFFECT,

ABSCISSA = WEIGHT.
ORDINATE = USEFUL EFFECT.

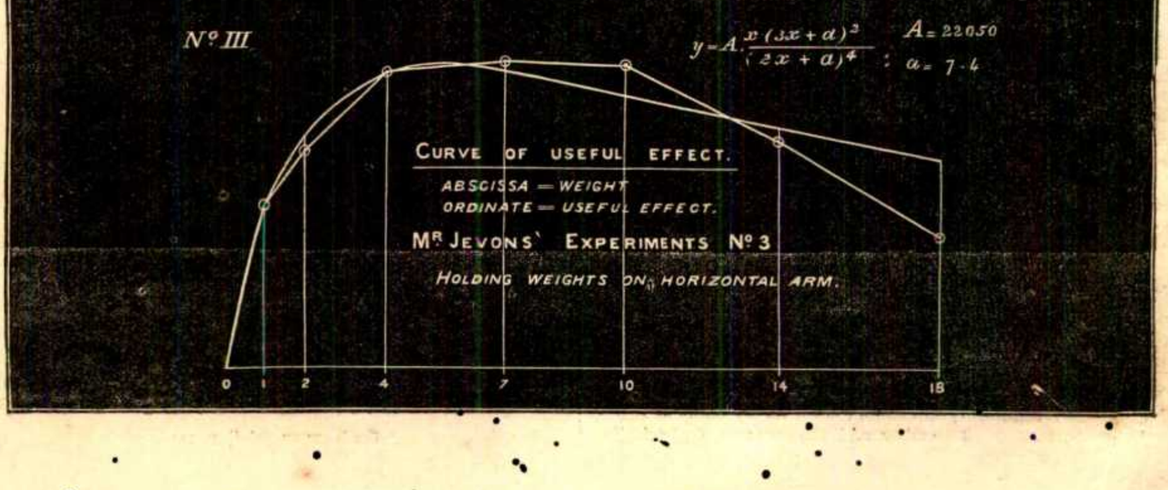
MR. JEVONS' EXPERIMENTS Nº 2
LIFTING WEIGHTS WITH PULLEY & CORD.

Nº II

$y\,x = A = 19107$
EQUILATERAL HYPERBOLA

Nº III

$$y = A.\frac{x(3x+a)^2}{(2x+a)^4} \; ; \; A = 22050 \; ; \; a = 7 \cdot 4$$



CURVE OF USEFUL EFFECT.

ABSCISSA = WEIGHT
ORDINATE = USEFUL EFFECT.

MR. JEVONS' EXPERIMENTS Nº 3
HOLDING WEIGHTS ON HORIZONTAL ARM.

itself, and this result agrees with the experiments. In throwing weights with the arm Mr. Jevons found that there was no true maximum of useful effect, and that it increases with the weight used.

It can be shown from the equations used by me, which are deduced from Law 1, that this result might have been predicted from theory.

The useful effect is proportional to

$$w \times R,$$

or to

$$w \times V^2,$$

which, by equation (2), becomes

$$w \times \frac{a^2}{l^2} v^2;$$

this expression varies as the following, by equation (3)

$$w \times \frac{(2w + x)^2}{(3w + x)^2} v^2;$$

and since, by equation (1) or Law 1, v^2 varies inversely as $w + x$; we obtain finally

$$\text{useful effect} = \frac{Aw(2w + x)^2}{(w + x)(3w + x)^2} \qquad (13)$$

The condition necessary to make this expression a maximum is

$$w(2w + x)(9w + 7x) = (w + x)(3w + x)(6w + x);$$

which reduces to the quadratic equation

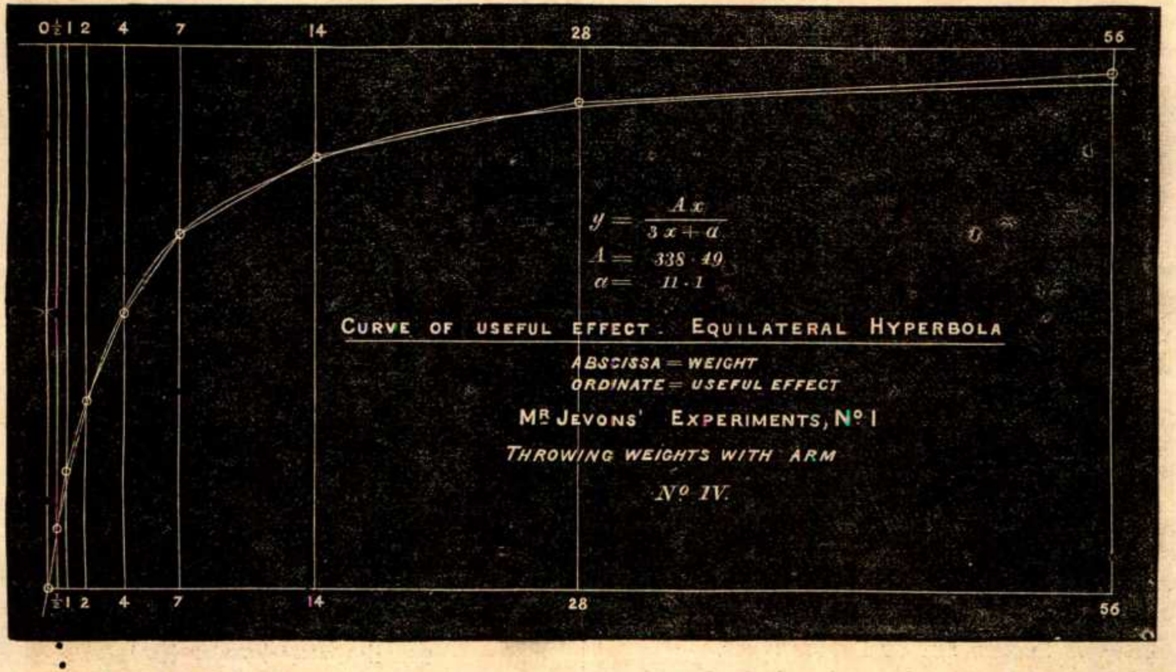
$$4w^2 + 3xw + x^2 = 0 \qquad (14)$$

This equation has imaginary roots, viz. :

$$w = x\left(\frac{-3 \pm \sqrt{-7}}{8}\right)$$

Hence there exists no real value for the weight thrown which will make the *useful effect* a maximum.

Mr. Jevons' second set of experiments consisted in raising and lowering various weights by a pulley and cord, through the convenient range of the arm, and noting the number of times the weights were raised, the rapidity of the motion being maintained constant.



The results of these experiments are

Weight.	No. of times raised.
56 lbs.	5·7
42 „	11·9
28 „	23·0
21 „	37·6
14 „	110·0

It is easy to see, on theoretical grounds, that the weight (x) of the arm will disappear from the equations that represent the work done in these experiments; for in raising the weights the work done is proportional to

$$(w - x) n,$$

and in lowering the weights the work done is

$$x n.$$

These two, added together, give for the total

$$\text{Work done} = w n.$$

I have verified this anticipation of theory by introducing x into the *ten* equations furnished by the *five* simultaneous values of w and n, and I find that the mean value of x turns out to be

$$x = 0·18 \text{lb.},$$

a value which, if the experiments were absolutely accurate, ought to become zero.

The rate of work is proportional to w, and multiplying this by the work done, in accordance with Law 2, we find

$$w^2 n = A \qquad (15)$$

This equation gives the following values of A, corresponding to the five simultaneous values of w and n :

A	w
17875	56 lbs.
20992	42 „
18074	28 „
16582	21 „
21560	14 „

The mean of these values is

$$A = 19017.$$

Solving equation (15) for n, we find

$$n = \frac{A}{w^2}. \qquad (16)$$

Substituting for A and w, in this equation, we find the following comparison of theory and observation :

w	n (observed).	n (calculated).	Difference.
56 lbs.	5·7	6·0	− 0·3 $(\frac{1}{19})$
42 „	11·9	10·8	+ 1·1 $(\frac{1}{11})$
28 „	23·0	24·2	− 1·2 $(\frac{1}{19})$
21 „	37·4	43·1	− 5·5 $(\frac{1}{7})$
14 „	110·0	97·0	+13·0 $(\frac{1}{9})$

This table is satisfactory, as the differences are less than possible errors of observation.

The *useful* effect admits of no maximum in these experiments ; for it is represented by wn, and

$$w \times wn = A ;$$

therefore—

$$wn = \frac{A}{w} ; \qquad (17)$$

which represents an equilateral hyperbola ; wn becoming infinite when the weight is zero.

The agreement between Laws 1 and 2 and Mr. Jevons' experiments, may be best shown by means of the accompanying tables and diagrams, which represent the curves of useful effect, calculated from Laws 1 and 2.

FIRST SET OF EXPERIMENTS.

Throwing weights.—It follows from equation (5) that the useful effect

$$= w R = A \frac{w (2 w + x)^2}{(w + x) (3 w + x)^2} \qquad (18)$$

Substituting in this equation, the values

$$A = 262{\cdot}2$$
$$x = 8{\cdot}1$$

we obtain the following table :—

USEFUL EFFECT NO. 1.

w	$w R$ (observed).	$w R$ (calculated).
0	0	0
½	13·2	13·6
1	23·6	23·8
2	39·2	38·2
4	58·6	55·6
7	74·3	70·1
14	90·4	86·4
28	101·6	98·5
56	108·1	106·5

In the accompanying curve No. 1, the abscissa is the weight, and the ordinate the useful effect ; and the centres of the small circles represent the actual observations.

SECOND SET OF EXPERIMENTS.

Lifting weight with pulley and cord.—In these experiments the useful effect wn may be at once calculated from equation (17), from which the following comparison is made, and Diagram No. 2 constructed.

Substituting for A, its value 19,017, we find—

USEFUL EFFECT NO. 2

w	wn (observed).	wn (calculated).
14 lbs.	1554	1358
21 „	790	905
28 „	644	679
42 „	500	453
56 „	319	340

The curve represented in Diagram 2 is a portion of an equilateral hyperbola, whose abscissa is the weight, and its ordinate the useful effect.

THIRD SET OF EXPERIMENTS.

Holding weights on hand extended horizontally.—The useful effect in these experiments may be calculated from equation (11).

$$\text{Useful effect} = w t = A \frac{w (3w + x)^2}{(2w + x)^4}. \qquad (11)$$

Substituting for A and x their values

$$A = 22050$$
$$x = 7{\cdot}4$$

we obtain the following comparison, and construct Diagram No. 3.

USEFUL EFFECT NO. 3.

w	wt (observed).	wt (calculated).
0	0	0
1	321	306
2	438	468
4	592	590
7	612	594
10	603	547
14	455	479
18	266	421

In Diagram No. 3, as before, the abscissa of the curve represents the weight, and the ordinate denotes the useful effect.

The equations of the three curves which represent the useful effect in Mr. Jevons' three sets of experiments, when expressed in Cartesian co-ordinates, are as follows :—

$$y = A . \frac{x (2x + a)^2}{(x + a) (3x + a)^2} \qquad \text{No. 1}$$

$$y x = A \qquad \text{No. 2}$$

$$y = A \frac{x (3x + a)^2}{(2x + a)^4} \qquad \text{No. 3}$$

It will be interesting, in conclusion, to explain why the simpler empirical formula, used by Mr. Jevons, coincides so nearly with the more complex formula deduced from theory.

According to Mr. Jevons' empirical formula, the useful work done in throwing weights is

$$\frac{B w}{2w + x}$$

where B = 231·4 ; and, according to my formula, deduced from theory, the useful work is

$$\frac{A w (2w + x)^2}{(w + x) (3w + x)^2}$$

where A = 262·2.

These two expressions, algebraically considered, can never become identical, but may become nearly so, if

$$(w + x) (3w + x)^2 = K (2w + x)^3, \qquad (19)$$

in which K is a co-efficient nearly constant.

Expanding both sides we have

$$9 w^3 + 15 x w^2 + 7 x^2 w + x^3$$
$$= K (8 w^3 + 12 x w^2 + 6 x^2 w + x^3) ;$$

from which we obtain four equations to determine the best value of K, viz. :—

$9 = 8 K \quad K = 1{\cdot}125$
$15 = 12 K \quad K = 1{\cdot}250$
$7 = 6 K \quad K = 1{\cdot}166$
$1 = K \quad K = 1{\cdot}000$

Mean . . 1·135

The co-efficients of the two sides of equation (19) will be most nearly equal, when $K = 1{\cdot}135$.

The constants used by Mr. Jevons and myself give

$$K = \frac{A}{B} = \frac{2622}{2314} = 1{\cdot}133,$$

which is about the value required by the preceding considerations.

The curve denoted by Mr. Jevons' equation is an equilateral hyperbola, the co-ordinates being parallel to the asymptotes, and the origin taken upon the cur ve itself,

It is easy to see, from an inspection of Diagram No. 1, that a portion of such an equilateral hyperbola would coincide nearly with the true curve, and with the points determined by experiment.

Addendum.—Mr. Robert B. Hayward, of Harrow, has kindly called my attention to an error in my application of Law 1 to Mr. Jevons' first series of experiments in which different weights were thrown by hand. In applying Law 1 to these experiments, I assumed, in equations (1, 2, 3, 4), that the total work stored up in a revolving body is that of a body of equal weight, moving with the velocity of the centre of *oscillation*. I should have stated that it is the work of a body of equal weight moving with the velocity of the centre of *gyration*. If this error be corrected, the four equations become much simpler in form, and lead directly to the empirical formula used by Mr. Jevons, and also furnish a curve which corresponds very well with the experiments.

Let k denote the radius of gyration, and let the other symbols remain as before; we find the following equations, which are to be used instead of the first four already given.

$$(w + x)\, v^2 = \text{Const.} \qquad (1)$$

$$v = V\frac{k}{a} \qquad (2)$$

$$\frac{k^2}{a^2} = \frac{1}{3}\cdot\frac{3w + x}{w + x} \qquad (3)$$

$$(3w + x) \times R = A \qquad (4)$$

Mr. Jevons' eight experiments furnish twenty-eight equations to determine the value of x, and the mean of all the values is

$$x = 11{\cdot}1\text{lb.}$$

This value of x, when substituted in the eight equations furnished by (4), gives us the following values of A.

w	A
56 lbs.	329·5
28 „	351·8
14 „	364·2
7 „	338·9
4 „	337·5
2 „	318·9
1 „	325·0
½ „	342·1
Mean	338·49

If the mean values of A and x be used in equation (4), we find the following comparison of theory and observation:—

w	R (*observed*)	R (*calculated*).	*Difference.*
56 lbs.	1·84 feet	1·89 feet	−0·05 feet
28 „	3·70 „	3·56 „	+0·14 „
14 „	6·86 „	6·37 „	+0·49 „
7 „	10·56 „	10·55 „	+0·01 „
4 „	14·61 „	14·65 „	−0·04 „
2 „	18·65 „	19·79 „	−1·14 „
1 „	23·05 „	24·01 „	−0·96 „
½ „	27·15 „	26·86 „	+0·29 „

The curve of useful effect, deduced from equation (4) is

$$wR = \frac{Aw}{3w + x}.$$

This curve represents an equilateral hyperbola, whose equation, referred to its asymptotes as axes of co-ordinates, is

$$xy + 417{\cdot}4 = 0.$$

Diagram No. 4 shows this hyperbola, while the centres of the little circles show the several experiments.

This hyperbola, derived from the property of the centre of gyration, represents the observations very well; and it nearly coincides with the more complex curve derived from the centre of oscillation, shown in Diagram No. 1.

Mr. Jevons' empirical formula corresponds at once to equation (4), provided the quantity (q) be used to denote *one-third* of the weight of the arm instead of *one half*. His formula becomes, multiplying above and below by 3,

$$R = \frac{347{\cdot}1}{3w + 11{\cdot}7}$$

The weight of the arm and shoulder used in throwing weights, viz. 11·1lb. or 11·7lb., is considerably greater than the weight of the simple arm, 7·4lb., a result quite consistent with the fact that the scapular muscles move with the arm in this kind of exertion.

S. HAUGHTON

NOTES

AT a preliminary meeting held at King's College on Monday last, the Rev. Dr. Barry, Principal, in the chair, it was resolved that a fund for a Memorial to the late Dr. Miller, in connection with King's College as having been for thirty years the principal sphere of his labours, be at once raised by subscriptions in the College, and from the general public. It was further resolved to form a committee for the purpose of carrying out this resolution, and as soon as the committee is complete a meeting will be summoned to adopt such further measures as are necessary.

WE are glad to hear that Prof. Frankland has accepted the Presidency of the Chemical Society for the ensuing session.

A CIRCULAR has been issued by the Science and Art Department of the Committee of Council on Education, South Kensington, directing that in future all teachers certificated by the Education Department shall be allowed to earn payments on the results of the instruction of their pupils in Stage I. of Mathematics, according to the rules of the Science Directory, without having previously passed the qualifying examination required by § XXXIII. The Department has also issued the following amended programme of the examinations for the diplomas of the Royal School of Naval Architecture and Marine Engineering, which is to come into force at the examination to be held in April 1872, the examination in April 1871, being held according to the programme contained in the existing Directory of the School:—"Diplomas will be given to all persons, whether they have received their instruction at the school or not, who pass the final examinations of the school, provided that they give satisfactory evidence of having gone through the course of practical work recommended by the Council of the Institution of Naval Architects. These diplomas will be of two grades, according to the success of the candidate in the examination, the title of the higher grade being Fellow, and of the lower, Associate, of the Royal School of Naval Architecture." These examinations will be held annually towards the end of April. Candidates who have not been students of the school will be required to produce certificates that they have been engaged for three years, at least, in—(1) practical wood or iron ship-building in a dockyard; or (2) practical engine and boiler building in a dockyard, or in the works of a marine engineer; or (3) practical work as a draughtsman in a dockyard (or marine engine works), during which the candidate must have himself gone through the complete formation of the design of a ship (or of a marine engine), with the whole of the calculations included in it. Such candidates will also be required to give references as to character and good conduct before they are admitted to the examination. All such candidates must apply to the Secretary, Science and Art Department, South Kensington, W., not later than the 15th March in each year.

IN our notice last week of the attainments of the Senior Wrangler, we should have included the statement which we made some time since, that Mr. Hopkinson took the degree of D.Sc. at the University of London, in 1870, in two distinct branches, viz. Mathematics and Physical Optics, this being the first similar instance on record. Dr. Hopkinson has also attained the additional distinction which so often falls to the lot of Senior Wranglers, that of First Smith's Prize-man. The Second Smith's Prize has been awarded to Mr. Temperley, of Queen's College, bracketed fourth wrangler. It is stated that Dr. Hopkinson, being disqualified as a Nonconformist from sharing in the emoluments of the University, intends following his father's profession of engineer.

THE following are the Council and Officers of the new Anthropological Institute of Great Britain and Ireland:—President: Sir John Lubbock, Bart., F.R.S.; Vice-Presidents: Prof. Huxley, F.R.S., Prof. Busk, F.R.S., Mr. John Evans, F.R.S., Dr. Charnock, Dr. Barnard Davis, F.R.S., Mr. George Harris; Director: Mr. C. Staniland Wake; Treasurer: Mr. J. W. Flower, F.G.S.; Council: H. G. Bohn, Colonel Fox, Dr. Hyde Clarke, W. Blackmore, W. Boyd Dawkins, F.R.S., R. Dunn, David Forbes, F.R.S., T. M'K. Hughes, Dr. A. Campbell, S. E. Bouverie Pusey, W. C. Dendy, Sir D. Gibb, Bart., Dr. R. King, Capt. Bedford Pim, R.N., Rev. D. I. Heath, Dr. J. Beddoe, Dr. George Harcourt, Joseph Kaines, F. G. H. Price, and C. Robert des Ruffières. Secretary: J. F. Collingwood; Sub-editor of Journal: F. W. Rudler. A Special General Meeting of the Anthropological Society will be held at the rooms, No. 4, St. Martin's Place, on the 14th February, at half-past seven o'clock, for the purpose of authorising the trustees of the Society to transfer its funds and effects to the trustees of the "Anthropological Institute of Great Britain and Ireland," in pursuance of an agreement for union under that name between the Anthropological and the Ethnological Societies of London. On the same evening, at eight o'clock, will be held the first ordinary meeting of the Anthropological Institute, when Sir John Lubbock, Bart., President, will read a paper "On the Development of Relationships."

THE Anniversary Meeting of the Royal Astronomical Society will be held to-morrow at three, and that of the Photographic Society on Tuesday next at eight.

THE following lectures on Comparative Anatomy are to be delivered in the Theatre of the Royal College of Surgeons on Mondays, Wednesdays, and Fridays, at four o'clock, commencing on Friday, Feb. 17, by W. H. Flower, F.R.S., Hunterian Professor of Comparative Anatomy and Physiology. Eighteen lectures on the Characters, Structure, Functions, and Modifications of the Teeth and Allied Organs in the Mammalia. Essential characters and structure of teeth. Development and succession of teeth. Classification and nomenclature of teeth—Dental formulæ. Modifications of the characters of the teeth in the different groups of the mammalia. Teeth of Man. Teeth of *Simiina*—Old-World Monkeys and New-World Monkeys. Teeth of *Lemurina*. Teeth of terrestrial *Carnivora*—Dogs and allied forms; Cats and allied forms; Bears and allied forms. Teeth of *Pinnipedia*—Sea-Bears and Seals; Walrus. Teeth of *Insectivora*—Hedgehogs, Moles, Shrews, &c.; Galeopithecus. Teeth of *Chiroptera*—Frugivorous Bats, Insectivorous Bats, Blood-sucking Bats. Teeth of *Rodentia*—Hares, Guinea Pigs, Porcupines, Rats, Squirrels, &c. Teeth of *Cetacea*—Odontocetes or Toothed Whales; Dolphins, Porpoises, Narwhal, Sperm-whale, Ziphius, and allied forms; Zeuglodonts; Mystacocetes or Whalebone Whales, rudimentary Teeth; Structure and function of baleen or whalebone. Teeth of *Ungulata*—Perissodactyles: ancient and modern forms; Palæotherium, Horse, Rhinoceros, Tapir; Artiodactyles: Pigs, Hippopotamus, Anoplotheridæ, Camels, Chevrotains, and Pecora (Deer, Giraffe, Antelopes, Sheep, Goats, and Oxen). Teeth of Hyrax, of Toxodon, of Typotherium, and other anomalous forms. Teeth of *Proboscidea*—Elephant, Mastodon; Dinotherium. Teeth of *Sirenia*—Dugong and Manatee. Teeth of *Edentata*—Sloths, Anteaters, Armadillos. Teeth of *Marsupialia*—Opossums, Thylacine, Dasyures, Perameles, Phalangers Kangaroos, Wombats; Fossil Marsupials. Value of dental characters in drawing inferences as to the affinities and habits of extinct animals. Horny teeth of *Monotremata*—Ornithorhynchus. The lectures are free to all who take an interest in the subject.

INTELLIGENCE has been received of the death of Dr. F. A. G. Miquel, Professor of Botany at the University of Utrecht and Director of the Botanic Gardens at Leyden, one of the few remaining distinguished systematic botanists on the Continent, and greatly esteemed by all scientific visitors to Holland for his generous hospitality and kindly nature. He was the author of several monographs, and had written largely on the botany of the Netherland-Indies, especially Java and Surinam; and also on that of Japan, which has been much worked out by Dutch naturalists; but will be best known by his magnificent "Annales Musei botanici Lugduno-batavi," in four folio volumes, with splendid illustrations. He was one of the foreign members of the Linnean Society of London.

A STATEMENT having appeared in the *Globe* that the M.A.'s of the University of London have made a representation to the authorities that the examinations for degrees are too stringent, and that it is probable that the standard will be lowered; we are authorised to state that the report is entirely incorrect; no such representation has been made, and no proposal for lowering the standard of the degrees is under the consideration of the Senate of the University.

IN consequence of a report made to the Senate of the University of London by the Examiners in Physiology, setting forth the insufficiency of the present *viva voce* examination as a test of the practical acquaintance of candidates with histology and practical physiology, an important alteration has been made in the regulations of the First Bachelor of Medicine Examination. The alteration will take effect in the year 1872 and subsequent years, and the Registrar has been directed to notify to the various medical schools in connection with the University, that the Candidates for the first Examination in Bachelor of Medicine will be required to pass a practical examination in Histology. The requisite provision is to be made in the University building for carrying out a plan which has been suggested by the Examiners, and approved of by the Senate, and of which the following outline is given by the *British Medical Journal*:—The candidates are to be examined in convenient batches, placed in a suitable room, fitted up with microscopes, glasses, reagents, needles, scissors, and razors. Each student is to have placed before him a few characteristic mounted specimens, and portions of fresh tissue or tissues prepared for minute examination, all numbered and carefully selected, and to be given three hours in which to examine and report upon the specimens presented to him, as well as to mount certain specimens of particular organs or tissues, as may be required by the Examiners. In this way, it is felt, the possession of mere book-knowledge by the candidate will be at once disclosed, and the physiological examination will be made a real and satisfactory one.

A ZOOLOGICAL RECORD ASSOCIATION has been established for the purpose of continuing the *Record of Zoological Literature* (an annual volume, containing an abstract of, and an index to, all that has been done in zoology during the previous year), which has been held in such high esteem by working zoologists that for some time past the British Association has been induced to vote an annual grant of 100*l*. in its support. Owing, no doubt, to the fact of its utility not being sufficiently known to the public, the undertaking has not hitherto proved a financial success. The new Association, which includes, we understand, all the leading zoologists of the country, hopes to have better luck, and in a few days it is expected that its programme will be before the world. Mr. Stainton, F.R.S., is the Secretary.

THE New York Association for the Advancement of Science has recently elected officers for the ensuing year, and the *American Chemist* notes that on looking over the list it finds that, among the names of fifteen officers, it does not recognise a single man of science. Six are clergymen, three are physicians. Though it is pleasant to see these men anxious to promote the cause of science, a little sprinkling of scientific men would tend to increase the confidence of the public in the discussions of the society.

THE *American Chemist* states that Prof. William Griffis, of Rutgers College, has gone to Japan as Professor of Chemistry and Natural Philosophy in the University of Japan.

THE following scientific appointments to American Colleges have recently been announced:—Mr. George C. Swallow, to the chair of Agriculture in the University of Missouri; Mr. Charles H. Wing, to the Professorship of Applied Chemistry, Dr. Alonzo M. Chase, as Instructor in Analytical Chemistry, and the Rev. J. J. Brown, to the chair of Physics, in Cornell University; Mr. T. H. Staver has been elected Professor of Agricultural Chemistry at Harvard University; and Mr. Albert H. Chester has been appointed to the Childs Professorship of Agricultural Chemistry in Hamilton College.

THE Legislature of Colorado, at its last session, appropriated thirty-nine hundred dollars for a building for a school of mines. The corner-stone of that building was laid on Monday, August 15, 1870, by Bishop Randall, in the presence of a large assembly of people.

THE *Scientific American* states that the Board of Trade of the city of Buffalo has obtained a franchise and organised a company to be styled the Oxyhydrogen Gas Company, having for its object the introduction of the oxyhydrogen gas light into that city. A committee of investigation has visited the oxygen gas works in New York, and with the information thus obtained we are informed that the work is to proceed at once. It would appear that Buffalo is to be the first city in America to adopt this splendid light. The experiment is an important one, and its success will be watched with considerable interest by gas-consumers in this country and America.

IT may be interesting to record that during the two mid-winter months of December 1870 and January 1871, the average temperature of the air at Blackheath was, according to Mr. Glaisher's tables, exactly 5° F. below the mean of the last fifty years. On 46 days the temperature was below the average, and above the mean on only 16 days, the mean for the 24 hours having been below the freezing-point on 27 days. The lowest average was on December 25th, when it fell as low as 18·9°, or 18 7° below the mean of the last fifty years. During nearly the whole of January the weather was also exceptionally warm in the neighbourhood of London; in the midland and eastern counties; the depression would be considerably greater.

THE *American Chemist* for January announces that it commences its existence with that number as an independent journal, devoted to Theoretical, Analytical, and Technical Chemistry. The articles, both extracted and original, among which is a curious one on Assaying one hundred and thirty years ago, promise a supply of varied and useful information. It proposes to present its readers with abstracts of all papers published in America or elsewhere, which involve chemical principles in their discussion.

THE *British Medical Journal* states that two additional cases illustrating the efficacy of the injection into the veins of ammonia in snake-bite, on the plan of Professor Halford, are published in the Melbourne papers. They are reported with great decision of statement as to the immediate benefits conferred, but without the scientific precision of detail which is needed, and which will, we hope, be forthcoming.

A MEETING was held in the Town Hall, Stranraer, on Friday, the 5th January, to discuss the advisability of establishing cheese factories in the county on the American system, and a committee was appointed to inquire into the details regarding the management of the Derbyshire cheese factories, and to report at a future meeting.

M. LITTRÉ has received a letter from M. Gambetta, praising him highly for his invaluable works, and nominating him as Professor of History to the Bordeaux Polytechnic School. The letter was published by public authority in the *Moniteur*.

THE new fuel invented for Paris is more substantial than had been supposed. It is the asphalt used for coating the side-ways of the streets. The total length of *trottoirs* is about two million yards, the breadth two yards, and the thickness half an inch, so that the cubic measurement of asphalt ready for use is 555,550 cubic yards. It is mixed with about half of its weight of sand, which reduces the real weight of asphalt to 277,777 cubic yards. It is difficult to burn asphalt without being suffocated with smoke. But all these drawbacks can be met successfully by scientific skill and ability. M. Le Troost, director of the Grenelle Gas Works, has erected furnaces for using tar as fuel in the distillation of tar. Tar is sufficient without the help of any other fuel to prepare gas for ordinary purposes or for inflating balloons. These furnaces were working successfully from the beginning of November, and this success has probably led to the idea of using asphalt as fuel for ordinary purposes.

ONE rather singular invention for remedying the actual want of fuel in private houses became very popular in Paris during the siege. They prepare cylinders of clay impregnated with bituminous substances; these combustible cylinders are used like the ordinary charcoal which is necessary in Parisian cookery. The earthy matters, of which the proportion is not greater than necessary, remain in the furnaces like ashes left by the combustion of charcoal. It is stated that it is proposed to continue the use of this kind of artificial fuel.

THE *Academy* states that the great attraction of the geological portion of the St Petersburg Museum consists in one entire gallery of mammoth remains, probably including many nearly entire skeletons, the bones of all of which are in a remarkably fresh state of preservation. Some years since (owing to the exertions of Sir Antonio Brady), an almost entire mammoth's skull, with tusks of colossal proportions, was obtained at the brick-pits at Ilford, in the Thames Valley brick-earth, and now adorns the geological gallery of the British Museum. Since the appointment of M. Dupont to the post of keeper in the Brussels Museum, the almost entire skeleton of a mammoth, found some time since, but neither repaired nor mounted, has been diligently mended up bit by bit, till they are able now to boast the possession of the best example of *Elephas primigenius* out of St. Petersburg. One is struck with the comparative height and slenderness of the mammoth's skeleton as compared with the modern elephant, and it seems probable that he was a more active and a lighter-built beast than the Asiatic or African species. It is to be hoped that the British Museum collection will ere long be enriched by the addition of a skeleton of the Siberian mammoth. The execution of the fortifications around Antwerp has led to the discovery, not only of elephants' and mastodons' remains, but of a most wonderful series of cetacean bones. These are now arranged in a fine gallery in the Brussels Museum, which, under the direction of M. A. de Borre, bids fair to be one of the most attractive of Continental institutions.

SHOCKS of earthquake were felt in Scinde on the 12th November.

ON the 24th October at 1.40 A.M. a smart shock of earthquake was felt at Yokohama, with a deviation from E. to S.W. The shock was remarkable for long duration.

THE NATURE OF THE EARTH'S INTERIOR*

In a previous discourse on volcanoes,† I directed attention to the phenomena of volcanic action, specially considered in relation to the part which such igneous or internal forces have played in determining the grand features of the external configuration of the sphere upon which we live.

If now we follow up this subject still further, it will naturally lead to an inquiry into the nature of the internal substance of the globe itself, within which the foci of such agencies must be situated; quite independent of this, however, I have little doubt but that many of you must at some time or other have already asked yourselves the question—What does the central mass of the earth beneath us consist of?

The answer which, in the first instance, would, I imagine, be most likely to suggest itself to your minds, would be that it consisted of solid stone, such as you see forming the body of its mountains, the foundations of its continents, and the rock basins which contain its seas. The belief in such an hypothesis would, however, be rudely shaken by the first personal experience of the shock of an earthquake, the sight of a volcano in eruption, or the consideration of the immense faults which have dislocated many parts of the solid land; since, so far from disposing us to regard the ground under us as entitled to the appellation of the *terra firma*, so commonly used by the ancients, the study of such phenomena could not but suggest grave doubts in our minds as to whether the earth was after all anything like so solid or stable as we at first sight felt inclined to imagine.

But very little inquiry into the subject is necessary, however, to convince any one of the great difficulties in the way of obtaining a satisfactory answer to this question, and to prove that at present we do not have at our command sufficient data or evidence to enable us to arrive at a thoroughly conclusive solution of this most interesting problem.

As the rapid advances made by the natural sciences are, however, daily adding to our information on this subject, and thus enabling us to correct or modify our previous deductions, so as to form a more and more trustworthy opinion on the nature of those parts of our globe, which, from their position, must always remain inaccessible to our powers of direct observation, I have imagined that a short sketch of the present state of our knowledge concerning the probable constitution of the interior of the earth might prove interesting.

In treating this subject, we will first consider what has already been done in the way of direct examination of the earth's substance in depth; yet when it is remembered that the mean diameter of our planet is some 7,912 miles, whilst the greatest depth hitherto attained by man's direct exploration has not even yet reached one mile from the surface downwards; this disproportion appears so enormous as to render it self-evident, in the pursuit of this inquiry, especially as regards the more central portions of the earth, that we must in the main rely upon data furnished by calling in the aid of the natural sciences. The direct examination of the exterior of the earth, even when restricted to this depth, does nevertheless furnish us with many important data from which to start in this to a great degree speculative inquiry, and to some of these we shall now direct attention.

It must first of all be remembered that all the rocks which we meet with at the surface, and which compose so much of the solid exterior of our globe as is actually known to us, may be arranged under two principal heads, viz., the volcanic or endogenous rocks, *i.e.* those formed within the body of the earth itself, and the sedimentary or exogenous, *i.e.* those rocks formed, or rather reconstructed upon its surface, out of the *débris* of previously existing rocks arranged in beds or strata by the mechanical action of water.

It was long taken for granted by geologists that the lowest sedimentary strata, in their normal or in a more or less altered condition, rested directly upon granite, which was for a long time regarded as the foundation upon which they, in the first instance, had been deposited, since this rock was looked upon as the oldest of all, and as representing the primeval or original surface covering of the earth. Later researches, however, have proved this hypothesis to be untenable, since it appears that no instance of a granite has as yet been met with in nature, which if followed up, does not at some place or other break through, and alter or disturb more or less, the stratified rocks in immediate contact with it, so that it naturally follows that such stratified rocks must have preexisted on the spot, or in other words, that they were older in geological chronology than the granite which came to disturb them.

In the present state of geology, however, it is utterly impossible for us to point out any variety of rock whatsoever as the one which may have served as a foundation upon which the oldest sedimentary rocks were originally deposited, in fact the oldest rocks which we know of at present are themselves sedimentary rocks (mostly in an altered condition) belonging to the Laurentian series in Canada, and as yet it has not been found out what these sedimentary beds may in their turn rest upon, *i.e.*, what is actually below them.

As therefore we have not as yet been able to reach down to investigate directly any rocks lower in the geological series than those pertaining to the Laurentian formation, we will now turn to the volcanoes, in order to examine the mineral products which they bring up for our consideration, from depths vastly beyond those which we can ever hope to reach directly. What volcanoes teach us with regard to the nature of the earth's interior, at a depth from which they derive their supply of mineral matter, may be summarised as follows:—

That at this depth, the earth's substance exists in a state of perfect molten liquidity, forming as it were a sea of molten rock, analogous in character to the eruptive rocks which have broken through the earth's crust in former times. Secondly, that the mineral products ejected from volcanoes are very similar to one another in chemical and mineral constitution, no matter from what part of the globe they may emanate from. And, lastly, that from the same volcanic orifice, and during the same eruption, lavas of two totally different classes may be emitted, viz., the light, acid, or trachytic lava, analogous to the granites, felsites, &c., of the oldest period, and the heavy basic or pyroxenic lava, all but identical with the dark basaltic or trappean rocks commonly met with as dykes, &c., disturbing most of the different sedimentary formations.

Another deduction from the study of volcanic phenomena, indicating that at a certain depth below their surface they must be in connection with a continuous sea of molten lava, is based upon the influence which the moon appears to have on volcanic eruptions; this opinion seems to have been confirmed by the observations of Prof. Palmieri made during the last eruption of Vesuvius, on which occasion he reported that distinct tidal phenomena could be recognised, thereby indicating that the moon's attraction occasioned tides in the central zone of molten lava, in quite a similar manner as it causes them in the ocean. A further corroboration of this view is seen in the results of an examination of the records of some 7,000 earthquake shocks which occurred during the first half of this century, compiled by Perry, and which, according to him, demonstrate that earthquakes are much more frequent in the conjunction and opposition of the moon than at other times, more so when the moon is near the earth than when it is distant, and also more frequent in the hour of its passage through the meridian.

Returning now to the more direct examination of the superficial parts of the earth, we find that the results of mining operations have also thrown considerable light, not only on the mineral nature of the rocks encountered in depth, but also upon some of their physical conditions. A numerous set of experiments made in deep mines in various parts of the world, often far distant from one another, has most conclusively proved that the temperature of the earth, at least as deep down from the surface as has been explored by man, increases in direct ratio as we descend towards its centre. Other observations on the temperature of the water from deep-seated and hot springs, and from artesian wells, fully confirm the experiments made in mines, and show that the temperature of the water furnished by them also becomes higher in proportion to the depth of the source from which it is derived.

As might naturally be expected, the interference of local causes renders it a matter of considerable difficulty to determine the true mean general rate of such increase in temperature of the earth's substance downwards; still, in the main, observers all agree in placing it at somewhere between $1\frac{1}{2}^\circ$ and $2\frac{1}{2}^\circ$ degrees F. for every hundred feet in depth, so that we cannot be far wrong, if for our purpose we estimate it at 2° F. for every hundred feet in depth, or a rate which amounts to 121° for each geographical mile nearer the earth's centre. Since no facts are at the present time known which can in any way invalidate the supposition that this or a somewhat similar rate of increase in temperature holds good in still greater depths, it is perfectly correct and justifiable reasoning to assume that such is actually the case, and therefore a simple calculation will show that at a depth of about twenty-five geographical miles from the surface downwards a temperature of about 3,000° F. should be attained, which would represent a heat at which iron melts, or one sufficient to keep lava in a state of perfect molten liquidity at the surface of the earth. As it must be remembered, however, that at this depth the substance of the earth would be exposed to the pressure of the superincumbent mass, and as it has been shown by experiment that many substances become more refractory—*i.e.*, require a greater degree of heat to melt them—when exposed to pressure than when at the surface, the above calculation will have to be modified considerably in order to meet this condition. Unfortunately, we have not as yet sufficient data at command to enable us to estimate the true ratio in which the melting points of such rocks would become elevated by pressure; yet we may safely take it for granted, after allowing far more than the maximum rate of increase, deduced from the experiments of Bunsen and Hopkins, that we should not require to sink so deep again in order to attain a temperature fully sufficient to keep such substances in a state of fusion, or, in other words, this deduction necessitates the supposition that the solid rock crust of the earth cannot, at the utmost, be more than 50 miles in thickness.

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* Substance of a Lecture delivered for the Sunday Lecture Society on January 29.

† See NATURE, vol. ii. p. 283.

If we now reason from the above data as premisses, it will follow as a natural consequence that our globe must in reality be a sphere of molten matter surrounded by an external shell or crust of solid matter, of very insignificant thickness when compared to the diameter of the entire globe itself; or, in other words, this deduction represents exactly such a state of things as would ensue in the event of a sphere of molten matter becoming consolidated on its exterior by the cooling action of the external atmosphere; and the figure of the earth itself, which is an ellipsoid of revolution, *i.e.*, a sphere somewhat flattened in at the poles, but bulging out at the equator, being that which a plastic mass revolving round its own axis would assume, is regarded by natural philosophers in general as all but conclusive evidence, that the earth at an early period of its history must have been in a fluid condition.

Although the doctrine that the earth is a molten sphere surrounded by a thin crust of solid matter was all but universally taught by geologists, there have of late years been brought forward several arguments to the contrary, which apparently are more in favour of its being a solid or nearly solid mass throughout, and these arguments are fully entitled to our consideration, as our object is not to defend any particular theory, but to arrive as nearly as we can at the truth. I will, therefore, in the first place proceed to scrutinise all which has been brought forward in opposition to the older hypothesis, and then to consider whether any other explanation yet advanced is more in accordance with the facts of the case.

First of all we have to answer the question as to whether it is possible for such a thin crust to remain solid, and not at once to become melted up and absorbed into the much greater mass of molten matter beneath it? This would doubtless be the case, if the central fluid mass had any means of keeping up its high temperature, independently of the amount of heat it actually possessed when it originally assumed the form of an igneous globe. This question, however, in reality answers itself in the negative, since it is evident that no crust could even commence to form on the surface, unless the sphere itself was at the moment actually giving off more heat from its outer surface to the surrounding atmosphere than it could supply from its more central parts, in order to keep the whole in a perfectly fluid condition, so that when once such a crust, however thin, had formed upon the surface, it is self-evident that it could not again become melted up or re-absorbed into the fluid mass below.

This external process of solidification due to refrigeration would then continue going on from the outside inwards, until a thickness of crust had been attained sufficient to arrest or neutralise (owing to its bad conductibility of heat) both the cooling action of the surrounding air and the loss of more heat from the molten mass within; and thus a stage would soon be arrived at when both these actions would so counterbalance one another that the further cooling down of the earth could be all but arrested: a condition ruling at the present time, since the earth-surface at this moment, so far from receiving any or more than a minute amount of heat from the interior, appears to depend entirely, as regards its temperature, upon the heat which it receives from the sun's rays.

We have next to consider the argument that, if the earth's exterior was in reality only such a thin covering or crust, like the shell of an egg, to which it has often been likened, that such a thickness would be altogether insufficient to give to it that stability which we know it to possess, and that consequently it could never sustain the enormous weight of its mountain ranges, such as, for example, the Himalayas of Asia or the Andes of America, which are, as it were, masses of rock piled up high above its mean surface-level.

At first sight, this style of reasoning not only appears plausible, but even seems to threaten to upset the entire hypothesis altogether. It requires but little sober consideration, however, to prove that it is more, so to speak, sensational in character than actually founded on the facts of the case; for it is only requisite for us to be able to form in our minds some tangible idea of the relative proportion which the size of even the highest mountain bears to that of the entire globe itself, to convince us, if such a crust could once form and support itself, that it could with ease support the weight of the mountains also. The great Himalaya chain of mountains rises to a maximum altitude of 31,860 feet, or six miles above the level of the sea; and if the earth could be seen reduced in scale down to the size of an orange, to all intents and purposes it would look like an almost smooth ball, since even the highest mountains and deepest valleys upon its surface would present to the eye no greater inequalities in outline than the little pimples and hollows on the outside of the skin of an ordinary orange. If this thin crust of the earth can support itself, it is not at all likely to be crushed in by the comparatively speaking insignificant weight of our greatest mountain chains, for in point of fact it would be quite as unreasonable to maintain such a supposition, as to declare that the shell of a hen's egg would be crushed in by simply laying a piece of a similar egg-shell upon its outside.

That a very thin spheroidal crust or shell enclosing a body of liquid matter such as an ordinary fowl's egg, does possess in itself an enormous degree of stability and power to resist pressure from without, is easily demonstrated by merely loading a small portion of its surface with weights as long as it does not give way under them. Even when placed on its side (or least strong position) it was found that a portion of the shell only one quarter of an inch square would sustain several pounds weight without showing any symptoms of either cracking or crushing; or, in other words, this simple experiment indicates that if the external crust of the earth was but as thick and strong in proportion as an egg-shell, it would be fully capable of sustaining masses equal in volume and weight to many Himalayas piled up one atop of another, without any danger whatever to its stability.

The next argument which has been advanced against the probability of the major part of the earth's substance being in a fluid condition, is one based altogether upon astronomical considerations. It having been demonstrated when two clocks are set agoing, the pendulums of which are similar to one another in all respects except that whilst the bob of the one is solid, that of the other is hollow and filled with mercury, that the latter will swing somewhat faster, and consequently the clock gain time upon the former. The late Mr. Hopkins, of Cambridge, applied this observation to the consideration of movements of the earth in space, and by a very elaborate course of mathematical reasoning and calculation, demonstrated that the earth, if not quite solid, must be nearly so, since according to his results, if it was merely a comparatively thin shell filled with liquid matter, the ratio of certain of its movements (the precession or nutation) would differ considerably from what they are actually known to be, and these conclusions appeared to be confirmed by the subsequent calculations of Sir William Thomson and Archdeacon Pratt. Although grave doubts suggested themselves as to the correctness of the values used in these calculations for two of their most important elements, viz., the condensing action of pressure and the expanding action of the very high temperatures within the globe—both of which have not as yet been determined with any certainty, and although it might also be surmised that the conditions of a pendulum bob of polished glass filled with heavy slippery mercury swinging at the end of a rod must be extremely different from those of a nearly spherical globe filled with viscid sticky lava revolving around its own axis; still geologists felt themselves quite unable to answer the arguments of the astronomers and mathematicians, and since none of them appeared to be sufficiently versed in either astronomy or mathematics to be able to submit the method of reasoning or the calculations to any strict scrutiny, they felt themselves, reluctantly no doubt, compelled to bow to the decision of such eminent authorities.

So stood the matter until the summer of 1868, when, fortunately for the advancement of this inquiry, M. Delaunay, now Director of the Observatory at Paris, an authority equally eminent as a mathematician and an astronomer, was induced to undertake the reconsideration of this problem; a labour which has not only resulted in altogether reversing the above decision, and demonstrating the complete fallacy of the premisses upon which so much elaborate reasoning had been expended, but which proved conclusively by experiment that a sphere filled with liquid matter would, under circumstances such as are present in the case of the earth, behave in precisely the same manner as an entirely solid one, and, consequently, that the fact of the earth being either solid or liquid in its interior could neither have any influence whatever on the rate of precession or nutation, nor be of any use as a means of deciding as to the real or approximative thickness of the earth's crust.

It may also be added that the conclusions arrived at by Mr. Hopkins, even when supported by Sir William Thomson and Archdeacon Pratt, were not universally acquiesced in; the celebrated German physicist, Helmholtz, amongst others, was not satisfied as to their correctness, and in opposition to the deductions of Sir William Thomson that the earth's crust must be some 1,000 miles in thickness, we have the entirely opposite conclusions of Mr. Henessey, whose calculations tend to show that the earth's crust cannot be less than eighteen miles or more than 600 miles in thickness. We may now, however, fairly conclude that all the objections as yet advanced from an astronomical point of view against the theory of the fluid condition of the interior of our planet, have been invalidated or explained away.

The only other argument in favour of internal solidity is one which bases itself upon the law, announced upon theoretical considerations by Professor Thomson in 1849, that the fusing points of bodies must become more elevated when subjected to pressure, or, in other words, that under the influence of pressure, bodies will require more heat to melt them.

Starting from this, Bunsen argued that the earth could not be other than solid to the core, since the enormous pressure accumulated at the centre would cause its internal substance to become so infusible that it could not remain in a molten state. To a certain extent this law was corroborated by the experimental researches of Bunsen and Hopkins, made upon some of the easily fusible substances like wax, spermaceti, paraffin, and sulphur; but as far as the later experiments went, it was not confirmed either in the case of metallic substances, nor did it appear to hold true with other than the more easily compressible bodies.

In the case of the earth, therefore, the conclusions of Bunsen cannot be accepted, since we have to deal with materials to which, as yet, this law has not been proved to apply; still, assuming, as seems most probable, that the materials composing the earth's mass do become to some extent more and more infusible according as they approach nearer to its centre, it must, on the other hand, be remembered that this would be more or less neutralised by the expansion which these substances would undergo from the action of the internal heat; and as incontrovertible evidence has been produced to prove that the temperature of the earth downwards from the surface increases in direct ratio with the depth, it seems most probable that the combined effects of expansion and elevated temperatures would more than counteract any tendency to solidification due to the effects of pressure.

Having now taken into consideration the various objections which have been urged against the theory of the earth's internal fluidity, as well as devoted some consideration to the opposing view of its solidity, it will be noticed, if we pass in review some of the distinctive features of the two hypotheses, that the former theory is a legitimate deduction from the data afforded by the direct study of the earth itself, whereas the latter, on the contrary, instead of making the explanation of the earth's phenomena its starting point, devotes itself almost exclusively to the task of proving that it could not be fluid.

Thus, how is it possible, if the earth's mass be solid throughout, to account for the great upheavals or sinkings down of large portions of the rock formations which compose its external crust? Do not these phenomena lead to the direct inference that the external crust cannot, by any possibility, rest in depth upon an unyielding mass of matter in the solid state, but that it must necessarily be superposed upon some more or less fluid substance which by its mobility can, when some one portion of the crust above sinks down, become displaced, and so make room for it by elevating, or, as it were, floating up some other part of the same?

In like manner the hypothesis that the earth is essentially solid necessitated that the phenomena of volcanoes should be explained upon the supposition that they had their sources in numerous small isolated local basins of molten rock scattered over the surface of the globe; a view which is altogether inconsistent with the results of chemical and mineralogical investigation, which proves that the ejected products are identical in constitution even if taken from volcanic vents the most distant from one another, nor does such a theory attempt to explain the tidal phenomena of volcanic outbursts and earthquakes previously alluded to.

So far, therefore, as we have gone into this subject, we may regard the balance of evidence as proving that at a depth of about fifty miles or less from the surface, there exists a continuous zone of molten rock or lava, such as is brought up to the surface by volcanic eruptions. Let us now consider how deep this zone or stratum of molten matter is likely to extend, and also what forms the more central mass of the earth below it.

In order to answer these questions we must look to other than direct evidence, and first of all must inquire whether the consideration of the mean density, or in other words the actual weight, of the earth itself, can throw any light upon these abstruse points. The consideration of the attraction which bodies exert upon one another in the ratio of their magnitude, has enabled the physicist to effect the at first sight apparently impracticable task of determining the entire weight of the earth itself, but it is out of our province to describe the mode of doing so, and we must content ourselves by accepting as a fact the results of such investigations, which prove that the total weight of our planet is as near as possible 5½ times the weight of a similar globe of pure water. Knowing now that the mean density, or specific gravity, as it is also called, of the earth, is 5½, and also from direct experiment that the mean density of the entire solid rock forming its external crust cannot be higher than 2½, or less than half that of the entire sphere, it naturally follows that the central parts must be very much more heavy in order to account for so high a mean figure as 5½, and it has been calculated that if we suppose that the earth was composed of three concentric portions of equal thickness, each in turn increasing in density towards the centre in arithmetical progression, we should then have an outer circle of specific gravity 2½, or as heavy as rock, an intermediate zone of 12, or as heavy as quicksilver, and a central nucleus of about twenty times the density of water, or as heavy as gold.

This increase of density has sometimes been erroneously represented as entirely due to the effects of the enormous pressure of the superincumbent mass; but this supposition is quite untenable, since the tendency of all the numerous experiments made in this direction has been to prove that no substances can be compressed or condensed to an indefinite extent, since what may be termed their approximative maximum density is soon attained, beyond which the effects of pressure become so much smaller and smaller in proportion to the force applied, that at last the further condensation effected by still greater pressure is all but inappreciable. Besides this, it must not be forgot that the crust of the earth is a species of dome like the shell of an egg, which supports itself without resting or floating upon its fluid centre; and further that the earth's high internal heat, by causing the materials which compose it to expand, must also counteract the effects of superincumbent pressure, so that when all these facts are taken into due consideration, it appears quite evident that the materials which actually form the mass of the interior must be infinitely denser than any of the rocks met with on the surface, and that they must be metallic in their nature, since no other bodies are known which could at all fulfil these conditions of density.

If now we suppose that the earth's interior is composed of a series of concentric zones or layers made up of substances which are of more and more dense nature as they are situated nearer the centre, and that the external one is rock of a density of 2·5, a calculation will show that the centre or nucleus will be about 10, or as heavy as silver. If now we suppose that the zone of molten lava, which we have already concluded must exist at a depth of about 50 miles below the surface, has a density of 3, or say even 4, to give the fullest allowance for the condensing effects of superincumbent pressure, then we should find by calculation that this zone could not extend deeper than about 400 miles, since below this depth the matter would be so heavy that its density can only be explained on the supposition that it is made up of metallic compounds, and as the density of the still lower zones would continue to increase up to the very centre of the earth, the inference is that the whole of this great central mass situated at a distance of some 450 miles or less below the surface, is actually formed of metals and their compounds.

Whether this great central metallic nucleus is fluid or solid may next be inquired into. According to Bunsen's theory previously alluded to, it ought to be solid, for owing to the enormous pressure to which it would be exposed, the solidification of the molten sphere should first commence at the centre. This view would be quite correct if the earth was composed of highly compressible non-metallic materials; but since this is not the case, and since, as before alluded to, the experimental data already obtained indicate that neither the metallic nor the less compressible substances become more refractory in proportion to the increase of pressure, we are more justified in assuming that the central nucleus also must be in a fluid condition, and the more so, not only because we know that metallic compounds are as a rule infinitely more fusible than rock silicates, but also as the well-known high temperature of the earth's interior would, by its expanding action, tend to counteract the effects of the pressure.

In summing up this inquiry, the balance of evidence appears to me to be decidedly in favour of the hypothesis that the interior of our earth is a mass of molten matter arranged in concentric layers or zones according to their respective densities, and the whole enclosed within a comparatively thin external crust or shell.

DAVID FORBES

SCIENTIFIC SERIALS

THE *American Naturalist* for January opens with a long paper by Prof. J. S. Newberry "On the Ancient Lakes of Western America: their Deposits and Drainage," which is stated to be a chapter from Dr. Hayden's forthcoming "Sun-pictures of the Rocky Mountains." Prof. Newberry states that the wonderful collection of fossil plants and animal remains brought by Dr. Hayden from the country bordering the Upper Missouri has been shown, by his observations and the researches of Mr. Meek, to have been derived from deposits made in extensive fresh water lakes, lakes which once occupied much of the region lying immediately east of the Rocky Mountains, but which have now totally disappeared. The sediments that accumulated in the bottom of these old lakes show that in the earliest periods of their history they contained salt water, at least that the sea had access to them, and their waters were more or less impregnated with salt, so as to be inhabited by oysters and other marine or estuary mollusks. In due time the continental elevation which brought all the country west of the Mississippi up out of the wide-spread Cretaceous sea raised these lake-basins altogether above the sea-level, and surrounded them with a broad expanse of dry land. Between these lakes were the areas of dry land covered with luxuriant and beautiful vegetation, and inhabited by herds of elephants and other great mammals, such as could only inhabit a well-watered and fertile country. Prof. Newberry's explanations throw much light on that remarkable feature of the western side of the great continent, the canons formed by the rivers, like the stupendous one of the Colorado, nearly 1,000 miles in length and from 3,000 to 6,000 feet in depth, with almost perpendicular sides. The Rev. A. P. Peabody contributes an account of the Chinese in San Francisco; Mr. H. Willey, a paper on Lichens under the microscope, with wood-cuts which very well illustrate their mode of vegetation and reproduction; and Dr. A. P. Barnard, a description of a new form of binocular for use with high powers of the microscope. The shorter articles and *Natural History Miscellany* contain, as usual, much interesting information.

The *Journal of Botany* for February commences a series of papers which will be very useful to systematic botanists; an alphabetical catalogue of the new genera and species of plants published during 1870 in the English botanical and gardening journals, not including the "Journal of the Linnean Society." The present number only carries the list down to *Dracontium*. Mr. J. G. Baker continues his monograph of the genus *Xiphium*, and Dr. Hance contributes an article on the so-called "olives" of Southern China, which he states to be produced by two species of *Canarium*, trees from twenty to thirty feet high, largely grown in the neighbourhood of Whampoa. The stones are beautifully and elaborately carved by the Chinese as ornaments, and, when set in gold, form exceedingly handsome brooches or bracelets. Two articles of special interest to systematists are Prof. Dyer and Dr. Trimen on *Polygonum nodosum*; and Mr. W. P. Hiern on the form and distribution over the world of the Batrachian (or aquatic) section of *Ranunculus*. There is also the usual section of short Notes and Queries.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, February 2.—Prof. Williamson, F.R.S., President, in the chair. The following gentlemen were elected Fellows: R. J. Friswell, R. F. Humiston, M.D., A. H. Mason, I. R. Justin. Prof. Frankland, F.R.S., read a paper "On the Development of Fungi in Potable Water." He began by alluding to the experiments Dr. Heisch had made some months back with waters contaminated with sewage matter. When to such waters some sugar was added, very soon a kind of fermentation ensued, and a rich fungoid growth made its appearance. Prof. Frankland has now repeated and extended these experiments and arrived, with one or two exceptions, at the same results. But in the course of his researches he encountered some reactions which revealed to him that the presence of sewage matter in saccharic water is in itself not sufficient to produce fungoid growth, but that the presence of phosphates in some form is indispensable to such production. Prof. Frankland further found that the germs which give rise to the development of fungi need not necessarily come from sewage contamination, but that they may be derived from the atmosphere. Finally, he found that animal charcoal does not remove those germs. Dr. Frankland thinks that the sugar test of Dr. Heisch for the detection of traces of sewage contamination may be turned into a very delicate reagent for the detection of minute quantities of phosphates; for when these defy the power of the usual laboratory tests, they yet are capable of feeding those germs and thus giving rise to the fungoid growth. From all his observations Prof. Frankland drew the following conclusions:—1. Potable water mixed with sewage, urine, albumen, and certain other matters, or brought into contact with animal charcoal, subsequently develops fungoid growth, and other organisms, when small quantities of sugar are dissolved in them and they are exposed to a summer temperature. 2. The germs of these organisms are present in the atmosphere, and every water contains them after momentary contact with the air. 3. The development of these germs cannot take place without the presence of phosphoric acid, or a phosphate or phosphorus in some form of combination. Water, however much contaminated, if free from phosphorus, does not produce them. A German philosopher has said "ohne Phosphor kein Gedanke." The above experiments warrant the alteration of this dictum to "ohne Phosphor gar kein Leben."

Anthropological Society January 31.—Dr. Charnock, President, in the chair. A paper was read by Mr. Joseph Kaines, on some of the Racial Aspects of Music. The author, in a very brief glance at the characters of the music of the various races of men on the globe, drew particular attention to a striking anthropological fact—namely, that the music of the people of the north-east of Europe, unlike that of all the rest, was pervaded by a settled melancholy. He sought to account for this phenomenon physically and psychically. He drew attention to the climatal and general physical conditions under which the peoples of the north-east of Europe live, and suggested that, in the constant war with Nature, and the endeavour to modify Nature's laws, they acquired a gravity, awe, and sadness, of which the peoples of the sunny south knew nothing, as their music showed, Nature having used them more kindly. The author contrasted the biographies (as well as the music) of the German and Italian composers, and showed that the men differed as widely; sadness and sorrow marking the one, brightness and gladness characterising the other. He commented upon the introspectiveness of the northern peoples, and the rapt attention and morbid analysis they give to the great problems of Life, Death, God, and Immortality; and stated that the contemplation of these and such sublime mysteries saddened and brightened by turns all their thoughts and impressions. It was curious to note that even the dance tunes and popular airs of the Germans, Norwegians, and Swiss, as has been remarked by Mr. H. F.

Chorley, the eminent musical critic, were in a minor key. "Joyousness," contined the author, "is a plant that does not flourish in the bleak north. It flowers and blossoms perennially in the south, because there the air is balmy and soft. There the skies are always bright, and beneath man's feet the earth is fruitful though untilled. There Nature uses her children kindly, and even "prepares for them a table in the wilderness.'" The author remarked incidenatlly that not music only, but the other arts of expression—architecture, sculpture, and the mythologies of the north of Europe—were imbued by the same melancholy spirit. He concluded by a few observations on the character of ancient Roman, modern Anglican, and dissenting Church music. The following gentlemen took part in the discussion: Mr. Mackenzie, Dr. Hyde Clarke, Mr. Bendir, Dr. Blake, Mr. Lewis, Mr. Wake, Captain Brine, Mr. W. R. Cooper, Mr. Quaritch, and the Chairman. The President announced that this was the last ordinary meeting of the Anthropological Society, an amalgamation with the Ethnological Society having been carried out by the delegates appointed for that purpose at the general meeting of this Society on January 17th. The new society was to be styled "The Anthropological Institute of Great Britain and Ireland."

Linnean Society, February 2.—Mr. G. Bentham, President, in the chair. The President announced the death of one of the corresponding members of the Society, Prof. Miquel, of Leyden; and also that the Council had agreed to recommend the election of Prof. O. Heer, of Zurich, to fill the vacancy in the list of foreign members caused by the death of Prof. Unger, of Vienna.—"Natural History of Deep-sea Soundings between Galle and Java," by Captain Chimmo. The ooze dredged up from a depth of 2,300 fathoms, where the temperature was found to be 35° F., consisted to the extent of 90 per cent. of organic matter, Foraminifera, chiefly Globigerinæ, together with Polycistinæ, with a few broken sponge-spicules. In the shallow water near Sumatra, the animal life had decreased to only about five per cent. of the ooze, the Globigerinæ having entirely disappeared. The water brought up from great depths was found to contain a large proportion of salts in solution, which crystallised out immediately on exposure to the air. Mr. Busk remarked on the great interest and importance of the observation of the low temperature of the deep water in a latitude within a few degrees of the equator, strongly confirming the conclusions as to a general circulation of the water between the equator and the poles drawn from similar observations in the Atlantic.

Victoria Philosophical Institute, January 30.—The Rev. J. H. Titcomb read a paper on "Archæology, with some of its Parallels and Contrasts;" it was a general review of the whole subject, and also showed how the resources of nature had been made use of by improving the arts. The discussion was carried on by the Chairman, Captain F. Petrie, Mr. V. Newton, the Rev. Mr. Heard, Mr. Shiffard, and Mr. Row.

EDINBURGH

Botanical Society, November 10, 1870.—Sir Walter Elliot, president, in the chair. The president delivered an opening address, in which he congratulated the Society on its continued prosperity, having now entered on its thirty-fifth year of existence. He reviewed the rise and progress of naturalists' field clubs in Britain, and concluded by giving short biographical notices of the members whom the Society had lost by death during the past year.—"Experiments on the Transpiration of Leaves," by Dr. W. R. M'Nab.—"On the Laws of Growth in Plants," by Col. T. B. Collinson.

December 8.—Mr. Alexander Buchan in the chair. The following communications were read:—"Botanical Excursions in July and August 1870, with pupils," by Prof. Balfour.—"Notice of some new and rare mosses collected on Ben Lawers," by Dr. Stirton. This was a continuation of a paper read by Dr. Stirton last session, recording the recent discovery of several new species of mosses on Ben Lawers, with notes as to place of growth, &c., of the rarer species found on that mountain.—"On the varieties of *Hieracium stoloniflorum* of Waldst. and Kit. at different seasons," by Prof. Balfour.—Prof. Dickson exhibited a plant of the Chinese primrose, having stamens and style of the same length (short), although in this species, as in the other dimorphic primroses, they are usually of different lengths. This form is interesting, inasmuch as in an abnormal cowslip, described some years ago by Mr. John Scott, the stamens and style, although of the same length, were both long.

BOOKS RECEIVED

ENGLISH.—The Text-book of Science; Algebra, and Trigonometry: W. N. Griffin (Longmans and Co.)—Strange Dwellings: Rev. J. G. Wood (Longmans and Co).—The Sun: Ruler Fire, Light, and Life of the Planetary System: R. A. Proctor (Longmans and Co.)—The Schools for the People: G. Bartley (Bell and Daldy).

DIARY

THURSDAY, FEBRUARY 9

ROYAL SOCIETY, at 8.30.—The Effect of Exercise on the Bodily Temperature: Dr. Allbutt.—Observations of the Eclipse at Oxford, Dec. 22, 1870: Prof. J. Phillips, F.R.S.—On the Problem of the In- and Circum-scribed Triangle: Prof. Cayley, F.R.S.—On the Unequal Distribution of Weight and Support in Ships, and its Effects in Still Water, in Waves, and in exceptional Positions on Shore: E. J. Reed, C.B.

SOCIETY OF ANTIQUARIES, at 8.30.—On Documents illustrating the Position of the Prior and Convent of Canterbury *sede vacante*: J. B. Sheppard.—On the hitherto undescribed Expedition of the Emperor Augustus into Britain: W. H. Black, F.S.A.

LONDON MATHEMATICAL SOCIETY, at 8.—On a Problem in the Calculus of Variations: Prof. Cayley, V.P.—On Surfaces of Negative Deficiency: Prof. Cayley, V.P.

LONDON INSTITUTION, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff, M.A., F.C.S.

ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

FRIDAY, FEBRUARY 10.

ROYAL ASTRONOMICAL SOCIETY, at 3.—Anniversary Meeting.

ROYAL INSTITUTION, at 9.—On Some Fallacies connected with Ships and Guns: E. J. Reed, C.B.

QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, FEBRUARY 11.

ROYAL INSTITUTION, at 3.—Laws of Life revealed in History: Rev. W. H. Channing.

SUNDAY, FEBRUARY 12.

SUNDAY LECTURE SOCIETY, at 3.30.—The Entozoa of Man and Animals in relation to Public Health and the Sewage Question: Dr. Cobbold, F.R.S.

MONDAY, FEBRUARY 13

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 4.—On the First Principles of Biology: Prof. Huxley. (Educational Course.)

TUESDAY, FEBRUARY 14

ROYAL INSTITUTION, at 3.—Nutrition of Animals: Dr. Foster.

PHOTOGRAPHIC SOCIETY, at 8.—Anniversary Meeting.

WEDNESDAY, FEBRUARY 15.

SOCIETY OF ARTS, at 8.—On the Commerce of India: Dadabhai Navroji.

METEOROLOGICAL SOCIETY, at 7.

ROYAL SOCIETY OF LITERATURE, at 8.30.

ANTHROPOLOGICAL INSTITUTE, at 7.30.—General Meeting.—Ordinary Meeting at 8.

LONDON INSTITUTION, at 7.—On Alizarine and other Colouring Matters: W. H. Perkin, F.R.S. (Conversazione.)

THURSDAY, FEBRUARY 16

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

LINNEAN SOCIETY, at 8.—On Tremellineous Fungi and their Analogues: L. R. and C. Tulasne.—Bryological Remarks: S. O. Lindberg, M.D.

CHEMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 9.—On the Wolf-Rock Lighthouse: James N. Douglass.

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ERRATUM.—Page 275, second column, line 22 from bottom, for "ardent" read "ordeal."

THURSDAY, FEBRUARY 16, 1871

THE EDUCATION OF CIVIL ENGINEERS

ATTENTION has been called to this important subject by a pamphlet recently issued by the Institution of Civil Engineers, containing a clear and well-digested account of the education and status of engineers at home and abroad. The pamphlet, however, is more remarkable for its omissions than for its contents, among which we find premisses warranting a conclusion or many conclusions concerning the education and position of the engineer in different countries. The documents which have been employed in its compilation have been collected, arranged, and issued, under the supervision of the Council of the Institution of Civil Engineers, a body most able to draw conclusions, and to give practical effect to any resolution they may adopt, and yet no conclusion whatever is drawn, and no resolution whatever is adopted. Perhaps, indeed, the Council consider that the education of engineers in England cannot be improved; this interpretation may easily be given to the short summary given of the English system, contained in the following passages:—

> There is, further, in England no public provision for engineering education. Every candidate for the profession must get his technical, like his general education, as best he can; and this necessity has led to conditions of education peculiarly and essentially practical, such being the most direct and expeditious mode of getting into the way of practical employment.
>
> The education of an engineer is, in fact, effected by a process analogous to that followed generally in trades, namely, by a simple course of apprenticeship, usually with a premium, to a practising engineer; during which the pupil is supposed, by taking part in the ordinary business routine, to become gradually familiar with the practical duties of the profession, so as at least to acquire competency to perform them alone, or, at least, after some further practical experience, in a subordinate capacity.
>
> It is not the custom in England to consider *theoretical* knowledge as absolutely essential. It is true that most considerate masters recommend that such knowledge should be acquired, and prefer such pupils as have in some degree attained it, and it is also true that intelligent and earnest-minded pupils often spontaneously devote themselves, both before and during their pupilage, to theoretical studies; but these cases, though happily much more frequent now than formerly, really amount only to voluntary departures from the general rule.
>
> This thorough proficiency in practical matters tends largely to compensate for—in many cases to outweigh—the deficiency in theoretical attainments; and it is undoubtedly this, influenced in some degree by the natural self-reliance and practical common sense inherent in the English character, which has given such a high standing to the profession in this country. The *practical* education in England is perhaps the most perfect possible, if the opportunities obtained during the pupilage are ample, and the pupil properly avails himself of them.

In marked contrast with this language comes the summary of the description of foreign engineering education.

> The education of foreign engineers is strongly contrasted with that in England in every particular. Practical training by apprenticeship is unknown; the education begins at the other end, namely, by the compulsory acquirement of a high degree of theoretical knowledge, under the direction, and generally at the expense, of the Government of the country. Partly with this, and partly afterwards, there is communicated a certain amount of information on practical matters; but this is imparted in a way differing much from the English plan, and probably with less efficient results.
>
> Thus, while the English engineer is launched in his profession with the qualification of a considerable practical experience, but with perhaps little or no theoretical knowledge, the foreign one begins with a thorough foundation of principles, but with a limited course of practice; a deficiency, however, which tends to correct itself with time.

After a few paragraphs showing how these principles are worked out in various countries, we have a summary of suggestions made by engineers and others, without approval or condemnation. The body of the pamphlet consists of dry statistics, which would have been of greater value had not much of the matter been already published in various blue books; and at the end we find suggestions by individuals and extracts from published documents quoted, without any partiality for one rather than another.

It is very difficult to understand why the Council of the Institution has issued a document of this kind. Parents and guardians may find the list of schools and classes valuable, but neither the general public nor the engineers required information of the kind here given. Since the Paris Exhibition, we have been deluged with letters, pamphlets, and evidence as to foreign Polytechnic Schools. What we do require is some authoritative recommendation of one scheme rather than another for raising the standard of engineering education in England. The Council may plead in extenuation of this grievous omission that they are not a legislative body; that their decisions would bind no one, and that they have always disclaimed all responsibility for the opinions expressed in the engineering papers which they publish. If they take no higher view of their functions than this, they might well have abstained from publishing this dry collection of statistics. If, on the other hand, they really mean that the education of young engineers in England needs no improvement, it is a pity that the opinion is not stated in so many words.

The language used conveys a mild expression of paternal approval of the good boys who "spontaneously elevate themselves to theoretical studies," but we almost see the bland smile of compassion with which the successful Nestor regards the proud enthusiasts.

We have the list of colleges where these good boys may be (spontaneously) diligent, but not a word indicating that the number of institutions is insufficient; that the number of classes in the institutions is in defect or excess of the requirements; far less any recommendation that engineers should make attendance on science classes compulsory instead of voluntary. We have an excellent short account of the Whitworth Scholarships, but no word of approval, no hope expressed that the example set may be followed. There is no suggestion that any new technical chairs are required, such as those lately founded in Manchester and Edinburgh, and we can well imagine that when other colleges or patrons approach Government asking for assistance to supplement local efforts, the Treasury may point to this pamphlet and say, Surely if the leading engineers in the kingdom are satisfied, we cannot be justified in giving you the assistance you ask.

No preference is indicated for Mr. Scott Russell's tremendous scheme, of Great Eastern proportions, no condemnation is hinted of collegiate education for our Indian engineers. Engineering degrees meet neither favour nor contempt; the gods of the profession seem to live far removed from all this turmoil, and do not deign even to nod approval.

Seriously, it seems impossible that the Council of the Institution can rest satisfied with such a contribution to the cause of education as this barren pamphlet. It is their duty to take action; their recommendations would have no legal force, but great moral weight. Let them say whether they desire great Polytechnic Schools on the continental model. Not improbably public money will ere long be granted for some such. Let them approve or condemn the Indian College. Let them recommend engineers to compel the attendance of their pupils at suitable classes, and to refuse all students as apprentices who cannot show that they have received proper preliminary training. If proper classes do not now exist for the students, let them tell us where they are wanted and what they ought to be. Let them declare what the preliminary training of a pupil must be. Let them fix a practical value on the engineering degrees of those colleges which deserve such encouragement; or finally, if they will do none of these things, let them say, if they dare, that they are perfectly well satisfied with things as they are.

RECENT PETROGRAPHICAL LITERATURE

II.

Untersuchungen über die Mikroskopische Zusammensetzung und Structur der Basalt-Gesteine. Von Dr. F. Zirkel. (Bonn, 1870.)

Mikromineralogische Mittheilungen. Von Dr. F. Zirkel. Pp. 801. (Neues Jahrbuch für Mineralogie, 1870.)

Beiträge zur Petrographie der plutonischen Gesteine. Von Justus Roth. (Reprinted from the Transactions of the Royal Academy of Sciences of Berlin.)

Sur les Crystallites, études crystallogeniques. Par H. Vogelsang. (Archives Néerlandaises, 1870.)

Kritische Mikroskopisch-mineralogische Studien. Von H. Fischer. (Freiburg.)

Mikroskopische Unterscheidung der Mineralien aus der Augit, Amphibole und Biotit-gruppe. Von G. Tschermak. (Proceedings of the Vienna Academy of Sciences, 1869)

AMONG the Continental petrographers who have led the way in the recent reform and extension of this branch of science, none can claim a more prominent place than Dr. Zirkel. Although still a young man, he has held professorships successively at Lemberg and at Kiel, and we rejoice to hear from him that he has been selected to succeed the venerable Dr. Naumann at Leipzig. He is the author of many excellent mineralogical and petrographical papers, and of the best text-book of petrography which has yet been published. Especially has he distinguished himself by the zeal with which he has followed out the ideas first broadly sketched by Mr. Sorby, and has shown how absolutely indispensable is the application of the microscope to the study of the composition and history of rocks. His researches, while extending over the length and breadth of Germany, have not been confined to the Continent, but have been carried with characteristic enthusiasm even as far as the peaks of Arran, and the cliffs and glens of our north-western isles.

A few years ago he resolved to devote himself to a comprehensive study of the rocks to which the general name of basalt has been given. Though abundant chemical analyses had made the ultimate chemical constitution of these rocks well known, the mineralogical composition of them still remained rather vaguely defined. Their compactness and dark opaque hue made it difficult to investigate the separate mineral ingredients of which they consisted, and men were still speculating about the mineralogical nature of that part of basalt which is soluble in acid, when Dr. Zirkel set to work to collect specimens of basalt from every available locality, and to prepare thin transparent sections of them for examination with transmitted light under the microscope. The result of these investigations appears in the little volume now before us, which is appropriately dedicated to Mr. Sorby. In a brief introduction the author recounts the state of the question when he took it up. Having collected and prepared upwards of 300 sections of basalt from the most varied localities, he believes that he has obtained samples of at least the chief types of composition and structure among the basalts, and he now gives us this first instalment of his labour.

The first section of the volume treats of the microscopic structure and peculiarities of the minerals which enter as chief ingredients into the composition of basalt—augite, felspar, nepheline, leucite, olivine, magnetic iron, &c. This is an especially valuable part of the book, seeing that it furnishes materials for speculating both upon the conditions under which basalt was erupted, and on the various metamorphic changes which the rock as a whole and its component minerals in particular, undergo under the influence of percolating water and atmospheric weathering.

The second part deals with the general microscopic structure of basalt-rocks. The common notion regarding that structure has hitherto been that down to its minutest particles basalt is a crystalline rock, that its individual microscopic ingredients mutually impinge on each other, and that the difference between the structure, for example, of granite and basalt consists in little more than in the varying relative size of their component minerals. Prof. Zirkel, however, shows that this notion, which has been founded on mere deduction and not on direct observation, must be changed. He finds that in the majority of the specimens examined by him, there exists between the most minute ingredients a more or less abundant substance, not individualised into crystals, but amorphous, acting like a cement, sometimes glassy in character, sometimes half-glassy, owing to the appearance of hair-like particles, and sometimes completely dentrified so as to present a confused aggregate of darker or lighter minute granules, needles, hair, and crystals. He regards it as hardly possible to doubt that this glassy base in basalt is the residuum of the original *magma* out of which the recognisable minerals in the rock crystallised, and that it furnishes us with a new proof of the igneous origin of basalt.

In the next section the author proceeds to offer a new subdivision, and detailed descriptions of the basalts. He bases his classification upon the mineralogical composition of the rocks, as made known by microscopic analysis; and taking the non-ferruginous, colourless silicate as his guide, finds that the rocks hitherto classed under the general term basalt, group themselves naturally in three divisions: (1) the Felspar-basalts; (2) the Leucite-basalts; and (3) the Nepheline-basalts. All the three groups always contain augite and magnetic or titaniferous iron, and almost always olivine. So far as Dr. Zirkel's researches go, it appears that all our British basalt-rocks belong to the first or felspar group.

In the last few pages of his memoir the author adds some pertinent remarks on the hitherto vaguely defined series of rocks, which, as he remarks, under the various names of greenstone, trap, melaphyre, &c., play among the secondary and palæozoic formations a part like that which is performed by the basalts in the tertiary formations. And here let us remark that in the chronological separation of igneous rocks made use of by our German fellow-workers, there is something eminently unsatisfactory. The term basalt is restricted by them to tertiary and post-tertiary rocks. But by what methods has the age of each rock been determined? No geologist who has ever had any experience in mapping a district of igneous rocks, can fail to realise how exceedingly difficult it sometimes is to decide upon the true age of such rocks. It is of course easy to say that all basalt is of tertiary or post-tertiary date, and, regarding this as an axiom, to look on every mass of basalt as of later origin than the secondary formations. But the axiom seems to us exceedingly doubtful. In Dr. Zirkel's memoir itself, we have basaltic rocks described which are not only certainly not tertiary, but are probably palæozoic. That igneous rocks have varied in the geological past is highly probable, but geologists are hardly yet in a position dogmatically to assert that no basalt was ever erupted before tertiary times. We cordially wish that our excellent friend Dr. Zirkel will take up the so-called melaphyres; and from what we have ourselves seen of the microscopic structure of the British examples, we shall be greatly surprised if he does not find that from these rocks to true basalt there is such an insensible gradation that no sharp line can be drawn between them. In the meanwhile he deserves the thanks and congratulations of all lovers of mineralogical geology for this admirable memoir on the basalts.

That Prof. Zirkel is still busy with his researches, is shown by the paper (second in the list at the head of this article) which appeared in a recent part of the *Neues Jahrbuch*, and in which he investigates the peculiarities in the minute structure of rock-forming minerals, and also of artificially-fused basalt and syenite.

If the limits of this journal and the patience of its readers permitted, a good deal ought to be said about Roth's most laborious work on the Petrography of the Plutonic Rocks. It is based on the analyses published from 1861 to 1868, which, given in full as an appendix, form half of the book. The word plutonic is used by the author in the sense of originating from igneous fusion, and he includes under it, not only igneous rocks commonly so called, but also gneiss, schist, and clay-slate. These, according to his view, are "the first crusts formed by the cooling of the earth's mass, not metamorphic, that is, not altered in various ways by dark, strange processes which appeared but once and never afterwards; although, indeed, these gneisses and schists, like other rocks, and even more than other rocks, by virtue of their antiquity and position, have undergone chemical changes." From this extract one may judge of Herr Roth's geology. He is a chemist rather than a geologist, and has gained deserved distinction for the great labour he has expended in the collection and discussion of analyses. In his present work, read as a memoir before the Berlin Academy of Sciences, he has amassed all the analyses he could find, which have appeared since the publication in 1861 of his *Gesteinsanalysen*, and has prefixed to them a discussion of the chemical composition of the various rock-species. As a work of reference in the chemical part of petrography, the book is of great value. Two important features are the analyses of decomposed rocks, and the account given of weathering.

Herr Voglesang is another ardent student of the microscopic structure of rocks. A few years ago he published a little work containing the most beautiful coloured illustrations of that structure which have yet appeared. In the present paper he describes under the name of *crystallites* the non-crystallised but yet more or less regularly grouped inorganic bodies which are found in crystals and rocks. As the paper, however, is to be followed by others, we reserve our notice of it for the present.

Professor Fischer's little pamphlet is a modest production, but one which could not have been prepared without a great deal of hard work. Finding that minerals, which to all outward appearance are simple and homogeneous, can yet be resolved by microscopic examination into as many as sometimes four distinct minerals, he has analysed by this method some sixty minerals, and publishes his results in the present paper, which should be in the hands of every petrographer.

Professor Tschermak's essay shows how by microscopical examination with polarised light, it is possible to distinguish augite and hornblende, even when minutely diffused through a rock. The paper is too important to be noticed at the end of this article, and we propose to return to it on a future occasion.

ARCH. GEIKIE

GODMAN'S NATURAL HISTORY OF THE AZORES

Natural History of the Azores or Western Islands. By Frederick Ducane Godman, F.L.S., F.Z.S. (Van Voorst, 1870.)

SINCE the time when Mr. Darwin called attention to the peculiarities of the fauna and flora of the Galapagos in his "Journal of Researches," and showed in his "Origin of Species" how important were the lessons to be learnt from oceanic islands in general, the subject has had great attractions for naturalists, and much material has been collected for its elucidation. Mr. Wollaston's bulky volumes on the Coleoptera of Madeira, the Canaries, and the Cape de Verdes, are models of careful research; but Mr. Godman appears to be the first who has, after a personal exploration of one of these oceanic groups, endeavoured to collect all that is known of its natural productions, and published the result in a condensed and convenient form; and for so doing he deserves the thanks of all naturalists.

Oceanic islands, as defined by Mr. Darwin, are those smaller islands or groups which are more than 300 miles from the nearest continent, from which they are separated by deep sea. Their distinguishing character is, that they possess neither terrestrial mammals nor batrachians, and rarely any reptiles, and that a large proportion of their animal and vegetable inhabitants are quite peculiar to them, although allied to those of the nearest continent. Mr. Darwin was the first to maintain that all such islands had derived their organic productions by migrations from the adjacent land, rather than by union with it; and the facts and arguments he has adduced have convinced most naturalists of the justness of this view. It is powerfully supported by the fact that a connection can in almost every case be traced between the adaptability for migration of a group of animals or plants, and the amount of distinctness of the island species of that group from their allies elsewhere. Thus the land-shells of Madeira are the most peculiar of all its productions, the beetles and the plants less so, while the birds (some of which still come over annually from the continent) are almost all of European species.

The Azores, being about a thousand miles from the nearest coast of Europe, and being separated from it by a profoundly deep ocean, are pre-eminently oceanic islands, and an especial interest therefore attaches to their natural history. The facts, however, are different from what would have been expected, since some of the most striking peculiarities of such islands are far less manifested here than in others much nearer to the mainland; yet on that very account they offer a most convincing illustration of the truth of Mr. Darwin's view, since the cause of their deviation can be detected. Although so much farther from the mainland than the Galapagos or the Madeiras, they possess a far smaller proportion of endemic forms, either of animals or plants; about eighty or ninety per cent. being European species, except in the case of the land shells, where only forty per cent. are European. The explanation of this anomaly is to be found in the fact that the islands are situated in an exceptionally stormy region, gales of wind from every point of the compass being very frequent. As a result, strange land birds of many species appear after these storms; and we cannot doubt that winged insects and the seeds of plants also arrive, although these pass unnoticed. Thus, although these islands may have been isolated quite as long as the Madeiras, their productions being continually crossed by fresh arrivals from the Continent, have not been able to become as much modified by local influences. It is a most interesting fact that the Galapagos, whose productions are so highly peculiar, are situated in an exceptionally calm region. No emigrant land birds are known to visit them, and the result is, that the few wanderers who, by some strange accident reached their shores in the distant past (when circumstances may have been more conducive to such emigration), have been isolated ever since, and have thus had time to become modified into very distinct species.

Mr. Godman's volume consists of a brief account of his journey, of carefully compiled lists, with critical remarks on all the chief groups of terrestrial animals and plants, and of a well-written summary of results. He has obtained the assistance of Mr. Crotch for the beetles, of Mr. Tristram for the land shells, of Mr. H. C. Watson, who has given a most elaborate critical review of all that is known of the flowering plants and ferns, and of Mr. W. Mitten for the mosses and hepaticæ. There are two useful maps of the islands, and the relations of the several species to those of the Atlantic Islands, Europe, and America, are fully pointed out. In conclusion we may notice that there is a full index, that the type and arrangement are very clear, and that the book is issued with cut edges; and we may congratulate the author on having given us more useful matter in a small compass and in a convenient form, than is often to be found in works of much higher pretensions and at ten times the cost.

W.

OUR BOOK SHELF

Hardwicke's Science-Gossip. An Illustrated Medium of Interchange and Gossip for Students and Lovers of Nature. Edited by M. C. Cooke, M.A. (London: Hardwicke, 1871.)

THIS is the sixth volume of a magazine which may be said to fill in scientific literature very much the position which *Notes and Queries* takes in the literary world. The two resemble each other, indeed, in many particulars, and in none more than in the very unequal value which attaches to the articles contained in their pages. There can be no doubt that *Science-Gossip* has fulfilled its object in becoming "a medium of interchange and gossip;" the large number of writers who discuss a yet larger number of subjects in its columns give evidence of this.

As an example of the best of the papers in the present volume, we would refer to one entitled "The Towing-net," by Major Holland, which appeared in the September number, which is pleasantly written, well illustrated, and thoroughly correct in its details—although somewhat marred by an unfortunate misletterіng of the engravings. It is evidently written from personal and practical experience, and is just the paper to awaken a taste for marine studies in any one who has time and opportunity to devote to them. Mr. Taylor's geological papers demand a word of praise; and a long account of a "formicary" in the November number will be read with interest. Mr. Harting and Mr. Robert Holland contribute respectively useful ornithological and botanical articles; and microscopy is well represented. In some of the papers, however, "gossip" appears to take precedence of "science," and thus we find a lady correspondent expressing her wonder "whether flowers suffer pain," and writing a paper of nearly two columns on "errors of the press;" which may be amusing, but certainly cannot by any effort of the imagination be called scientific. On the whole, however, the volume is a satisfactory one, the illustrations being especially well executed; and we have no doubt that it exercises a beneficial influence upon amateur naturalists. We should be glad to see the rule which obtains in *Notes and Queries*—that correspondents replying to queries should refer to volume and page where such questions are to be found—enforced by the editor of *Science-Gossip;* the convenience of reference to a correspondence on any particular subject would thus be much augmented.

Etudes faites dans la collection de l'Ecole des Mines sur des Fossiles noveaux ou mal connus. Premier fascicule. Mollusques Tertiaires. Par F. Bayan, &c. 4to., pp. 81, 10 plates. (Paris: F. Savy, 1870. London: Williams and Norgate.)

M. BAYAN, who occupies the office of Engineer of Bridges and Roads in connection with the Ecole des Mines, presents us in the work before us with descriptions of 47 genera and 106 species, illustrated by 139 well-executed figures of Fossil Tertiary Mollusca. The localities represented comprise the *Calcaire Grossier*, &c. (Eocene), of Longpont, St. Parres, Rilly, Roncà, Parnes, Aizy, Grignon, Anvers, Chaumont, and numerous other localities in France, Belgium, Italy, and Algeria. This is the author's second work, but out of the 106 species, upwards of 80 are christened by M. Bayan; which implies that either he has in the course of his researches come upon an unusually large number of new forms, or, not having been able to refer his new examples to the species already described by M. Deshayes and others, on account of their imperfect condition, he has preferred the easier method of giving them new names. Thus we find fifteen new Tertiary species of the genus *Cerithium* added to our already overburdened nomenclature, one half of which, at least, might have been referred to well-known species. This is all the more surprising as M. Deshayes' grand collection is now deposited in the Museum of the Ecole des Mines, besides numerous other well-known typical collections. The book is written and transferred to stone by a new auto-lithographic process, the result of which is that there are forty-seven more or less important errata given, which the reader must correct before he can use the book with safety. We are sorry to be unable to avoid what may appear a harsh criticism of M. Bayan's work, but those only know the labour which such monographs cause who require to use them as works of reference in the scientific determination of fossils. We are arrived at a period in palæozoology when we cannot be satisfied with merely getting a name to a fossil, but *we must have the right name*—that which expresses its identity with, or distinctness from, other nearly-allied forms obtained from other similar or different geological horizons around; so that as our work progresses we find we are helping to reconstruct a history of these old marine faunas, as a whole; not a series of disjointed essays. We want to see the work of Professors Beyrich and von Koenen in Germany, fitted in with that of M. Nyst in Belgium, and these with our countrymen Messrs. S. V. Wood and Frederick E. Edwards, joined to M. Deshayes' grand work on the Paris Basin; these again with Michelotti, Bellardi, and other Italian works, joined to Hörnes's great work on the Vienna Basin. By no other plan can we hope to arrive at a clear notion of the value of terms applied to geological horizons, and the names of the species by which they are characterised.

H. W.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Scientific Instruction in Elementary Schools

WHAT is to be brought under the new Act in our elementary schools? The never-ending permutations and combinations of the three R.'s, attendant on Mr. Lowe's Revised Code of Education, will, doubtless, soon be at an end, or at least limited in number. What improvement will come? What encouragement will Government give to Science teaching? Under the Revised Code it well-nigh disappeared, or, if it lingered on in some few spots, became almost worthless, *per se*, owing to its necessarily disconnected and unsystematic nature. We say it *disappeared*, which implies that it once had a footing; it certainly had, and was to some considerable extent followed out in very many of our elementary schools. The Committee of Council encouraged it, not only by simply recognising it, which they have not done of late years, but by making special reduced rates to assist the teacher in experimental lessons. Ten or twelve years ago educational periodicals teemed with hints on the subject, and specimen lessons were frequently inserted at full length; books for the use of teachers were written by scientific men; the teaching of "common things," though not altogether scientific in its way, yet showed the general opinion of competent persons in the matter. Why has all this been allowed to die out? We know schools at the present time where the apparatus liberally granted by Government, in days long gone by, has been carefully locked up in the cabinet for years, waiting for more enlightened times to return. We do not say that the Committee of Council on Education has positively prohibited all scientific instruction in our elementary schools; of course, they have done no such thing directly, but, indirectly, they have prevented it—(1) by not recognising it as formerly; (2) by discontinuing grants of apparatus; (3) by making the examination in reading, writing, and arithmetic so rigid as virtually to confine the attention of the master to these three subjects. The examination of the boys in these schools has, in fact, been proportionally much more severe than that of candidates for the Civil Service, and at the same time more so than that of the pupil teacher placed over them. Hence the teacher has hardly dared to venture on giving time to other subjects with the value of which he was at the same time well acquainted.

Our middle-class schools, as well as those of a still higher grade, are gradually becoming more and more convinced of the value of a scientific education, and we must not lose sight of it in our elementary schools. It is even more important there than in the others; there the "working man" receives his education, and if our artisans are to become really intelligent men, fitted to compete in their various branches of labour with their fellow workmen on the Continent, we must prepare them for it in early life, and they are to be found as children, not in such places as Eton, Harrow, or Marlborough, but in our National and British schools. Education, we are told, is to be extended and improved; how can it be better improved than by the increase of scientific instruction? Any elementary schoolmaster will tell us that the general education given in our schools now is far below what it was before Mr. Lowe introduced his changes. The three R's are, no doubt, better taught in many instances, but the intellectual powers of the children have not been drawn out and properly cultivated; we question whether in many cases they have not been stunted in their growth. The same three lessons morning and afternoon, day after day, for twelve months, until H. M. I. comes round again—what has it been to both teacher and children, but unwelcome drudgery? We do not think the Educational Department need feel the slightest apprehension that ordinary subjects of instruction would suffer by restoring the former state of affairs (though it is to be hoped they will go further than that); there can be no doubt that an extra hour or two during the week set apart for science lessons would be not only no loss of time but a positive gain in many respects, by enlarging the intellect of the children, and enabling them better both to apprehend and comprehend what is placed before them in other subjects. It is a great relief to teacher and children to turn for even half an hour a week from the usual monotonous course to some subject totally different, both in its nature and in the mode of treatment. We have seen the faces brighten up, and the eyes sparkle again at the preparations made for an experimental lesson in physical science, or for one in natural history.

We hope, then, to see a change for the better in this respect; we trust that the almost forgotten apparatus and the rolled up diagrams will once more see the light of day, and be restored to their legitimate use. As to the plans to be adopted, each teacher is the best judge, perhaps, in his own school; he knows how may hours per week can be spared from other subjects; but two branches of natural science at least should be taken up, one of which should be natural history, as at once the most interesting, the best adapted for cultivating the powers of close observation and discrimination, and as a study which can be most easily followed up by the pupil himself. But every boy in our elementary schools ought to have a fair knowledge of the laws of heat, of sound, of fluids, of health and ventilation, of mechanics; of the outlines of electricity, the chemistry of the commoner elements and their combinations, *e.g.* oxygen, hydrogen, nitrogen, carbonic acid, atmospheric air, coal gas, &c. &c.

The branches to be taught might vary in different localities, according to the nature and productions of the district, and the industrial occupations of the inhabitants, *e.g.*, in mining districts a certain amount of practical geology should be taught, together with a course of lessons on the minerals obtained there; in coal districts, the nature of the coal, the formations in which it is found, the principles of mining, the nature and properties of carbonic acid, light and heavy carburetted hydrogen, coal-gas, the principles of ventilation, especially in reference to mines; also to some extent lessons on flame and heat, the "Davy" lamp, and others used in the mines, &c. In manufacturing districts, similarly, a practical course on mechanics and steam might be followed out, and a certain amount of chemistry with regard to the operations performed such as bleaching. While in agricultural districts much might be done towards making the field labourer a more intelligent being than he is usually supposed to be, by giving lessons in agricultural chemistry, the art of draining, the laws of evaporation and condensation, the value of woodland, the construction of various implements used, &c. But in every locality lessons should be given in physiology, health, and ventilation, while natural history would everywhere form a not less pleasing than instructive alternative subject. The walls of every school, upper, middle, or elementary, should be covered with diagrams, and at the foot of these should be descriptive and explanatory notes.

As regards the teachers themselves, it may be left an open question as to whether they should be required to go through the ordeal of an examination in the subjects they take up, or whether it may be taken for granted that the mental discipline undergone for two years in the training college is sufficient to enable them to perfect themselves for the task. Those among them who have been at work for ten or fifteen years might probably feel indisposed to prepare again for examination; and we are ready to admit that if a man can bring up his pupils to the required standard, he may well be excused from any other test. At any rate it is to be hoped that the subject of scientific instruction in our elementary schools will receive that attention from Government which is due to it, and that having been once again taken up, it will never be allowed to fall into desuetude.

HENRY ULLYETT,
Hon. Sec. Folkestone Nat. Hist. Soc.
National School, Folkestone

The Prevalence of West Winds

IN NATURE of 29th September, 1870, there is an abstract of a paper read at the British Association by Mr. Laughton, maintaining that the preponderance of west over east winds over the entire globe is such as to point to a cosmical force of some kind, moving the atmosphere round the earth from west to east; and concluding "that the motive force for which we are seeking, is really the disturbing force of the attraction of the heavenly bodies."

The course here suggested, if it were operative at all, would act in the opposite direction to that required. The only possible effect of the attraction of the heavenly bodies on the atmosphere is an atmospheric tide, and the motion of a tide is necessarily from east to west, with the apparent diurnal motion of the heavens. But it is only when resisted that a tidal wave can give origin to currents, and it is scarcely probable that any tidal current can be formed in the atmosphere of sufficient force to blow a feather. I maintain the truth of the ordinary theory, that all winds originate in a disturbance of atmospheric equilibrium by unequal solar heating in different parts of the earth, but are changed in direction by the earth's rotation. This theory is quite consistent with the fact on which Mr. Laughton insists, that west winds preponderate over east winds. It is true that the mechanical effect of any wind whatever must be balanced by an equal mechanical effect in the opposite direction. Mr. Laughton appears to see that this would necessarily be the case if the ordinary theory of the winds is true, but he does not see how the compensation is effected. Were the winds all from the west, they would in an infinitesimal degree accelerate the earth's rotation; were they all from the east, they would in an infinitesimal degree retard it: and if either of these effects were proved to take place, the inference would be certain that the winds are set in motion by some other agency than unequal heating in different places; because, in virtue of the universally true principle of the "conservation of rotation," no such motive power could have any effect on the earth's rotation whatever.

The unbalanced effect of any wind on the earth's rotation will be due to the product of three factors, namely, the area covered by the wind, the east or west component of its force, and the radius of the parallel of latitude at the place. Mr. Laughton has probably overlooked the last-mentioned factor. It gives *leverage*. An east wind near the equator has more effect in retarding the rotation of the earth than a west wind of equal extent and force at a higher latitude has in accelerating it, just as a weight at the end of the long arm of a lever outweighs an equal weight at the end of the short arm. It is for this reason that the west winds, which are mostly in the higher latitudes, are of greater force, and probably cover a greater area than the east winds, which, under the name of the trade-winds, predominate near the equator.

JOSEPH JOHN MURPHY
Old Forge, Dunmurry, Co. Antrim, Jan. 25

Can Weather be Influenced by Artificial Means?

SOME remarks on this subject made by the Rev. R. B. Belcher before the Geological Section of the British Association (reported in the *Athenæum* of October 29), reopen a question of popular meteorology, which has not, perhaps, been sufficiently attended to, from an exact and scientific point of view. Such evidence on the subject, as is at present available to the public, is too general to have much claim to correctness of detail on points which require particular and *local* information; and I offer the following abstract of it principally in the hope that it may lead to further inquiry and observation.

The idea that large fires do, in some way, bring on rain, is very old; but it was, I believe, for the first time stated as a fact and explained on scientific grounds by the late Professor Espy. His theory is, that the heating of the air causes a rapidly ascending current, and that the moisture which air near the surface always contains, is thus carried into the upper regions of the atmosphere to be condensed and to fall as rain. In support of this view, he has given several instances in which rain did immediately follow the kindling of a fire, when no clouds had previously been visible, but in a problem of this nature, negative examples have more weight than positive; and it is necessary to admit that, though in some very remarkable instances rain has followed a large fire, in other instances, quite as remarkable, there is no notice of rain. It is, of course, difficult to speak with absolute certainty; it may almost always be said that a few drops have fallen, sufficient to bear out the truth of Espy's theory, but it seems to me that to establish it in its entirety, something more than this is necessary, and that an extraordinary fire ought to produce a very decided shower, if not a heavy downpour. Last summer, in July, a large fire raged for several days on the moorland and through the pine copses a few miles to the east of Wimborne-Minster. The weather was hot and dry. I was myself on a walking tour in that part of the country, and passed close by this fire, near the village of Longham, where it had been burning with great fury for a week. The farmers were all complaining of the long and excessive drought, and I can give my personal testimony to the fact that there was absolutely no rain. Similarly in the very dry summer of '68, the papers were for some weeks filled with accounts of fires raging fiercely in various parts of Wales and Scotland; but there was no word of any break in the drought. It is only by referring to exceptional and recent examples of this kind that we can feel any degree of certainty; wet weather and heavy rain are so common in this country, that under circumstances not very remarkable, they would scarcely be noticed; and these instances I have just mentioned show the necessity for caution before attributing to fire and consequent rain the relationship of cause and effect; and, as bearing directly on Espy's theory, I may add that there are no observations which would show that the rainfall in our large towns is greater than in the adjacent country; yet the thousands of fires in London, for example, must heat the air sufficiently to cause some ascensional tendency.

The evidence regarding the effect of violent explosions is in very much the same state of imperfection. Theoretically, we can admit that a violent shock may throw large masses of the lower air to a considerable height, so as to cause condensation of its vapour; and there are many instances which seem to support the conclusion that it does do so. The clearest on record are perhaps those mentioned by Dr. Darwin, in his Journal of Researches, where he describes how on several occasions a shower of rain fell, in the middle of the *dry* season, in Chili and other parts of South America, after an earthquake. After the earthquake in Peru, in the latter part of the year 1868, I went carefully through the published accounts, but found no mention of any similar showers In our own climate, in which rain is not uncommon at any time of the year, the circumstances require to be examined into more critically, and the evidence afforded by them can never be quite so satisfactory. Whitaker's Almanack for last year, in the list of Remarkable Occurrences, &c, for 1869, mentions several serious explosions of powder mills, collieries, &c., but not one of them is followed by any notice of remarkable weather, storm, or rain; still, as one of these is the conflagration at Bordeaux on September 28th, which, as Mr. Belcher has pointed out, was followed by very heavy gales, both in the Bay of Biscay and in the English Channel, it is likely enough that heavy weather following some of the other accidents may have been omitted, or have been insufficient to call for special mention in such a general list.

The effect of great battles, again, has always been a favourite theme amongst popular meteorologists; but in regard to this also there is the same uncertainty. Several battles and bombardments have been followed by storms; but, on the other hand, several have not. The general opinion amongst naval men is that heavy firing beats down the wind and produces a calm; and it is tolerably certain that this is frequently the case, more especially if the wind is light. Whether it does or not, the sudden irruption of millions of cubic feet of gas into the lowest strata of the atmosphere and within a very limited area, must have a tendency to cause disturbance, a tendency increased by the undulatory movement due to the noise of the guns. The battle of Trafalgar (October 21st) was fought in a very light westerly breeze, which, towards the afternoon, almost entirely died away. During the night the wind gradually freshened, and by noon the next day blew very hard. Lord Collingwood in his despatch, dated the 22nd, says it had "blown a gale of wind ever since the action;" but that this is merely a general way of speaking is seen by a reference to the logs of the various ships of the fleet. (Despatches of Lord Nelson, vol. vii., pp. 159, *et seq.*) A westerly gale, on the coast of Portugal or Spain, is not such an unusual occurrence in the end of October, as to compel us to attribute one to a battle which took place twenty hours before it came on. Again, the battle of St. Vincent, of the Nile, Rodney's action off St. Domingo, and many others, do not seem to have been followed by any interruption to the usual fine weather. Quatre-bras was followed by heavy rain; but the night after Waterloo was fine, and the bright moonlight is specially mentioned as advantageous to the pursuers; and though Mr. Belcher speaks of rain as having set in in the east of France during the present campaign, I have not seen any notice of its following the battles of Gravelotte or Sedan, so as to be clearly referable to them.

What appears to me the only explanation of the apparent contradictions in the evidence is this; that large fires, explosions, battles, and earthquakes, do tend to cause atmospheric disturbance, and especially, to induce a fall of rain; but that for the tendency to produce effect, it is necessary that other conditions should be suitable; that rain does not follow, unless the lower air contain a great deal of moisture; and that therefore the ascensional movement does not reach to a height such as we might at first be led to conceive; that, in fact, the height is for the most part very trifling. With regard to storms said to have been caused by some of these agencies, the evidence is still more unsatisfactory; and, in our present ignorance of the cause of storms generally, is quite insufficient to compel us to attribute any one particular gale, extending probably over a wide area, to some very limited and comparatively insignificant disturbance.

J. K. LAUGHTON

Natural Science at Cambridge

THE rejoinder in your last number (p. 287) to my inquiries is rather ingenious than ingenuous. "The Writer of the Article in Question" vouchsafes no answer to my very plain questions (p. 264), but is kind enough, instead of justifying his own statement, to propound sundry suggestions, surmises, and what not. I simply asked for confirmation of a certain assertion. This he avoids giving, and, though I regret the cause, I do not wonder at it. Some persons may think it would have been better taste to have acknowledged that the assertion was unfounded. Perhaps your correspondent is of another opinion; if so, I do not wish to interfere with him. I have only to remark that substantially he admits the force of the doubt I expressed by now modifying his previous statement as to "most of the colleges" into "some colleges"—perhaps "one college" would have been nearer the mark, but I will let that pass, hoping that when he again writes on the subject, events may have proved him to be a true prophet.

Cambridge, Feb. 10 M. A.

Glass Globes

IN consequence of the letter of Col. Greenwood, which appeared in NATURE of the 1st December last (No. 57, p. 87), I wrote to Dr. G. O. Sars, of Christiania, who is the Inspector of Sea-fisheries in Norway, for information on the subject. In his reply, just received, he says he was not aware that such globes were used as net floats in any other country than Norway, and that until quite recently they had been used there in the great cod fisheries at Lofoten only. At Sondmor, in Christiansand, they had begun to come into use in 1869, but by no means generally. He has no doubt that the floats washed ashore [illegible] the Hebrides came from Lofoten; but he remar[illegible] course in a south-westerly direction is quite contrary [illegible] might be expected from the action of the equatorial current [illegible] he suggests that before reaching our northern coasts the flo[illegible] made a very long course, having been first carried northwards until they got within the range of the Polar stream, by which they may have been carried southwards along the coast of Greenland, and so at last coming within the influence of the Gulf-stream, they were carried eastward across the Atlantic, and thence drifted by south-westerly winds to their destination. He adds that the fact of their having been found on the *west* coast of the Isle of Lewis seems to confirm this hypothesis.

Feb. 13 J. GWYN JEFFREYS

P.S. on another subject. The correspondents in recent numbers of NATURE do not appear to have been aware of Dr. Carpenter's and my remarks in the *Athenæum* of the 22nd of October last on the hypothesis of the Cretaceous formation being continued or not continued up the present time.

The Primary Colours

HERE is another proof that violet is the third primary. Prof. Tyndall, with that sagacity which results from the right use of his imagination, has given us a reason for the blue colour of the sky. It is probably the true reason, and it proves that blue is not a primary colour. The smallest light-waves are those of the extreme violet. These, therefore, are the waves which will be reflected in greatest proportion by the minute particles of foreign matter in the air. Why is not the sky violet then, instead of blue? Plainly, because although violet predominates in the reflected light, there is also a small proportion of green and a still smaller proportion of red. Suppose the ratios to be 1 red, 3 green, 6 violet, and suppose the ratios for white light to be 1 red, 2 green, 4 violet. Then, the light of the sky will be white *plus* 1 green, 2 violet, which is blue. In Professor Tyndall's experiment on the decomposition of sulphurous acid gas by a beam from the electric lamp, he tells us that, as the particles of the sulphur cloud grow larger, the colour changes from pale blue to deeper blue, and then to whitish blue and white.

No colour but blue makes its appearance, because there is always some green and red, as well as violet; and always a surplus of green *over* red, more than enough for white light.

Leicester, Jan. 28 FREDERICK T. MOTT

Yellow

IT would seem to me that the great difficulty of conceiving yellow as a compound colour is the brightness or lightness of yellow, as compared with its components. In the spectrum, we have the maximum of light in the yellow, and it is against our experience to put two dark colours together and form one light one, as, for example, to put the red and green together and form yellow. But there is just the same difficulty in the production of white light from all the colours of the spectrum, for we regard white as lighter than any of the colours, even than yellow. This, however, is a mental fallacy, and if once exposed, seems to me to do away with the difficulty of conceiving white as made up of a series of colours. Our ideas of colours and tints are derived from our own experience, and we produce tints by precisely the opposite method to that of nature. Nature's method of painting a blade of grass is, to throw on it all the colours of the spectrum, and afterwards pick out those which shall have a *residue* of green. The artist's method is to see what colours will produce green and then lay those on. I do not think it too much to say that all colours are merely tints of white—that, for example, yellow is really yellowish white, green, greenish white, and so on. If we take two cards, one green, the other red, and hold them so that the light is properly reflected from the one on to the other, the cards do not appear black but white. If the red and green were pure colours, the cards should of course appear black, for the light from the green card would be totally absorbed by the red card, and similarly with the red card. The white cannot be produced by a combination of the red and green, but by the extinction on one card of the green that gives the greenish white, and on the other of the red which gives the reddish white. That yellow is but little different from white is well illustrated in the beautiful experiment of Newton's of syn-

[illegible] ours of the spectrum by reflection from seven [illegible] rors. With the mirrors placed at equal distances [illegible] other, the spot of compound light is not white but [illegible], that is to say, it is yellowish white, the colours lost [illegible] ween the mirrors being just those necessary to bring out the full white.

C. J. WOODWARD

Meteor

A FINE meteor was seen here to-night at about 9 10 P.M.; it was described to me as starting from near θ Orionis, and proceeding towards a point a little north of γ Eridani, when it was lost behind a belt of cloud. J. M. WILSON

Rugby, Feb. 13

Snake Bites

IN NATURE of Dec. 22 I notice a note extracted from the *Pall Mall Gazette*, giving a return of the excessive number of deaths which take place annually in the Bengal Presidency, from the effects of snake-bite. That 11,416 persons die from this cause alone, "and that no efficacious means are adopted to check its ravages," are very startling announcements, and strike me as being well worth the attention of the readers of NATURE.

Upwards of a year ago Dr. Fayrer recorded an elaborate series of experiments on snake poison, in the *Indian Medical Gazette*, from which he concludes "that if an animal, and probably a man, be fairly bitten by a fresh and vigorous cobra or daboia, it or he will inevitably succumb unless some immediate or direct method of arresting the entering of the poison into the circulation be practised." This direct method is to apply ligatures and cauterisation; for, says the same authority, "to conceive of an antidote, in the true sense of the term, to snake poison, one must imagine a substance so subtle as to follow, overtake, and neutralise the venom in the blood, or that shall have the power of counteracting and neutralising the deadly influence it has exerted on the vital forces." I remember reading some time ago of another doctor in India or Australia, who had tried ammonia as an antidote, but I cannot recollect with what result. It seems to me, however, that this real antidote has still to be found: and cauterisation, to prove effectual, must follow the course of the poison, which it cannot do; nor, indeed, is it possible for it to do much more than burn the walls of the wound, so it is not to be wondered at if some of those subjected to this powerful treatment do not recover.

I have long thought that the best cure for snake-bite would be powerful suction, applied to the wound by means of an instrument made for the purpose, and similar in principle to a boy's sucker. This would draw off a considerable quantity of the blood in the neighbourhood of the wound, and by so doing wash the poison out before it. Above the wound there might be a ligature applied, but sufficiently distant from it to ensure that the blood in the small vessels between the wound and ligature be competent to wash out the poison.

Where such an instrument is not at hand, and the assistance of a second party can be obtained, he might tie a ligature above the wound, and suck the latter with his mouth for a considerable time, spitting out all the saliva and blood which accumulates. It would be advisable, too, to make the wound a little larger before commencing to suck, with a sharp knife or otherwise, in order that the greater flow of blood may the better discharge the poison. The operation of sucking the poison into the mouth need not be feared, for even although a small portion of it were swallowed, it could do no harm. I believe I am correct in stating that a quantity of poison which will prove fatal on entering the circulation, would have no injurious effect when taken into the stomach. Where the bite is in such a part of the body that the party bitten can easily suck it himself, then he ought to do so: but unfortunately this is seldom the case, it usually being the lower extremities which are attacked.

In support of this method, I may say that I read, two or three years ago, an account of how the bites of snakes were counteracted in a woody portion of South America. The writer said when any one was bitten—and there were one or two almost daily—he was sure to die in thirty minutes to one hour afterwards if his wound was not immediately sucked. There, however, in order to make quite certain that the poison, when sucked out of the wound, would have no injurious effect on the sucker, the latter filled his mouth with olive oil before applying it to the wound: and I imagine this would be a sure precaution, for it provides a plentiful supply of matter for the poison to diffuse in, without interfering much with the absorbents of the mouth. The result of this writer's experience was that the bitten person seldom, if ever, died; and he who sucked the wound never felt an injurious effect.

This subject seems so important that I have ventured to address you at this length upon it, in the hope of drawing from some of your correspondents further details concerning antidotes and methods of curing snake-bite. T. L. PATTERSON

The Cretaceous Period

IN NATURE of Jan. 19, a letter appeared from Prof. Wyville Thomson defending the expressions, "we are still living in *the* Cretaceous epoch," "*the* chalk is being formed at present in the bed of the Atlantic." When first this announcement was made, it was followed up by various strong comments implying that the similarity of the Atlantic mud to the chalk in lithological character, and in many of the imbedded organisms* "would seem to unsettle much that has generally been accredited to geological science," would, in fact, revolutionise geological classification.

As these unfortunate expressions are again put forward, notwithstanding the protest of our most distinguished geologists,† Sir Roderick Murchison and Sir Charles Lyell,‡ it may be useful to consider what the question at issue really is. Simply stated, it is this: Have we sufficient evidence for drawing one of our strongest lines at the base of the tertiary deposits, and saying that it marks the commencement of a new epoch or period?§

In grouping the rocks, we have been obliged frequently to adopt an arbitrary classification. The thickness will not furnish the necessary tests, as accumulation is more rapid at one time and place than another. Lithological character will not do alone, as a bed often passes both vertically and horizontally into one quite different. The organic remains will not do alone, as the fauna migrated and re-migrated to suitable areas when favourable conditions recurred.

But sometimes we have the commencement of a new period well defined by seeing that the group of deposits which form the record of it rest unconformably on an older formation, which has been in part at least heaved up, denuded, and used to form the new series. At the bottom of this newer series we must infer a considerable break—a portion of unrepresented time.‖ This time may be represented elsewhere, but we have a point in time well marked by the first grain of the new deposit laid upon the older denuded rocks.

So we are quite safe in saying that there had been a considerable lapse of time, and that new conditions prevailed over large areas when the Cambrian was laid unconformably upon the Laurentian, when the Upper Old Red was laid unconformably on the Lower Old Red and Silurian, when the Permian was laid unconformably on the Carboniferous and more ancient rocks.

Probably deposition went on longer over one area than another—very likely deposition has never in the earth's true geological history been entirely arrested, so that the connecting deposits between any two formations and intermediate forms of life may possibly still be preserved under the depths of ocean or on the vast still unexplored continents. It would be of course difficult under such circumstances to identify in a series of more or less continuous deposits the base line we have so well marked elsewhere by visible unconformity; and this difficulty occurs in the older rocks, as, for instance, in the case of the base of the Upper Old Red or Carboniferous in South Wales, of the Permian when the Rotheliegende is present, and many others, but this arises from our want of data. We might fairly hope that if we could find the continuous deposits after enormous intervals represented by known unconformities, we should read a wonderful story, and know, for instance, more clearly by what variation of forms and invasion of stronger life from adjoining areas we are left at the end of a long period with an ammonite instead of a goniatite, or a nautilus instead of either.

Now, to return to the particular case under notice. Have we at the bottom of the Tertiary formations evidence of a break so large, of a lapse of unrepresented time so long, of a change in conditions over large areas so great that, we are justified in saying that this is a convenient place to draw one of our strong lines?

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* Carpenter, Lecture Royal Institution, Ap 1869

† Pres. Add. Geog Sect B.it Assoc, Liverpool, 1870.

‡ Students' Elements of Geology, 1871.

§ See also an able article by Mr Green, *Geol. Mag*, Jan 1870.

‖ See Ramsay, Pres. Add. Geol. Soc, 1863-4.

The overlap and irregular occurrence of the Tertiary on various parts [*] of the Cretaceous deposits, the immense banks of flints containing Cretaceous fossils in the Tertiary beds, point to an enormous amount of change and denudation between the consolidation of the Cretaceous and accumulation of the Tertiary deposits. This is accompanied by an almost entire break in the higher forms of life.[†] It is true that the researches of Dr. Carpenter and his colleagues have brought to light many forms which have survived from the Cretaceous to our own time; but these discoveries are only of the same kind as the discovery in recent times of the genus Lingula, or of forms allied to Encrinites. When we trace back to a remote antiquity ferns and other plants not very unlike those of our own day, Crustacea differing but little from our King Crab, Paludinas hardly distinguishable from recent forms—that does not throw doubt upon the useful grouping of the rocks from carboniferous to recent times.

Species are continually being found common to two beds known to be separated by enormous intervals of time. Upon this fact Barrande founded his theory of the Colonies. But the classification into Mesozoic and Tertiary depends upon evidence that cannot be shaken by the discovery of a few more forms common to the two. The wonder always was that the break in life was so complete as it appeared to be at the close of the Cretaceous period, and the deep-sea dredging expeditions confirm what was *a priori* almost a necessary inference, that deep-sea conditions prevailed somewhere during the whole of the period from the Cretaceous age to our own, and that some forms of life have not been destroyed or developed into anything else during that period; but that is a very different thing from saying that there is not sufficient reason for holding that the base of the Tertiaries marks the commencement of a new epoch.

T. M'K. HUGHES

Insulation of St. Michael's Mount, Cornwall

HAVING read Mr. Peacock's letter in your publication of the 2nd inst, I beg, through the same medium, to show that his reasons for supposing "the mount could not have been an island in 1086" are groundless.

He begins by giving the measurement of "Domesday Book," date 1086, of "the Land of St. Michael," and afterwards writes as follows:—

"There is an entire absence in 'Domesday Book' of any mention of island or islands on any of the coasts of Cornwall. . . . In short, the mount could not have been an island in 1086, because it contained at least eight times as much land as it does at present, probably connecting it with the main land, from which it is even now only one-third of a mile distant. . . . The present area of the mount is only thirty acres, so that there are 210 acres missing . . . since 1086; and in 1099, thirteen years after, we have a record of a catastrophe which would fully account for the loss"—that being the great irruption of the sea in 1099, as recorded in the Anglo-Saxon Chronicle.

When your correspondent quoted the measurement of "The Land of St. Michael" above referred to, he evidently imagined St. Michael's Mount, with the Church or Monastery on its summit, to have been like a nobleman's seat in the midst of a large park, with the sea at a great distance from the centre—and all this to have been comprehended in "The Land of St. Michael." The fact, however, is that in 1086 the Mount was, as it still is, a rock about five furlongs in circumference at its base, and insulated by every tide, whilst the two parishes on the mainland nearest to it—viz., those of St Hilary and Perran-Uthnoe (which may be identical with "The Land of St. Michael"), were then holden by the Church or Monastery of the Mount."[‡] As the mount, however, is now almost universally allowed to be the *Iktin* of Diodorus Siculus, we may be sure that it was long before the commencement of the Christian era insulated daily as it is at present. I have written very fully on this subject in my work already referred to, published in 1862, and also in a paper printed in the Transactions of the Plymouth Institution for 1867-68 (pp. 17-37), in both of which I have exposed the error of all the translators of Diodorus in calling the mount *Iktis* instead of *Iktin*, and have also shown that the Mount, which was called in the Cornish language *Bre*-tin ("Tin-*Mount*") as well as *Ik*-tin ("Tin-*Port*"), has given its name, not only to *Mount's Bay*, but probably also to the whole of *Britain*.

R. EDMONDS

Plymouth, February

P.S.—I had written the above before I saw Mr. H. Michell Whitley's letter in your last number, which states that instead of "*Keiwal holds the Church of St. Michael,*" as Mr. Peacock has translated the passage in Domesday Book (p. 2), it should have been "*The Church of St. Michael holds Keiwal*" (or Treuthal, as it is also called on p. 11), which is the name of a manor in the parish of St. Hilary. This confirms what I have above written, although I have adopted a different way of disproving Mr. Peacock's theory.

Aurora Borealis

A FINE Aurora was seen last night, or this morning, from 1 to 3 A.M. It first appeared as a *transverse* band from N.E. to S.W., and passed in that course far South of the Zenith, or between Arcturus and Mars. Subsequently it spread laterally and upwards; presently radiated from near the Zenith to all azimuths; and at 2.30 A.M. some of the rays N.E. were strongly pink.

In the spectroscope, the usual green line was gloriously bright. I saw it first, with a hand spectroscope, in the darkened light of the rough glass panels of a stair door. There were also faint lines more refrangible over the regions of E, b, and F. Rather to my astonishment, I was totally unable to see a red line, even when looking at rays abundantly pink to the naked eye. This was a disappointment, to say the least of it, because I had prepared, and had in the lower part of the field of view, red chemical lines to compare with anything red that should appear in the Aurora; and I had seen the red line perfectly well in the fine auroras of last autumn, but then I had no such checks on its place.

However, my spectroscope is still a very rough, home-made affair; and I am living in hopes of something better when Government supplies this Observatory at last with its long-desired, long-delayed equatorial.

Royal Observatory, Edinburgh, Feb. 13

C. P. S.

THE THEORY OF GLACIAL MOTION[*]

MR. CROLL'S papers on Ocean Currents are a powerful application of the modern theory of heat and force, to show the fallacy of Captain Maury's explanation of the causes of oceanic circulation. They also discuss other matters of great interest, but as the concluding part is not yet published, we shall say no more about them at present, but that they well deserve careful study.

The other paper is a criticism of the Rev. Canon Moseley's supposed proof that glaciers do not descend by the force of gravity, and of the arguments of Messrs. Ball and Matthews on the other side. It will be remembered, that Canon Moseley determined by experiment the "shearing" force of ice, that is, the force required to fracture it by parallel pressure. A plug of ice of known cross-section is fitted into a hole through two smooth boards, and the force required to break the ice by sliding the boards over each other is the "shearing" force. Increasing this in proportion to the dimensions of a glacier, or of any large portion of one, it was calculated that the force required to cause the different parts of a glacier to slide over each other (as they must do in descending a valley of constantly varying form and size) was at least thirty times greater than the force of gravity on a slope such as glaciers easily descend. Canon Moseley came to the conclusion that expansion and contraction of the ice by heat and cold was the moving power; and the fact that the glaciers move slower by night than by day, and in winter than in summer, was supposed to prove conclusively that heat is the cause of motion.

Mr. Croll believes that Canon Moseley has demonstrated that gravity alone does not cause glaciers to descend, but he completely demolishes the theory of contraction and expansion. He admits that heat aids the motion, but maintains that it does so by acting on the molecules of the ice, which it loosens momentarily from their mutual cohesion, and allows to be re-arranged under the influence of gravity. Heat, he says, is the *condition*, gravity the *cause* of the motion which takes place, molecule by molecule rather than in masses. It seems very doubtful, however, if this theory is more tenable than the one it is intended to supersede. If heat entering the glacier loosens the molecules in its passage and enables them to move insensibly into new positions, it is difficult to understand what causes the numerous longitudinal and transverse fissures of a glacier, the production of which is often attended by loud reports, and which indicate movements of masses, not of molecules. And how could molecular motion lead to that heavy grinding of the ice over its bed, which scores and wears down the hardest rocks, and whitens great rivers with the finely triturated mud?

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[*] See Lyell, Student's Elements of Geol., pp. 258, 261, where attention is called to higher cretaceous beds than those on which the Tertiaries rest in England.

[†] Lyell, Op. cit p 256.

[‡] See my "Land's End District—its Antiquities and Natural History," p. 166.

[*] "On Ocean Currents." By James Croll, of the Geological Survey of Scotland (3 parts). "On the Cause of the Motion of Glaciers." By the same author (Extracted from the *Philosophical Magazine* of 1870.)

None of the opponents of Canon Moseley have noticed what seems to the present writer to be a radical fallacy in his argument about "shearing force." He assumes that, whatever the bulk or weight of the glacier, or of any portion of it to which the formula of the shearing force may be applied, the whole mass shears at once by the action of gravity on the same mass, and does not recognise the possibility of one portion of a glacier acting by its weight to shear another and much smaller portion. But this must inevitably occur; for, owing to the excessive irregularity of the bed in which every glacier moves, the mass must be every where in varying states of tension and compression, and must contain at each instant certain lines and planes of least resistance, the extent of which lines and surfaces may be very small compared with the dimensions of the glacier itself. At any moment, therefore, the whole descending weight of a portion of the glacier containing perhaps thousands of cubic yards of ice, may act so as to cause the shearing of a few superficial feet where the tension is greatest. This being effected, a partial equilibrium is produced there; but the points or surfaces of greatest tension are shifted, and another small shear or fracture occurs; and by this process and the continued regelation of fractured surfaces brought into contact, it may easily be seen that the glacier as a whole would be gradually moulded to its bed, which it would descend as surely as if it were a viscous mass. Another source of motion not taken into account either by Canon Moseley or Mr. Croll is the irregular melting away of the under surface of the glacier by terrestrial heat, which would often form unsupported hollows till a fracture occurred, and every such fracture must result in a downward motion of a portion of the glacier. The observed difference of the rate of motion between winter and summer, day and night, is more probably due to the different quantities of water which descend the crevasses into the bed of the glacier at those periods, than to any direct action of the heat. It is well known that in the higher portions of a glacier the supply of water from melting snow diminishes during the night, as it does in a still greater degree during the winter; and the large quantity of water that flows beneath every glacier in the summer must greatly assist its motion, both by melting away its lower surface, and by, to some extent, buoying it up.

Mr. Matthews's important experiment of the bar of ice which gradually curved by its own weight, should be tried again in an atmosphere kept at the freezing point. This would settle the question whether heat is an essential condition for the curvature or motion of ice by gravitation; but so far as the facts lead us at present, the arguments of Canon Moseley and Mr. Croll by no means *prove* that glaciers do not descend by the force of gravity alone.

ALFRED R. WALLACE

[The publication of this article has been delayed. It was in our hands before the appearance of Mr. Ball's paper in the *Philosophical Magazine* for February, where a view almost identical with Mr. Wallace's is ably advocated.—ED.]

AN ACCOUNT OF THE ECLIPSE AS SEEN FROM VILLASMUNDA BY AN UNSCIENTIFIC OBSERVER

THOSE set in authority over the branch of the Eclipse Expedition stationed at Agosta having decided against depending only upon observations to be made from the Observatory there, deputed Mr. Ranyard to proceed to another point upon the line of totality, and selected me as his coadjutor. Accordingly we set off, accompanied by Jarvis and Burgoyne, two of Colonel Porter's Sappers, at half-past nine in the morning of the eventful day; and, after driving some eight miles inland, we attained about eleven o'clock a point which appeared to my companion to present advantages for our object. Leaving the road, we went into the middle of a field of springing oats, on the highest point of a rocky ridge at an elevation of 600ft. above sea level, and of 520ft. above the *glacis* of Fort Agosta, where were posted the rest of our friends. The spot which Mr. Ranyard selected as the most suitable lay about a hundred yards from a roadside farmhouse, called Casa Vecchia, upon the property of that friend of Science, the Marchese di Sangiuliano, and about two miles distant from the village of Villasmunda. A keen wind was blowing with considerable violence from the north-west, and the situation we had chosen being exposed to its full fury, we at first felt very uneasy with regard to our probable success, for we feared every moment that the telescope would be overturned and injured. A happy thought, however, soon extricated us from our dilemma. Causing our luckless coachman (who wept true Sicilian tears over the imaginary danger to his springless vehicle) to drive it, in the cause of Science, over the rock-sprinkled field, we utilised our carriage as a temporary shelter for the precious instrument, and were ready some time before

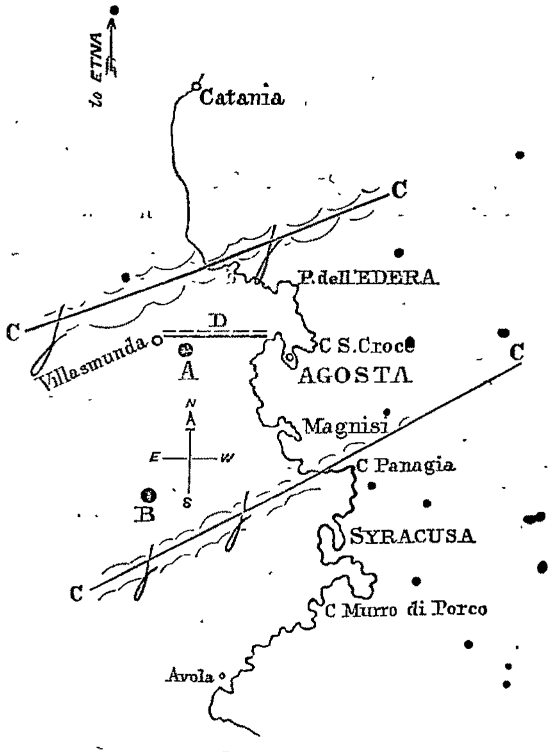

FIG. 1.
A our position, B the sun; CC, CC the lines of cloud, D the road to Agosta.

the first contact took place. During the time occupied in perfecting the necessary preliminaries, I noted the position and the structure of the cloud-banks which were instilling into our minds feelings of the keenest anxiety. We were standing in the centre of what I may describe as a comparatively cloudless longitudinal "slit" in the sky, which was otherwise completely covered; so that, while over our heads the sun was shining brightly, its refulgence obscured only occasionally by light, fleecy, flying clouds; to our front and rear were lying parallel lines of heavily-banked "cumuli-strata" running from south-west outh-east. Perhaps the accompanying rough diagram (Fig. 1) may serve to illustrate their position in relation to our own.

I also set down the following readings of the barometer and thermometer (wind N.N.W.):—

	At first contact.			Five minutes before totality.	At 2.20 P M
Barometer	28·65	28 79	28·88	28·80	28·75

At first contact.

Thermometer in shade . . . . 56°
„ in sun exposed to wind . . . . . . 54°
„ in sun . . . . 58°

Immediately after totality.

Thermometer in sun exposed to wind . . . . . . 55°

Two minutes before the commencement of totality, the clouds behind us, and those in front of us, were black and threateningly lowering, as if a thunderstorm were imminent. Etna, which lay well in our view to the N.N.W., was clothed to its very base with a shroud of the most sombre clouds, while as the seconds flew by the temperature fell sensibly lower and lower.

To the S, and S.W. also, the sky was filled by a strange, steamy, V-shaped (the point of the V being near the earth), filmy mist, through which the sun showed such a feeble and uncertain light, as to give me cause for fear lest our observations should be brought to an abrupt and resultless termination by the total disappearance of the sun behind this curious veil. *Immediately* before totality commenced, a dark vaporous shadow glided very swiftly up over the heavens from the westward, or a little south of west, and, as it came on towards us, seemed to swallow up the earth, leaving it dark in its rear, until at the moment of totality it reached the sun. As it drew near him, a herd of oxen feeding behind us, with one or two exceptions, lay down. With the beginning of totality the air was colder than ever, and for about one minute's space, not more, there fell a small thin rain, which I fancy must have been the result of the condensation of the steamy mist which I have a little while ago described. Of totality itself, as a spectacle, I am almost afraid to speak. To endeavour to describe the inconceivable grandeur of the sight would be a hopeless task. I can only say that nothing will ever efface it from my memory. But if I cannot hope to give you any idea of the sublimity of the scene, at any rate I will do my best to state simply the appearances which I saw. Round the dark moon gleamed the luminous circle of the corona, shining with about half the radiance of an English sun upon a winter's afternoon; while there streamed forth from it in eight directions as many sets of brilliant rays. These "sets" of rays were composed of four sets in the position of a Greek cross, as in Fig. 2; with a St. Andrew's cross, as in Fig. 3, placed upon it, forming something similar to Fig. 4, Fig. 3 extending only about half as far as Fig. 2, which reached as far outwards as the apparent diameter of the sun.

I described the phenomenon to Mr. Ranyard as having struck me by its resemblance to the "glory" round the heads of statues of saints in Roman Catholic shrines.

Jarvis and Burgoyne also made sketches of the "rays" separately, and without consultation with me or with each other, at my request. The similarity of the three is striking. Jarvis described the rays as resembling "the pipes of an organ."

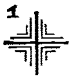

FIG. 2.

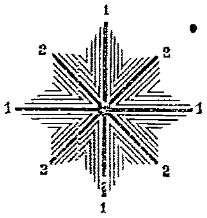

FIG 4.

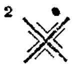

FIG 3.

As the moon passed off from the sun's face, and for some seconds after the contact had wholly ceased, the clouds in the S.E. were suffused with deep red copper colours, which gradually faded away as the sun regained his power. During totality I made the following observations (according to Mr. Ranyard's previous request) with a Nicol's prism with Savart's bands, in the use of which instrument Mr. Ranyard had instructed me, and with which I had constantly practised during our week's preliminary residence in camp at Agosta; viz, I determined the polarisation of the sky at two points: (1) high up on the sky to the S.E. of the sun; (2) under the sun; and at both these points I found the plane of polarisation to be vertical. Totality ceased while I was taking a third observation.

Before concluding these imperfect remarks, which I have only ventured to make public because I thought that they might be of some interest, from the fact of my having had the good fortune to be one of the very few who obtained a perfectly clear and distinct view of the Eclipse during totality, I should like to bear my testimony to the great kindness of Colonel Porter, R.E., in placing all the resources at his command at the service of our party, and to his care of our creature comforts during our ten days' "dwelling in tents" under his charge. It is but just also to mention the zeal and alacrity displayed by his party of Sappers, and their intelligence in mastering the details of duties which were certainly new to them. Nor will it be considered out of place if, in conclusion, I thank the authorities of Agosta, and the Italian astronomers of distinction who did so much, not only by giving us every assistance in their power to render our stay at Agosta so successful, but also by their hospitable attentions to make it as enjoyable as possible. For my own part I can only say that I shall always look back with feelings of pleasure to the Sicilian Eclipse Expedition of 1870.

HENRY SAMUELSON

NOTES

THE Royal Commission on Scientific Instruction and the Advancement of Science is now in full work. This week they meet three times, and last week they met twice.

IT has been announced at the Royal Geographical Society that Livingstone has arrived at Ujiji on his return journey.

THE Hunterian Oration has been delivered this year by Sir W. Fergusson, who *inter alia*, according to the *Times* report, "playfully referred to the suggestion of a distinguished philosopher, whose views appeared in the columns of the *Times* last autumn, that when the microscope did not seem to give satisfaction in minute research, imagination might be substituted; and pointed out that this style of philosophy was not new, for it had been put into Hamlet's mouth by Shakespeare:—

> Imperial Cæsar, dead, and turned to clay,
> Might stop a hole to keep the wind away."

Mr. J. C. Buckmaster has been appointed by Her Majesty's Commissioners to deliver an address on the value of the International Exhibition, with its bearing on industrial instruction, designed particularly for the working classes in all the large towns of the country which express a desire to hear it.

Science has derived many wrinkles from the siege of Paris, and we now learn from the correspondent of the *Daily Telegraph* that all the galleries of the Louvre are filled with sacks of earth to protect the interior from shells; and the damp and comparative warmth of the last few days have provoked active vegetation, so that the bags are covered with grass and weeds; each window is converted into a lively and promising garden. If the arrangement is left undisturbed much longer, we shall have flower beds there. This is certainly a novel kind of window-gardening, which we have no desire to see introduced into this country.

M. Steenaekers is about to issue a report on aeronautical ascents which took place during the siege of Paris. Individual reports are asked from aëronauts, but, unfortunately, very few of them are in a position to give a correct idea of their impressions, being mostly sailors of the Royal Navy.

Amongst the learned men who escaped from Paris in a balloon we must cite M. d'Almeida, author of a treatise on Physics. He escaped on the 17th of December, at one o'clock in the morning, in the "Guttemberg," with four or five other persons. The "Guttemberg" was despatched a few minutes after the "Parmentier," which carried three persons and a bag of letters, and fell within Prussian lines. M. d'Almeida landed at Sanson, eight miles from thence, but he has not been heard of since that time, and it is to be feared that he was taken prisoner with his companions and sent to some German fortress.

The French Government had amalgamated the post-office and telegraphs during the siege of Paris, under the management of M. Steenaekers, for the whole of France except the besieged capital. This reform was inaugurated in imitation of the English system under the pressure of circumstances. But a retrograde step was taken a few days ago, and two different services have been created as formerly. It is to be hoped that so unscientific a measure will soon be cancelled.

French *savants* do not appear to be successful politicians under universal suffrage. Amongst more than 2,000 citizens who offered themselves as candidates to the Parisian electors, we notice only a few members of the French Académie des Sciences. The only names who have come under our notice are M. Sainte-Claire Deville (we do not know if it is Henri or Charles), and M. Nelaton, the physician who cured Garibaldi's wound. M. Berthelot was also a candidate, but he does not belong to the Académie des Sciences. The Académie Française was more successful, as Michelet, Victor Hugo, Henri Martin, Thiers, Jules Favre, and many others, were chosen.

We learn from the *British Medical Journal* that the distinguished ophthalmologist, Dr. Liebreich, has just passed an examination and been admitted a member of the Royal College of Surgeons. It is stated that he will be invited to accept the ophthalmic chair of St. Thomas's Hospital.

The death is announced of Dr. Sheridan Muspratt, F.R.S., the well-known analytical chemist of Liverpool, and author of a Dictionary of Chemistry.

The following is the list of officers and council of the Royal Microscopical Society for the current year:—President: W. K. Parker, F.R.S. Vice-Presidents: Chas. Brooke, F.R.S., J. E. Gray, F.R.S., J. Millar, F. H. Wendham. Treasurer: Richard Mestayer. Secretaries: Henry J. Slack, Jabez Hogg. Council: R. Braithwaite, M.D., John Berney, James Glaisher, F.R.S., W. J. Gray, M.D., Henry Lawson, M.D., Henry Lee, Jas. Murie, M.D., G. W. Royston Pigott, M.D., J. W. Stephenson, Chas. Stewart, Chas. Tyler, T. C. White. Assistant Secretary: Walter W. Reeves.

Mr. A. R. Wallace has printed his Anniversary Address, delivered before the Entomological Society on Jan. 23rd. After referring to the loss Entomology has sustained during the past year by the death of J. T. Lacordaire and A. H. Haliday, and to the publication of Mr. Crotch's papers on the Genera of Coleoptera studied chronologically, McLachlan's Catalogue of British Neuroptera, Dr. A. S. Packard's Guide to the Study of Insects, and some other works of the year, the greater part of the address is occupied by a critique on Mr. Andrew Murray's important paper "On the Geographical Relations of the chief Coleopterous Faunæ." Of Mr. Murray's division of the Coleoptera of the world into three grand stirpes or races—the Indo-African, the Brazilian, and the Microtypal—the first comprehending all the characteristic forms of the eastern tropics, the second all those of tropical America, and the third those of the temperate regions of the whole world, not excluding even America—Mr. Wallace remarks that the two first will probably be generally accepted, while the third group, as of equal value to the others, will be as generally rejected. Mr. Murray's theory, derived from the geographical distribution of the Coleoptera, that the whole of the Atlantic Islands, from the Azores to the Cape de Verdes, and even to St. Helena, are portions of a vast submerged continent connected with Southern Europe, is combated by Mr. Wallace as far as the Madeira group is concerned, while he considers there is more to be said in favour of their connection with one another at some remote period.

The third session of the Ladies' Educational Association of London is considered a successful one. The number of tickets issued has been:—for English Literature, 56; English Language, 40; Psychology and Logic, 46; French Literature, 39; French Language, 20; Experimental Physics, 25; Chemistry, 10; Total, 236, against 292 last year. The falling off is almost entirely in the class of English Literature, in which 104 tickets were issued last year, a decrease of 48 in the present session. The attendance is reported as regular. Lecture associations of a similar kind for ladies have also been established in the northern and eastern districts of the metropolis.

The Annual Report of the Canadian Natural History Society states that the recent dredgings of Mr. Whiteaves have added many facts to our knowledge of the creatures which inhabit Canadian seas. The marine mollusca have been carefully monographed, and instead of 60 or 70 species, we now know of nearly 130, the number having been thus nearly doubled. The careful identification of the inhabitants of the deep sea, in addition to its zoological importance, will do much to illustrate the conditions under which the Canadian post-tertiary deposits have been accumulated.

We have on our table a number of important papers referring to the Colony of Victoria, prepared by Mr. W. H. Archer, Registrar-general to the Colony, and published by authority of the Colonial Government: viz. Patents and Patentees from 1854 to 1866, Abstracts of Specifications of Patents applied for from 1854 to 1866 Ac. to Bu., and Indexes to Patents and Patentees for 1867 and 1868. Also Reports of the Mining Surveyors and Registrars for the quarter ending Sept. 30th, 1870, Report on the present condition of the Geological Survey of the Colony, and Abstracts of English and Colonial Patent Specifications relating to the Preservation of Food, &c., compiled from original documents, by Mr. W. H. Archer. We would call especial attention to the last as a well-arranged epitome of very useful information.

Dr. M. T. Masters and Dr. J. H. Gilbert reprint from the *Proceedings of the Royal Horticultural Society* "Reports of Experiments made in the Gardens of the Society at Chiswick in 1869 on the Influence of various Manures on different Species of

Plants." The experiments afford striking illustrations of the varying powers of accumulation and assimilation possessed by plants belonging to different families, to different genera of the same family, and even to different species of the same genus, under the same external conditions of supply in soil and atmosphere, indicating, as the authors think, not only very varying ranges of root-collection, but also quantitatively varying functional characters, both of the feeders and the elaborating organs. It is expected that results of considerable practical value to agriculturists will be obtained from the second series of experiments which were conducted during 1870. The Royal Horticultural Society appears to be applying its resources to a very useful end in the carrying out of these investigations.

WE have received from Mr. F. Bradley, of Chicago, a pamphlet, entitled "Northern Lights, Shooting Stars, and other Meteoric Phenomena proved to be not of modern origin," in which he explains several well-known Biblical figures by the supposition of an acquaintance on the part of the writers with the phenomena of the Aurora Borealis, and combats the views of a recent writer that the alleged occurrence of the Aurora within the last three centuries for the first time, is a sign of the approaching end of all things.

THE great difficulty which has been experienced by farmers during the present winter in procuring sufficient food for their cattle may have the effect of directing attention to fresh substitutes for the ordinary winter keep. Turnips and Swedes have been a complete failure throughout a large breadth of the country, and the hay crop has been generally so deficient that many agriculturists have been already brought to the verge of ruin. In the Argentine Republic, as is well known, vast herds of cattle are reared upon the natural grasses of the pampas, or upon the lucerne which grows there in great abundance, but in the province of Catamarca both man and beast depend for support mainly upon the leaf and fruit of the algarrobo. It provides their principal stock of food during the winter months, and is said to be exceedingly nutritious. The algarrobo (*Hymenæa Courbaril-Jetaiba*) is indigenous to the country, and its fruit is gathered annually and stored with much care. The long pods are pounded in a wooden mortar, and the residuum is then passed through a sieve, and the meal converted into circular cakes, which, after having been dried in the sun, are fit for use. In this state it is called "patay," and is exported as a bread-stuff into other districts of the Republic, in some of which it forms the exclusive food of the people. Its merits as a means for fattening cattle are also thoroughly appreciated, and it might be a great boon to our farmers, if the Acclimatisation Society would ascertain whether the algarrobo might not be introduced into this country with advantage. The tree grows to a height of forty feet, with wide-spread branches, and a rather slender stem, and flourishes best upon a dry soil.

CONSIDERABLE discussion took place in the *Times* last autumn as to whether acorns were suitable for employment as food for cattle; and the evidence adduced certainly favoured a negative view. Dr. Robert Brown, however, tells us that those produced in California by several species of oak form an important article of food. "The acorns of California are mostly large, and the trees in general produce abundantly, though in some years there is a great scarcity, and much misery ensues among the poorer natives. The acorns are gathered by the squaws, and are preserved in various methods. The most common plan is to build a basket with twigs and rushes in an oak tree and keep the acorns there. The acorns are prepared for eating by grinding them and boiling them with water into a thick paste, or by baking them into bread. The oven is a hole in the ground about eighteen inches cubic. Red-hot stones are placed in the bottom, a little dry sand or loam is placed over them, and next comes a layer of dry leaves. The dough or paste is poured into the hole until it is two or three inches deep; then comes another layer of leaves, more sand, red-hot stones, and finally dirt. At the end of five or six hours the oven has cooled down, and the bread is taken out, an irregular mass, nearly black in colour, not at all agreeable to the eye or to the palate, and mixed with leaves and dirt."

THE Proceedings of the Somersetshire Archæological and Natural History Society for 1868-69, just published, consists almost entirely of papers which would come under the first head. The only ones bearing directly on Natural Science are "On the Geology of the Mendips," by Mr. C. Moore, and "On the Rodentia of the Somerset Caves," by Mr. W. A. Sanford. Is it necessary that Reports of the Proceedings of Local Societies should be published at so high a price as practically to take them out of the reach of the general public?

MR. F. ABBOTT has reprinted a paper read at the meeting of the Royal Society of Tasmania, Oct. 11, 1870, "On the Sun and its Office in the Universe."

WE recently referred to Mr. Townend Glover's paper "On the Food and Habits of Beetles," issued in the Report of the Commissioner of Agriculture for Washington. A work, similar in style, but more comprehensive, has been published by the Board of Agriculture for the State of Missouri, which has secured "an appropriation for this purpose from the Legislature of the State." This "First Annual Report on the noxious, beneficial, and other Insects of the State of Missouri" is furnished by Mr. Charles V. Riley, State Entomologist, and from its plain descriptions, and rough, yet accurate, woodcuts, seems admirably adapted to fulfil the purpose for which it is published—that of giving the farmer a knowledge of his insect friends and foes which he may turn to practical account.

THE *Geraniaceæ*, taken as a whole, are by no means noted for their economic properties. In Tasmania, however, a form of the common *Geranium dissectum* having a thick rootstock is employed by the aborigines, who are in the habit of digging up the large, fleshy roots and roasting them for food. About Launceston it is called "native carrot," and is common throughout the colony. The typical form of *G. dissectum* is generally diffused over the temperate regions of the northern hemisphere in the Old World, where it is annual. In the Eastern United States a biennial or annual form, *G. carolinianum*, takes its place, the typical *G. dissectum* being only known as an introduced weed; although connecting links between the two may readily be detected. West of the Rocky Mountains the stock often appears to be perennial, and it cannot then be distinguished from some of the Australian forms.

WE may call attention to an interesting paper, reported in our present number, by Mr. Cecil Smith, read before the Somersetshire Natural History Society, on the relations of the Great Bustard, which has once more visited England during this severe winter. Mr. Smith removes the bird from the Rasores, among which it was placed by Mr. Yarrell, and restores it to the position assigned it by Cuvier, among the Grallatores, near to the Plovers.

THE question of killing deadly snakes at Government expense in India is again under discussion. The Government is losing its subjects at the rate of above a hundred a day, or 40,000 a year by snake bites, but it fears losing rupees in the crippled state of its treasury. The last enforcement of the law was under Mr. Commissioner Plowden, many years ago in the Bancorah district, one of the smallest portions of the Burdwan division. Deadly snakes were brought in at the rate of some 1,200 a day, and although the scale was only from threepence to sixpence apiece, in about a couple of months 10,000*l.* was drawn out of the treasury, and the Government ordered the snake crusade to be stopped.

MOUNT WASHINGTON IN WINTER

BY the kindness of an American friend, we have been favoured with a Boston newspaper containing an account of an important meteorological experiment which has recently been tried in America with the greatest success; we refer to the establishment of a winter observatory on the summit of Mount Washington at an elevation of something like 10,000 feet above sea-level.

Everybody knows that Mount Washington, in New Hampshire, is visited by thousands of persons in the summer months, and that its climate corresponds better with that of Labrador or Greenland than with that of New England. In the winter universal desolation reigns there; not even the proprietors of the hotels upon the summit venturing from their snug quarters below to learn what events are transpiring upon the icy cone. The aborigines declared it to be sure death if anyone climbed the mountain—since it was the sanctuary of divinities who would not suffer their abode to be scanned with impunity. Their successors first adventurously found the summit—then erected rude places for shelter, and finally constructed a carriage road and railway, so that even the most feeble persons could view the broad panorama. But these visits were confined to the warmer months, with a few rare exceptions.

Twelve years ago Profs. C. H. Hitchcock and J. H. Huntington, independently of each other, conceived the project of spending the winter upon the summit of Mount Washington, but the project did not take shape till the organisation of the Geological Survey of New Hampshire in 1868. They found it impossible to make the necessary preparations for occupying the summit during the winter of 1868, chiefly for want of a dwelling. Hence they sought for a less elevated summit, where a single winter's experience might prepare the way for the greater adventure. That peak was Mooselauke, nearly 5,000 feet above the ocean. The lessee, D. G. Marsh, of Warren, N.H., obligingly placed his house at the service of Prof. Huntington and his comrade, A. F. Clough, of Warren, photographer. Their three months' occupation of Mooselauke was full of adventure, and experiences were acquired of the highest importance. The scientific results were important, disclosing the knowledge of violent winds there accurately measured, and remarkable forms of frost-work never before described or photographed. Attention was called to this mountain, and a carriage road became a necessity, which was constructed in the following summer.

In the month of September, 1869, the Mount Washington Railway Company generously tendered the use of their depôt upon the summit to this meteorological party during the winter; and the necessary supplies were immediately purchased and forwarded to the mountain. The enterprise, though of a meteorological character, has been adopted by the Geological Survey of the State, while the expense has been assumed by the State geologist, relying upon a sympathising public to provide the funds by subscription.

Congress recently appropriated funds for the establishment of a "Bureau of Telegrams and Reports for the Benefit of Commerce." After some correspondence with the efficient officer in charge of this bureau, General Myer ordered an insulated telegraph wire with suitable instruments to be sent to the mountain, in order to facilitate the transmission of the meteorological reports, both to the public and to the office of the bureau in Washington. The wire has been laid, and the summit is now in telegraphic communication with the world. And the chief signal officer also detailed for special service upon the mountain an experienced telegrapher and meteorologist, Sergeant Theodore Smith, of the U.S. Army.

The photographers of the expedition are Mr. A. F. Clough, of Warren, and Mr. Howard A. Kimball, of Concord, N.H. The latter gentleman spent much time in providing photographic material for the mountain, and in soliciting subscriptions. Their views of the peculiar phenomena of the mountain will soon be exhibited.

Thus the party consists of six persons: Prof. Hitchcock, whose office in Hanover, N.H., is connected by telegraph with the summit; Prof Huntington and Mr. Nelson, observers; Messrs. Clough and Kimball, photographers; and Sergeant Smith, telegrapher and observer. All of them are not upon the mountain at the same time. They relieve each other to a considerable extent in the work, and the public will be kept informed of their whereabouts.

From the dépôt of the Mount Washington Railway in summer, the ascent on foot, if a person is accustomed to walking, is comparatively easy. Although the ties are three feet apart, and there is a rise of a foot in three part of the way, yet a person with muscles strong from exercise, can walk to the very summit of the mountain without sitting down to rest. But suppose it is winter. The snow has accumulated to a considerable depths even in the ties, but then it is no great hindrance; should it, however, be attempted a second time, you will find that the snow that was compressed beneath the feet has changed to ice, and the oval forms give a still less secure footing; if it is thawing, and the ice is almost ready to slip off as you tread upon it, every one will see that upon a trestle thirty feet high walking is *somewhat* dangerous, and to walk down is a feat from which even a most expert acrobat would shrink. If at the dépôt we take snow shoes, we can walk with comparative ease half way up, and then the snow is so compact that they are no longer needed, and as there are few irregularities in the surface the walking is better than in summer.

Above the limit of the trees the railway is covered with ice of every fantastic shape, and the framework of the Gulf tank is now so ornamented that one can hardly believe that it is the rude structure seen in summer. The Lizzie Bourne Monument, which one is accustomed to see only a rough pile of stones, is now an object of architectural beauty such as no sculptor can carve from marble. Immediately above the monument the timber trestles are completely covered with deposits of frozen mist extending two or three feet horizontally from the timber on which the track is laid, and every piece of timber which forms the trestle is ornamented with beautiful forms of frost-work, deposited in graceful curves as the wind sweeps through the trestle.

On the summit, the buildings, the piles of rock and stones, so rough in summer, are now completely covered with frost, while the snow fills the spaces between the jagged rocks. On the sides of the buildings towards the north-west the frost has accumulated, so that now it is more than a foot in thickness. The frost-work on the dépôt, while it has everywhere the same general appearance, the points show exactly the direction of the wind as it came into every nook and corner of the building. The frost on the braces and timbers that extend outward seems like one triangular mass, and on the chains it is often two feet in diameter. The correspondent then adds:—

"Although I was on the mountain for ten days in October, yet I did not go to stay permanently until November 12. It was expected that some of the party would be ready to go with me, but as they were not I went alone, and it was not till the last day of November that anyone came, so that I had the house and the mountain all to myself for nearly three weeks. During the greater part of November the weather was remarkably pleasant. On the 15th, all day long, and far into the night, the clouds were below the summit of the mountain, and most of the time they covered the entire country. At times only the very highest point of Mount Washington was above the clouds. It reminded me forcibly of the time, during the Champlain period, when the whole of New England, except Mount Washington, was beneath the waves of the ocean. As, the masses of clouds came over Adams and Jefferson, I looked until I almost persuaded myself that there were immense icebergs coming from the uplifted frozen North, but the illusion vanished as the summits appeared above the clouds. But what was remarkable is that the next day was pleasant.

"During the last two weeks in November, the average of the thermometer was 16°. The wind was north-west nearly the whole time. The weather was generally very mild, and the number of clear days was much greater than the average."

We may add to this interesting account that the daily telegrams received from the mountain have been full of interest. A temperature of 40° below zero has not been uncommon, and we may well hope that the efforts of the solitary watchers on the inclement mountain-top will tend to advance the science of which they are such devoted students.

SCIENCE IN VICTORIA

THOSE who have read the Marquis de Beauvoir's "Voyage round the World" will recollect the high praise with which he speaks of Sir Redmond Barry's prolonged and continuous efforts for the advancement of Science in Victoria. His latest good deed in this direction is the aid which he has afforded in the establishment of a School of Mines at Ballarat, which, to use his own words, may now be regarded as "an accomplished fact," since "everything connected with its establishment and maintenance has been thoroughly debated and determined on." The prospectus of the Institution states that "the object sought to be obtained is the combination of the highest scientific with the most practical training for all men engaged in the enterprise of mining in its various branches — whether so engaged as mining managers, engineers, surveyors, mechanists, working miners, directors or promoters of companies;" and, as there is every reason to believe that the school will be supported by the Government, by the several mining boards, and by the general public, we may hope to see it in actual operation in a few months.

We have not space to give a sketch of Sir Redmond Barry's excellent address, further than to remark that he points out the general bearings of chemistry, electricity, geology, steam, &c., on the progress of mining; and we must content ourselves with the following extract relating to the progress of Science in Australia:—

"We are not," he observes, "even in our time, satisfied to import the discoveries of others, and to invite the man of science and the skilled artisan to direct their operations. Australia can point with a modest satisfaction to the invention of Osborne in photo-lithography, and the test for blood by Dr. Day;[*] to the cure for snake-bites by Professor Halford; to the method of removing pyrites, that inveterate enemy of the metallurgist, as well as the native alloys, from gold, and toughening the metal by the use of chlorine gas, by Mr. Miller of the Sydney Mint; to the scientific preservation of the meat of our redundant flocks and herds, so largely exported; and also to many others, mention of which would delay us too long."

THE CONTRACTION OF THE EARTH[†]

THE phenomena, which were ably presented by the distinguished geologist, Mr. Lesley, to the National Academy of Sciences, and which seem to demonstrate that the outer shell of the earth has sensibly shrunk, in some directions at least, since its original formation, naturally invite the attention of physicists to the possible causes of such a result. The most obvious cause of the shrinking of the earth is its cooling. But to shrink two per cent. linearly, which is the amount deduced by Mr. Lesley from the observed geological phenomena, involves a probable cooling of the whole earth of not less than two thousand degrees centigrade, which would require that its original temperature should be higher than would be consistent with the solidity of these shrunk strata.

Another source of change of form, which would produce shrinkages in different directions in different parts of the earth, is to be found in the diminution of oblateness arising from the diminished velocity of rotation upon the axis. Such diminution of the velocity of rotation has several years ago been shown by Mr. Ferrel to be caused by the action of the moon in producing the tides; this is, therefore, a true cause, and it is only necessary to examine how great its amount can be under any circumstances. This is all which is proposed in the present investigation, and the application to facts is reserved for geologists.

It is sufficient, for the present object, to regard the earth as homogeneous. Under this condition Laplace has shown that the time of the earth's rotation could not be less than about one-tenth of a day, which corresponds to a ratio of the axis of the equator to that of the pole, equal to 2·7197, and an equatorial circumference 94 per cent. greater than the present one. Such is then the amount of shrinking which might have taken place, if any cause could be assigned capable of producing so great a reduction of the earth's velocity. The whole surface of the earth would have been about 130 per cent. larger than at present.

But the only cause at present known which would produce a sensible reduction of the earth's velocity is the lunar action upon the tides. But in this mutual action between the moon and the earth, the common rotation area of the earth and moon must remain unchanged. The question then arises, How great a reduction of the rotation area of the earth would have passed into that of the moon? In this inquiry it may be assumed that the moon revolves in a circular orbit in the plane of the earth's equator.

Now the moon's rotation area is 3·716 times the earth's. But if, in the origin, it had revolved just in contact with this earth, its rotation area would not have been less than 0·480 times the earth's, so that it could not have absorbed a rotation area from the earth greater than 3·236 times the earth's present rotation area, and therefore the earth's rotation area could never have exceeded 4·236 times that which it has at present. But, with the maximum velocity of rotation given by Laplace, the earth's rotation area would have been $37\frac{1}{2}$ times greater than at present. It can never, therefore, have been reduced to so great an extent by the moon's action on the tides. But since, when the oblateness is small, the rotation area is nearly proportional to the velocity; and the excess of the square of the equatorial above that of the polar axis is nearly proportional to the square of the velocity, this excess may have been originally nearly eighteen times as great as at present, or about $15\frac{1}{2}$ per cent. of the square of the polar axis. This would correspond to a figure of the earth in which the equatorial radius would have been about $2\frac{1}{3}$ per cent. greater than at present; so that it is sufficient to account for the observed phenomenon.

This peculiar form of shrinkage would produce the highest mountains at the equator, and the tendency of the mountain ranges would then be to assume the direction of the meridian. But nearer the poles the mountains would be less elevated, and would rather tend towards the direction of the parallels of latitude.

It is, next, expedient to consider the mechanical question of the loss of living force in the case of the moon's action upon the waters of the earth, and its effect upon their different motions. In this connection there are problems worthy of the attention of geometers; such as the relative motions of bodies rotating above the same vertical axis, towards which they are drawn by weights, and acting upon each other through the friction on the axis. For one of the bodies a rotating wheel may be substituted. There is also the case of two planets revolving about a primary, and acting upon each other through some form of friction.

In this way it will be seen that the planet or satellite once formed is constantly removed from the primary, and that planets tend to approach each other. It is interesting to consider whether this may not be one of the actual problems of nature.

B. PEIRCE

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[*] A full account of Dr. Day's remarkable colour-tests for blood and pus may be found in Dr. Richardson's Report on Toxicology in the last number of the *Medico-Chirurgical Review.*—ED.

[†] From the Proceedings of the American Academy of Arts and Sciences, vol. viii.

SOCIETIES AND ACADEMIES

London

Royal Society, February 9.—"Abstract of Paper on the Effect of Exercise upon the Bodily Temperature." By Dr. T. Clifford Allbutt. The object of the author in carrying out the experiments recorded in the present paper was to inquire whether the regulating power of the organism held good under great variations of muscular exertion. For this purpose he made frequent daily examinations of his own temperatures during a short walking tour in Switzerland, and found that the effect of continuous muscular exertion upon himself was to sharpen the curve of daily variation, the culmination being one or two-tenths higher than usual, and the evening fall coming on more rapidly and somewhat earlier. Charts of the daily temperatures were handed in with the paper. The author made reference also to some observations of M. Lortet, which differed from his own. These observations, which did not come into Dr. Clifford Allbutt's hands until his own experiments were partially completed, were adduced by M. Lortet to prove that the human body was very defective in regulating power under the demands of the combustion needed to supply the force expended in muscular exertion. Dr. Clifford Allbutt's results were very decidedly opposed to those of M. Lortet; for only on two occasions did he note the depressions of temperature which M. Lortet regards as constant. It would seem, however, that the body is more or less liable to such depressions when engaged in muscular exertion; but the cause of them is very obscure. Of the two low temperatures noted by the author, one occurred during a very easy ascent of lower slopes, and the second was observed during a descent. The author thinks that they may be due to some accidental deficiency in combustion, and inquires whether the capacity of the chest in different individuals may account for the varying influence of muscular effort upon them, and perhaps for the earlier or later sense of fatigue. The sphygmographic tracings added by M. Lortet to his temperature charts seemed to show a great inadequacy of circulation.—"Observations of the Eclipse at Oxford, December 22, 1870." By John Phillips, F.R.S.

Geological Society, January 25.—Mr. Joseph Prestwich, F.R.S., President, in the chair. Richard Atkinson Peacock, of St. Helier's, Jersey; Arthur W. Waters, Davos Plaz, Canton of Grisons; R. Koma, of University College, London; and Ransom Franklin Humiston, M.A., Professor of Chemistry in Cleveland University, U.S., were elected Fellows of the Society. The following communications were read: 1. "On the Physical Relations of the New Red Marl, Rhætic beds, and Lower Lias," by Prof. A. C. Ramsay, LL.D., F.R.S., F.G.S. The author commenced by stating that there is a perfect physical gradation between the New Red Marl and the Rhætic beds. He considered that the New Red Sandstone and Marl were formed in inland waters, the latter in a salt lake, and regarded the abundance of oxide of iron in them as favourable to this view. The fossil footprints occurring in them were evidence that there was no tide in the water. The author maintained that the New Red Marl is more closely related to the Rhætic, and even to the Lias, than to the Bunter; and in support of this opinion he cited both stratigraphical and palæontological evidence. He described what he regarded as the sequence of events during the accumulation of the later Triassic deposits and the passage through the Rhætic to the Lias, and intimating that the same reasoning would apply to other British strata, especially some of those coloured red by oxide of iron, including the Permian, the Old Red Sandstone, and a part of the Cambrian. Mr. Etheridge thought the question of the nature of the Rhætic beds was to a great extent palæontological. The main point in connection with them was as to how the British beds were to be connected with the Lombardic and Middle European areas. It certainly seemed probable that in this part of the world the conditions of life were different, the deposits being much less in thickness, and the fauna much diminished; and where represented at all, the shells occurred in a dwarfed and stunted form. The exact horizon and nature of the Sutton beds had still to be determined. Mr. Godwin-Austen believed that every mass of red sandstone would ultimately be referred to either a brackish or freshwater origin. A comparison of the ancient and present area of the Caspian Sea would tend to remove any doubt that might remain on the mind of geologists as to the possibility of the existence of such vast internal seas as those which had to be called in to account for these formations. He regretted that former observers had not attached more importance to the duration and extent of those freshwater conditions which were found so commonly to have prevailed between the periods of deposit of the great marine formations. There was another fact to be borne in mind, that even in existing lakes the water at the one end was sometimes completely fresh, and at the other end salt, each, of course, with a different fauna. Prof. Rupert Jones said that, although there were good grounds for the lake theory, something might be said for shallow seas. He remarked that sulphate of lime was deposited from sea-water before salt, that oxide of iron might originate from chloride of iron diffused in water whether of lakes or seas, and that the hæmatites of Permian age were probably deposited in the sea. He considered that Foraminifera required great caution when used as criteria, as the varietal forms giving the facies were of more importance than the genera and species. The *Estheriæ* were never marine, although often occurring in plenty in temporary freshwater pools on the sea-shore. In his monograph of *Estheria* he had said much to substantiate the notion that freshwater conditions often prevailed during the formation of the Keuper. Both in the Old Red Sandstone of the Baltic provinces and in the Lettenkohle and Keuper of Germany, when *Estheria* comes in, *Lingula* dies out. The *repeated* set of formations in the Permian and the Trias precludes their contemporaneity, as supposed by Messrs. Godwin-Austen and Marcou. Mr. Bauerman marked that the Hallstatt beds which had been cited as marine contained large deposits of rock-salt. M. Marcou thought that the difficulties in regarding these beds as of freshwater origin were greater than the author supposed. The absence of fossils in gypsum, though almost universal, was not total. He had himself seen three specimens of *Trigonia* in gypsum from Stuttgart. Mr. Tate mentioned the discovery by Mr. Burton of marine fossils in the Red Marl, in one instance in combination with vegetable remains. He commented on the sharp demarcation observable in Ireland between the Rhætic beds and the marl below, whereas it was almost impossible to separate them from the Lias above. He doubted, however, whether the true relations of the Rhætic beds were to be worked out in this country. As to the fossils of the Sutton Stone, they were all purely Liassic. Mr. Burton stated that the fossils from the Red Marl came from a spot about five miles from Retford, in the direction of Gainsborough, but he had not seen them *in situ*. There are, however, no Rhætic beds within some miles. Rev. Mr. Winwood, in the absence of Mr. C. Moore, from ill health, inquired whether the author regarded the White Lias as Rhætic or Liassic. Prof. Ramsay, in reply, was quite willing to accept marine fossils as coming from the Red Marl. The fact of *Estheria*, a brackish or freshwater form, occurring in certain bands, was in favour of his views, as he considered that at intervals the freshness of the water in such a lake as he had suggested must have varied. He could not accept the probability of oxide of iron having been deposited in a large sea area to such an extent as to colour the sands. All rocks that could be proved to be of marine origin, even when they contained iron, were not stained red unless by infiltration from above. He pointed out that the old area of the Caspian was far larger than the lake in which he had suggested that the New Red Marl had been deposited. If, as was more than probable, there had been during all geological time continental areas somewhat in the same positions as those of the present day, there must have been large areas of inland drainage in which some such deposits as those in question must of necessity have been formed.—2. "Note on a large Reptilian Skull from Brooke, Isle of Wight, probably Dinosaurian, and referable to the genus *Iguanodon*." By J. W. Hulke, F.R.S., F.G.S. The author stated that the skull described by him was obtained from a Wealden deposit at Brooke, in the Isle of Wight, from which many remains of Dinosauria have been obtained. He described its characters in detail, and remarked that its most striking peculiarities were:—the completeness of the bony brain-case; the obliteration of the sutures, especially those of the basicranial axis; the massiveness of the skull; and the great downward extension of the basisphenoid, with the attendant upward slant of the lower border of the basipresphenoidal rod. The first of these characters occurs elsewhere among reptiles only in *Dicynodon*; and the first and second characters combined were regarded by the author as approximating the skull to the ornithic type. The reference of this skull to *Iguanodon* was founded chiefly on the place from which it was obtained, which has furnished abundant remains of that genus, and on the obliteration of the sutures, which the author stated to be a character of the mandibles of *Iguanodon*. Prof. Huxley congratulated the society on the progress being made in our knowledge of this interesting group of reptiles and of their ornithic affinities. Mr. Seeley remarked on the similarity of the internal cavity of the skull to that of *Ichthyosaurus*. Some of the external characteristics differed much from what he was acquainted with in other Dinosaurian skulls, which more closely resembled those of ordinary lizards. He considered that the affinities of *Dinosaurs* were in the direction of *Teleosaurus*, from which the position of what were supposed to be the optic nerves in this skull materially differed. On the whole, he was not at once prepared to accept this skull as that of an Iguanodon. Mr. Hulke briefly replied, and observed that he had limited his speculations to those which legitimately arose from the facts before him. The following specimens were exhibited to the meeting:—Specimens from the Keuper Marls and Rhætic beds near Gainsborough; exhibited by Mr. F. M. Burton, F.G.S. Examples of the borings of two species of *Pholas;* exhibited by Mr. E. Charlesworth, F.G.S.

Zoological Society, February 7.—Mr. G. R. Waterhouse, Vice-President, in the chair.—The Secretary read a report on the additions to the Society's Menagerie during the month of January, amongst which were particularly mentioned a specimen of the kakapo, or night-parrot of New Zealand (*Strigops habroptilus*).—Mr. J. E. Harting exhibited and made remarks on a fine specimen of the red-breasted goose (*Anser ruficollis*) lately killed in England.—Mr. H. E. Dresser exhibited some specimens of rare European birds'-eggs, amongst which were those of *Micronisus brevipes* and *Reguloides superciliosus*.—Mr. E. Ward exhibited a skin of a white variety of the tiger (*Felis tigris*), obtained from an animal killed in the Mirzapore district.—Mr. W. B. Tegetmeier exhibited a specimen of an eel, believed to be new to the fauna of Great Britain. It had been obtained from fresh-water in the Scilly Islands, and was referred by Dr. Günther to a variety of *Anguilla vulgaris*, called *A. cuvieri* by Kaup.—A communication was read from Dr. Robert O. Cunningham, containing notes on some distinctive points in the osteology of *Rhea americana* and *Rhea darwinii*.—Mr. J. E. Harting read a paper on the Arctic collection of birds presented by Mr. John Barrow to the University Museum, Oxford, and drew attention to some interesting facts in connection with the geographical distribution of birds in northern latitudes.—A communication was read from Professor Carl J. Sundevall, containing an account of the birds obtained in the Galapagos Islands, during the voyage of the Royal Swedish frigate *Eugenia*.—Mr. R. B. Sharpe read a paper on the birds of Angola, founded on collections made in that country by Mr. J. J. Monteiro and Mr. Charles Hamilton, being his third communication on this subject.—Mr. J. Verreaux pointed out the characters of a new species of *Promerops* from Natal, which he proposed to call *Promerops gurneyi*.—Dr. J. E. Gray communicated a description of *Platasterias*, a new genus of *Astropectinidæ* from Mexico.—A communication was read from Mr. D. G. Elliot, containing the description of a new species of pheasant of the genus *Euplocamus* from Burmah, proposed to be called *Euplocamus andersoni*.—Dr. John Anderson pointed out the characters of three new species of squirrels (*Sciurus*) recently obtained by him during the Yunan Expedition. Dr. Anderson also gave an account of a new Cetacean which he had lately discovered in the Upper Irrawaddi, and which he proposed to call *Orcela fluminallis*. A third communication from Dr. Anderson contained a note on the occurence of the remarkable parasitic Crustacean *Saccalina* in the Bay of Bengal. The species, which had been found on the common swimming crab of that district (*Thalamita crenata*) did not appear to differ from that which is found on *Carcinus mænas* on the shores of Great Britain.

Royal Institution, February 6.—Monthly General Meeting. Sir Henry Holland, Bart., F.R.S., President, in the chair. Henry Edward Colvile, Esq.; John H. Dallmeyer, Esq.; Duncan Davidson, Esq.; Warren William De la Rue, Esq.; James N. Douglass, Esq.; Mrs. Gibbs; Abraham Goodall, Esq., F.R.C.S.; Alexander Macfarlan, Esq.; Robert Turtle Pigott, Esq., F.R.G.S.; Robert Sabine, Esq.; Charles Southwell, Esq.; Henry Stilwell, Esq.; Richard Valpy, Esq.; Mrs. Jacob Waley, were elected members of the Royal Institution. The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same.

Entomological Society, February 6.—Mr. A. R. Wallace, President, in the chair.—Pastor Kawall, of Pussen, Kurland, was elected a corresponding member.—Mr. Bond exhibited *Pachinobia alpina*, *Gelacha borcella*, varieties of *Thera juniperata*, and *Larentia cæsiata*, and portions of web of *Hyponomeuta padi* over a yard long, all from Perthshire; also photographs of eggs of bird parasites, taken from slides prepared by Mr. Norman. He likewise exhibited a curious instance of monstrosity in *Vanessa Atalanta*, the butterfly still retaining the larval head. —The Rev. H. S. Gorham exhibited *Oxytelus fulvipes* of Erichson, a new British beetle.—Mr. Muller exhibited specimens of oak-galls from Tangiers, collected by Mr. Blackmore.—Prof. Westwood exhibited a minute species of *Corixa* from India, destructive there to the eggs of the most valuable freshwater fish; also drawings of a species of *Coccidæ* injurious to the leaves of an exotic *Cypripedium*. The male shield consisted of a small disc, with six raised radiating lines produced into spines. —Mr. Butler read a descriptions of a new genus, and of seven new species of *Pierinæ*.

MANCHESTER

Literary and Philosophical Society, January 24. — Mr. E. W. Binney, president, in the chair.—Mr. Brothers exhibited a drawing from the fine photograph of the solar Corona, taken by him at Syracuse, during the late total eclipse of the sun.—Dr. Joule, F.R.S., read the following letter, dated January 21, 1871, which he had received from Mr. William H. Johnson, of Bowdon:—"Since the last meeting of the Philosophical Society I have made some further experiments on the 'Effect of cold on the strength of iron.' In these I have maintained a nearly fixed temperature, and thus avoided to a great extent the error occasioned by the rise in temperature, consequent on sudden torsion. January 11. A piece of a charcoal wire rod, ·237in. diameter, gave the following results:—

	1st.	2nd.	3rd.
At about 40° F.	20 twists	19 twists	17 twists.
Adjacent 6" at temperature of melting zinc	10 twists	9 twists	7½ twists.

	4th.	5th.
Twisted very slowly, surrounded by salt and snow	19¼ twists.	16 twists.
Adjacent 6" at about 40° F	15 twists	

The twisting under salt and snow was performed so slowly, each experiment lasting a quarter of an hour or more, that the temperature cannot have been affected by the torsion. The same care was taken at the temperature of 40° F. The great diminution of strength at the melting point of zinc is remarkable. I take the liberty of communicating these results to you, as unfortunately I shall be away at the next meeting, and thus shall not have an opportunity of seeing you." Mr. Brockbank remarked that these experiments did not affect the conclusions stated in his paper, read at the last meeting. He believed that the strength of iron under torsion was most affected by the heat developed by the twisting, and that the cooling mixture employed by Mr. Johnson would have the effect of making the wire stand a greater number of twists by counteracting the excessive heat produced by the torsion. Mr. Brockbank exhibited a drawing of the machine used by him in his experiments on the strength of cast iron at different temperatures.—"Experiments on the Oxidation of Iron," by Prof. E. Crace Calvert, Ph.D., F.R.S., &c. Some two years since Sir Charles Fox inquired of me if I could give him the exact composition of iron rust, viz., the oxidation found on the surface of metallic iron. I replied that it was admitted by all chemists to be the hydrate of the sesquioxide of iron, containing a trace of ammonia; to this he answered that he had read several books on the subject, in which the statements referring to it differed, and from recent observations he had made, he doubted the correctness of the acknowledged composition of iron rust. He further stated that if he took a bar of rusted wrought iron, and put it in violent vibrations, by applying at one end the fall of a hammer, scales would be separated which did not appear to him to be the substance I had described. This conversation induced me to commence a series of experiments which I shall now detail. I first carefully analysed some specimens of iron rust, which were procured, as far as possible, from any source of contamination. Thus one of these samples was supplied to me by Sir Charles Fox, as taken from the outside of Conway Bridge, the other secured by myself at Llangollen, North Wales. These specimens gave the following results when submitted to analysis:

	Conway Bridge.	Llangollen.
Sesquioxide of iron	93·094	92·900
Protoxide of Iron	5·810	6·177
Carbonate of iron	0·900	0·617
Silica	0·196	0·121
Ammonia	Trace	Trace
Carbonate of lime	——	0·295

These results clearly show the correctness of Sir Charles Fox's foresight, that the composition of the rust of iron is far more complicated than is stated in our text-books. Therefore the question may be asked, Is the oxidation of iron due to the direct action of the oxygen of the atmosphere, or to the decomposition of its aqueous vapour; or does the very small quantity of carbonic acid which it contains determine or intensify the oxidation of metallic iron? To reply to it I have made a long series of experiments, extending over two years, and which I hope will throw some light on this very important question. Perfectly cleaned blades of steel and iron, having a gutta percha mass at one end, were introduced in tubes which were placed over a mercury trough, and by a current of pure oxygen conducted to the top of the experimental tube; the atmosphere was displaced, and it was then easy to introduce into these tubes traces of moisture, carbonic acid, and ammonia. After a period of four months, the blades of iron so exposed gave the following results:—

Dry Oxygen	No oxidation.
Damp ,,	In three experiments, only one blade slightly oxidised.
Dry Carbonic Acid	No oxidation.
Damp ,,	Slight appearance of a white precipitate of the iron, found to be carbonate of iron. Two only out of six experiments did not give these results.
Dry carbonic acid and oxygen	No oxidation.
Damp oxygen and carbonic acid	Oxidation most rapid, a few hours being sufficient. The blade assumed a dark green colour, which then turned brown ochre.
Dry oxygen and ammonia	No oxidation.
Damp ,, ,,	No oxidation.

The above results prove that under the conditions described, pure and dry oxygen does not determine the oxidation of iron, that moist oxygen has only feeble action; dry or moist pure carbonic acid has no action, but that moist oxygen containing traces of carbonic acid acts most rapidly on iron, giving rise to protoxide of iron, then to carbonate of the same oxide, and lastly to a mixture of saline oxide and hydrate of the sesquioxide of iron. These facts tend to show that carbonic acid is the agent which determines the oxidation of iron, and justify me in assuming that it is the presence of carbonic acid in the atmosphere, and not its oxygen or its aqueous vapour, which determines the oxidation of iron in common air. Although this statement may be objected to at first sight, on the ground of the small amount of carbonic acid gas existing in the atmosphere, still we must bear in mind that a piece of iron, when exposed to atmospheric influences, comes in contact with large quantities of carbonic acid during twenty-four hours. These results appeared to me so interesting that I decided to institute several series of experiments. When perfectly clean blades of the best quality of commercial iron are placed in ordinary Manchester water, they rust with great facility, but if the water is previously well boiled and deprived of oxygen and carbonic acid, they will not rust for several weeks. Again, if a blade of the same metal is half immersed in a bottle containing equal volumes of pure distilled water and oxygen, that portion dipping in the water becomes rapidly covered with the hydrate of the peroxide of iron, whilst the upper part of the blade remains for weeks unoxidised; but if a blade be placed in a mixture of carbonic acid and oxygen, a very different chemical action ensues, as not only that portion of the blade dipping in the water is rapidly attacked, but the upper part of it immediately shows the result of chemical action, and also the subsequent chemical reactions are greatly modified by the presence of the carbonic acid. For in this case that portion of the blade is only covered with a film of carbon, together with a dark deposit composed of carbonate of the protoxide and hydrate of the sesquioxide. The fluid, instead of remaining clear, becomes turbid. These series of experiments substantiate the interesting fact observed—that carbonic acid *promotes* oxidation. A long series of experiments were also made to try and throw some light on the curious fact, first published by Berzelius, subsequently studied by other chemists, and well known to soap and alkali manufacturers, namely, that caustic alkalies prevent the oxidation of iron; my researches can be resumed as follows:—(1) that the carbonates and bicarbonates of the alkalies possess the same property as their hydrates; and (2) that if an iron blade is half immersed in a solution of the above-mentioned carbonates, they exert such a preservative influence on that portion of the bar which is exposed to an atmosphere of common air (oxygen and carbonic acid), that it does not oxidise even after a period of two years. Similar results were obtained with sea water, to which had been added carbonates of potash and soda.

Microscopical and Natural History Section, January 9.—Mr. J. Baxendell, President of the Section, in the chair. "On *Carex flava* L., and its allies, of the Manchester Flora," by Charles Bailey. The prevailing form in the district, and one very common to the south of Manchester, is the *Carex lepidocarpa* Tausch.; this is the *C. Œderi* Sm., and of Grindon's Manchester Flora, and the *C. flava* var. β of Buxton's Guide. The true *C. flava* (α *genuina* E.B.), as stated long ago by Mr. Buxton, is nowhere met with in the district. Specimens of *C. Œderi* Ehrh. from Mere Mere, the locality mentioned in Buxton's Botanical Guide, were recently exhibited at a meeting of the Society, and the sandhills at Southport are, so far as I know, the only locality in the neighbourhood for this species.

TAUNTON

Somersetshire Natural History Society, February 6.—The following notice was read by Mr. Cecil Smith.—The Great Bustard has now so long been considered extinct in England, that we may look upon it as interesting in an archæological as well as in a natural history point of view. This bird, one of a flock of eight that made their appearance on the last day of the old year at Braunton, near Barnstaple, in North Devon, is probably a young hen bird of the first, certainly not more than the second, year. On looking at this bird, perhaps, one of the first things that strikes one is the shape of the foot, the three toes in front and no hind toe; and this brings me at once to the subject of classification or order. Yarrell, whose system is best known and most generally adopted, has made the bustards a group of the Rasoreal order, where I cannot help thinking they are not a little out of place, there being no very nearly allied group in that order. I am much more inclined to agree with Baron Cuvier and some of the other older systematic authors who placed the bustards amongst the Grallatores or Stilted birds, where they seem naturally to fall into place next to their near relations, the Plovers; the absence of the hind toe, a conspicuous mark of the true Plover, would at once suggest this position. The form of the sternum or breast bone also points to a relationship with the plovers rather than with any of the Rasores. Another point which can scarcely be passed over in silence in a paper on the Great Bustard is the gulor pouch. It seems surprising that the use, and even the existence, of this pouch has so long been a matter of doubt and perplexity to naturalists. Much light has, however, lately been thrown on the subject by Prof. Newton, Mr. Bartlett, and Dr. Murie, and it seems finally to have been set at rest by Dr. Cullen, the result of whose examinations has been reported in the *Ibis* for 1865. He readily found the opening into the pouch under the tongue, and describes it as large enough to admit the little finger; the pouch itself, he says, extended as far down as the furcular bone, and was a separate and distinct, though delicate, bladder. He then describes the performances of the male bustard in the breeding season, at which time, he says, it makes a peculiar sound, resembling the word "ook," and he strongly favours the idea that the pouch is merely an organ of sound, and that it is acted upon by the muscular tissue covering it; in fact, that it is a sort of bird bag-pipe, and seems not to produce much more melodious sounds. Although the Great Bustard was formerly resident in England throughout the year, it is generally a migratory bird, its migratory propensities being much developed by stress of weather and scarcity of food; whether either of these causes or the war in France, has been the reason for the present unusual migration to England is perhaps difficult to say; myself, I should be inclined to think bad weather and the consequent loss of food the more probable cause. The gradual extinction of the Great Bustard in England has been the general theme of most of onr writers on ornithological subjects from the time of Bewick and Montagu to the present time of Mr. Stevenson, who in his still unfinished work on the "Birds of Norfolk," gives a most interesting account of the decline and fall of the Great Bustard in that county. In Devonshire, where this bird was killed, the Great Bustard, although never resident, seems from time to time to have paid occasional migratory visits. Montagu mentions the occurrence of one near Plymouth in the year 1798, two more in the next year, and one in 1864. Another Devonshire specimen occurred, after a long interval, on the 31st December, 1851, near Clovelly, and was recorded by Mr. Gatscombe, in the *Naturalist*. The 31st December seems to have been a favourite day with the Great Bustard in North Devon, for the flock, of which this present specimen was one, appeared on Braunton Burrows, near Barnstaple, on that day last year; the flock consisted of eight, and was first observed in a field near Croyde, where two were killed and one wounded. The remainder of the flock then alighted. Some boys, who were sliding close to Braunton, pelted them with stones, upon which the birds flew off, and were not heard of for some days; subsequently, I heard they were seen at Halsworthy, in the west of Devon, not very far from the border of Cornwall, but none were obtained there. In Devon they were considered to be wild turkeys, and the following paragraph appeared in the *North Devon Journal*:—"Wild fowl.—During Christmas week a flock of eight wild turkeys visited this parish, and alighted in a field at Croyde. They were seen by Mr. William Nuich, who followed and shot one which weighed 9lb., and was much admired. The others took their flight to the west, and have not since made their appearance." This migration of the Great Bustards was not confined to Devon alone, for in the February number of the *Zoologist*, specimens are reported from Middlesex, Northumberland, Wiltshire, and Somersetshire. The Somersetshire specimen is of course the most interesting to us: it was seen by Mr. Harting on the 27th of September, when journeying by rail from Bishops Lydiard to Wells in the low marsh country near Shapwick. His attention was first attracted by seeing a bird crouch at the approach of the train. He kept his eyes on the bird until the distance was considerably decreased, when the bird jumped up and ran swiftly away, exhibiting to his astonishment the long legs and white flanks of a bustard. Mr. Harting subsequently published an account of his interview with this bustard in the *Field*. He seems to have no doubt himself that the bird he saw was a bustard, and as he seems to have had sufficient opportunity to identify it, we must therefore take it that this Shapwick bustard was the pioneer of the December migrants, and that we have to thank Mr. Harting for this important addition to our Somersetshire avi-fauna. Although this migration has refreshed our memory of the Great Bustard for a time, I am afraid we must in England look upon it as a bird of the past, certainly as a resident, one or two migratory appearances like the present may no doubt from time to time stir up the ornithological world, and cause a temporary excitement; but that a bird of such considerable size and conspicuous plumage should for any time continue to exist in such a highly cultivated and thickly inhabited country as England seems impossible. Should it do so in any considerable numbers we should very soon have an outcry from some of our farmers, as it is addicted to feeding upon corn, both when green and ripe, and is moreover especially fond of turnip greens. No doubt in more ways than one the present system of high farming and the amount of machinery used is most fatal to the Great Bustard as a resident, and we must soon look upon him as having his memory perpetuated like the Druid,* only as the sign of a public house, and being classed by some future Mr. Weller with a "griffin, a unicorn, or a King's Arms, as is well known to be a collection of fabulous animals."

PHILADELPHIA

Academy of Natural Sciences, Nov. 8.—Mr. Thomas Meehan referred to a potato presented to the Academy some months ago by Mr. Henszey, a member, which had the appearance of one potato growing out of the centre of another. The opinion of all who saw it was that it was really a case of this kind. It had been handed to him by the curators, and on dissection, though no exact place of origin could be traced, there seemed nothing to indicate any other theory of origin than that one potato had really grown out of the centre of the other. But there were serious physiological reasons in the way of such a theory. A potato tuber is really but a thickened axis, in which the greater part of the interior structure would be incapable of developing a bud which would produce a tuber such as this one had done. The origin of a new tuber from an old one would be nearer the old one's surface. He had been looking for some further explanatory facts, and believed he had them here this evening, in the potato tubers he now handed to the members. They were about the size of hen's eggs, and were pierced in every direction by stolons of the common couch grass, *Triticum repens*. They had gone completely through, as if they were so much wire, and in one instance two tubers had become strung together by the same stolon, as if they were two beads on a string. One would suppose that the apex of the stolon, when it came in contact with the hard surface of the tuber, would turn aside and rather follow the softer line of the earth; but there was no appearance of any inclination to depart from their direct course. They had gone apparently straight through. He had no doubt the potato before referred to was a similar case, a potato stolon had penetrated another potato, and instead of going through as these grass spears had done, terminated in the centre, and formed the new potato there. It was worthy of thought whether so much attention had been given to this direct force in plants as the subject deserved. It was well known that a mushroom would lift a paving-stone many times its own weight, rather than turn over and grow sideways, which it would appear so much easier for it to do; and tree roots growing against walls would throw immensely strong ones over, though one would think the pressure against the softer soil would give room for their development, without the necessity of their expending so much force against the wall.

November 15.—Dr. Ruschenberger, President, in the chair. Prof. Leidy directed attention to some fossil bones which had been submitted to his examination by Prof. J. D. Whitney. According to the accompanying label, they were found under Table Mountain, near Shaw Flat, Tuolumne Co., California. The bones are friable, and have attached portions of a light ash coloured gravel. Several masses of the latter substance accompanying the bones, contain casts of some fruit. Prof. Leidy further directed attention to a fossil fragment of the lower part of a small pachyderm, which Prof. Hayden had obtained from Henry's Fork of Green River, Wyoming. The specimen contained the fourth, the sixth, and the seventh molars. The teeth resemble in form and constitution those of the *Lophiotherium cervulum*, a small pachyderm, described by Prof. Gervais, from an Eocene formation of France. The crowns of the fourth to the sixth molars have four lobes; that of the seventh molar has an additional lobe. The crescentic summit of the postero-external lobe joins, by its anterior horn, the antero-internal lobe. A proportionately well developed basal ridge embraces the crowns, except internally, where it is entirely absent. The series of the back four molars occupies a space of 16 lines. The last molar is 5½ lines fore and aft. The base of the jaw is nearly straight the length of the fragment, which is an inch and a half. The depth of the jaw below the fifth molar is about an inch. The species may be named *Lophiotherium sylvaticum*.

November 29.—Dr. Ruschenberger, President, in the chair. On "Bud Varieties," by Thomas Meehan.—A few years ago Mr. Isaac Burk called my attention to a form of *Rubus villosus*, L., in which the terminal leaflet was very large, cordate, and on very long petioles. It is a very striking variety, the leaflets appearing at first glance like large linden leaves. On the idea that varieties originated from one common centre, it is not easy to account for the existence of the same forms so many miles apart, as we find in the above, except by the accidental carrying of seeds. But I have reason to believe that seeds of *Rubus* rarely germinate in a wild state. In experiments which I have made in raising the seeds artificially, none of the seedlings come exactly like the parent. There is a certain general resemblance, but some distinction, more or less, can be traced in each individual. But, in native places, one exact form will be found to occupy extensive tracts. Sometimes several forms will be together, but only a very few. If the seeds made plants readily, there would be innumerable forms, instead of the very few we see. I found, in my experiments, that it took a long time for a blackberry seed to germinate; sometimes a whole year. Such seedlings have a poor chance to vegetate in a state of nature. Other more rapidly-growing vegetation would crowd it out. The only distributing agency I can think of is that of birds. But I find no birds eat blackberry seeds; and, if they did, when we consider that of the millions of seeds which fall about the place of their origin, few, if any, grow; the chance of those growing which birds may carry, even if there be some to eat them, which I have failed to find, is extremely small. Hence, we find great difficulty in believing that identical forms of *Rubus*, widely separated, can have originated from a common centre. It is well-known that fruits, after being grown for some time in one locality, will change their characters to such an extent that a person acquainted with one will fail to recognise it elsewhere, and all this without the intervention of any seminal power. Thus, the nectarine is believed to be a bud evolution from the peach; the Penn apple is a similar creation from Baldwin, and the Reading from the common Isabella grape. Though apparently originating in this way from external or local causes, the characters peculiar in this change are retained when, by grafts or cuttings, the plants are removed to other localities. I have here, however, and exhibit with this paper, evidence of bud variation, in which there is no possibility of hybridism,—a root of the common sweet potato, *Convolvulus batatas*, in which some of the tubers are of the red Bermuda, and the others of the white Brazilian variety. The sweet potato never flowers in this part of the country, so that seminal power could have had no influence whatever on the phenomenon. Even in the south, and I believe elsewhere, where this plant is cultivated for its roots, it rarely flowers, and I think there is little doubt but that the whole ten or twelve varieties under culture have originated without seed, and in the way we see them here. The points I wish to make in this paper are :—1st. That identical varieties sometimes appear in localities unfavourable to the idea of a common centre of origin. 2nd. Varieties have originated in which probably no hybridism or any seminal agency operated. 3rd. Varieties have certainly originated in the sweet potato by evolution, without seminal agency, and that the same variety in this way has appeared in widely-separated districts. 4th. As the discoveries of Darwin have shown, in many cases, varieties to be the parents of species, species may originate in widely-separated localities by bud variation.—"A Sketch of the Classification of the American Anserinæ," by B. H. Bannister. The following remarks are based upon an examination of the specimens of American geese in the collection of the Smithsonian Institution. The subfamily Anserinæ by many recent authors is made to include the genera *Dendrocygna* and *Chenalopex*, and doubtlessly correctly. In the present paper, however, we shall not consider these genera, leaving them provisionally out of the sub-family; if included, they would form at least one well-marked section, following those we are about to describe. The distinguishing characters of the Anserinæ, as thus limited to the true geese, are, the lengthened tarsus, covered with hexagonal or subquadrate scales; the neck more elongated than in the ducks and less so than in the swans; the short, high bill gradually narrowing toward the tip, which is altogether composed of the large recurved nail; together with the more or less terrestrial habit of life, and the usually similar plumage of the two sexes. The geese of the North American continent have been long known, and being for the most part closely allied to, and in many cases identical with, well known European forms, they fall readily into the systematic subdivisions based upon the latter. Another basis of division of the American Anserinæ is found in the presence, in two species—one North American and the other a Southern form—of deep rough superorbital depressions and reversed relative proportions of the tarsus and middle toe, together with an exclusively sea-coast habitat, and a carnivorous diet, corresponding in some of these respects to the Oidemiæ and Somateriæ amongst the ducks. These latter characters we have taken as the basis of the two sections into which we divide the subfamily, as at present considered, since they correspond with equivalent characters in one of the subdivisions of the Fuligulinæ. The presence of the deep superorbital depressions is a very general character amongst the carnivorous Natatores, though not universal.

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* The Druid and the Bustard are both the signs of public-houses on Salisbury Plain.

BOOKS RECEIVED

English.—The Year-Book of Facts for 1871: J. Timbs (Lockwood and Co).—A Treatise on the Action of Vis Inertiæ in the Ocean: W. L. Jordon (Longmans and Co).

Foreign.—(Through Williams and Norgate)—1er Nachtrag zum Lehrbuche der Aufbereitskund: P. R. von Rittinger.—Biblioteca Malacologica, II, Ipsa Cheireghinii Conchylia di Spiridion Brusina — Populare wissenchaftliche Vortrage: H. Helmholtz.

PAMPHLETS RECEIVED

English —Quarterly Weather Report of the Meteorological Office, July-October 1869.—On the Relations between Chemical Change, Heat, and Force: the Rev. H. Highton —On Ocean Currents: James Croll —On the Cause of the Motion of Glaciers: James Croll.—Letter to the Right Hon Col Wilson-Patten on the Future Establishment and Organisation of our Land Forces: Lieut.-General Sir Percy Douglas, Bart.—Report of the Cheltenham College Natural History Society for the year 1870.—Statistical Review of Ten Years of Disease in Manchester and Salford: Dr. A. Ransome.—Double Spectra: W. Marshall Watts —On the Spectra of Carbon: W. Marshall Watts —On the Reason why the Difference of Reading between a Thermometer exposed to Direct Sunshine and one Shaded Diminishes as we Ascend in the Atmosphere: James Croll.—An Address read at the Anniversary Meeting of the Entomological Society of London, January 23, 1871: Alfred R. Wallace.—On the Chemical Composition and Microscopic Constitution of certain Cornish Rocks: J. A. Phillips —Proceedings of the Somersetshire Archæological and Natural History Society for 1868-69.

American and Colonial —Report of the Present Condition of the Geological Survey of Victoria.—Reports of the Mining Surveyors and Registrars (Victoria) for the Quarter ending September 39, 1870.—Abstracts of English and Colonial Patents and Specifications relating to the Preservation of Food, &c.: W. H. Archer (Melbourne).—Patents and Patentees (Victoria), 1814-1865, Index to ditto for 1868 and 1869; Abstracts of Specifications of Patents applied for from 1854 to 1866, Ac-Bu (Victoria): W. H. Archer —Descriptions of new Fossil Shells of the Upper Amazon: T A. Conrad (from the *American Journal of Conchology*) —Report of Committee on New Remedies to the Muskingum County, Ohio, Medical Society for October, 1870 (Buffalo).

Foreign —Die Geschichte der Forschungen über die Phosphorite des mittlern Russlands von W. v. Gutzeit (Riga).—Rend'conti del reale istituto lombardo, Ser. ii vol. 3, fasc 19, 20, and vol. 4, fasc 1.—Correspondenzblatt der Naturforscher-vereins zu Riga.

DIARY

THURSDAY, February 16.

Royal Society, at 8.30 —On some of the more important Physiological Changes induced in the Human Economy by Change of Climate, as from Temperate to Tropical, and the Reverse (concluded): Dr. Rattray, R.N. —On a Registering Spectroscope: Dr Huggins, F.R.S.

Society of Antiquaries, at 8 30 —On the Topography of Jerusalem, with special reference to the results obtained by the Palestine Excavation Fund and the Site of the Temple of Antonia and of the Acra: Thomas Lewin, M.A, F.S.A.

Linnean Society, at 8.—On Tremellineous Fungi and their Analogues: L. R. and C. Tulasne.—Bryological Remarks: S. O. Lindberg, M.D.

Chemical Society, at 8.

Royal Institution, at 3 —Davy's Discoveries: Dr. Odling.

FRIDAY, February 17.

Royal Institution, at 9.—On the Wolf-Rock Lighthouse: James N. Douglass

Geological Society, at 1.—Anniversary Meeting.

Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, February 18.

Royal Institution, at 3.—Socrates: Prof. Jowett.

SUNDAY, February 19.

Sunday Lecture Society, at 3.30.—On the Religion of Health: Dr. Elizabeth Blackwell.

MONDAY, February 20

Entomological Society, at 7.—On the Dispersal of Non-migratory Ins by Atmospheric Agencies: Mr. Müller.

Victoria Institute, at 8 —Phyllotaxis; or the Arrangement of Leaves according to Mathematical Law: Prof. Henslow.

Royal College of Surgeons—On the Teeth of Mammalia: Prof. Flower.

London Institution, at 4 —On the First Principles of Biology: Prof. Huxley. (Educational Course.)

Royal United Service Institution, at 8.30.—On the Turret Ships now building for Her Majesty's Navy: E. J. Reed, C.B.

TUESDAY, February 21.

Zoological Society, at 9.—Note on the *Tænia* from the Rhinoceros: Dr. W. Peters, F.M.Z S.—Remarks on certain species of Abyssinian Birds: J. H. Gurney.—On certain Indian Reptiles: Dr. J. Anderson

Statistical Society, at 7.45.—On Currency and Pauperism: Mr. Ernest Seyd.

Royal Institution, at 3.—Nutrition of Animals: Dr. Foster.

WEDNESDAY, February 22.

Society of Arts, at 8.—On Water Meters: F. E. Bodkin.

Geological Society, at 8.

Royal College of Surgeons—On the Teeth of Mammalia: Prof Flower.

Royal United Service Institution, at 8.30 —The Organisation of our Military Forces: Lieut.-Colonel Arthur Leahy, R.E. (Adjourned Discussion.)

THURSDAY, February 23

Royal Society, at 8 30.

Society of Antiquaries, at 8.30.

Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

London Institution, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff, M.A., F.C.S.

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THURSDAY, FEBRUARY 23, 1871

THE MEDITERRANEAN ECLIPSE, 1870

II.

IN my former article under the above title, written from Venice, I gave as shortly as I could the conclusions at which I had arrived as to the results of the various Eclipse expeditions as gathered from the very imperfect information then at my disposal. Since I returned home, I have naturally become possessed of more facts, though even yet the time has not arrived for discussing all the observations as they must be discussed before an absolutely final verdict can be given.

Still, there is so much general interest taken in the recent work, that I venture to return to it at the present time, more especially as I can now print a letter from a distinguished American astronomer, giving his view of the work done, and also as I am anxious to refer to Prof. Young's article which has recently appeared in NATURE.

Prof. Peters, whose long and laborious researches on the sun are well known to all of us, thus writes in reference to my former article:—

> "Its perusal has been to me a source not only of pleasure but of much instruction. You have placed on record, with great lucidity, the question as it stood before the Eclipse, and the points to be examined by the various ways of observation for bringing the question nearer to its solution. Although the unfavourable state of the weather over the entire zone of totality, as it seems, from Spain to Sicily, has greatly obstructed the execution of the plans and the extensive preparations made with the liberal aid of our respective governments; and although hitherto, of course, only imperfect, mostly verbal, information has reached us of what the parties really did succeed in obtaining—still the result that is to be drawn from the sum total, as you are showing, seems of importance. The spectroscopic, polariscopic, and telescopic observations altogether agree in demonstrating an interior portion of the corona to belong to the sun. The existence of such a *solar* stratum is sustained also by my researches on the motion of spots when near the limb, pointing to a refraction on, or rather above, the sun's surface. I concur further in your opinion that the outer, more irregular radiating portion of the corona very likely owes its origin to our atmosphere. It is highly to be regretted that our Etna parties, in elevations respectively of 3,100, 5,500 and 8,000 feet, suffered disappointment from a heavy cloud at the critical moment of totality. Their observations would have been decisive as to the local and atmospheric cause of the radiating coronal phenomenon."

One more extract before I proceed. With reference to the suggestion (based on my observations of injections into the chromosphere) contained in my article, that probably the green line seen in the spectrum of the Corona might indicate a new element lighter than hydrogen, Prof. Young, claiming priority in the suggestion, writes:—

> "In *Silliman's Journal*, November 1869, I wrote, 'should it turn out that this line in the spectrum of the aurora does actually coincide with 1474, it will be of interest to inquire whether we are to admit the presence of *iron vapour* in and above our atmosphere, or whether in the spectrum of iron this line owes its origin to some foreign substance, probably some occluded gas as yet unknown, and perhaps standing in relation to the magnetic powers of that metal.'
>
> "This is the only reference I am able to make here. In my paper published in the Proceedings of the American Association for 1869, the same thing is, I think, more forcibly expressed. I think you will find it also in my Eclipse Report in the 'Journal of the Franklin Institute' (and in my letter to NATURE last spring).
>
> "The idea that 1474 might represent some new element occurred to me at once when I found it in the Corona, but of late I own I have more inclined to the opinion that it might possibly be a true iron line, and caused by meteoric iron dust of almost infinitesimal fineness; yet I have always felt the difficulty of supposing the complicated iron spectrum reducible to this one line."

I feel it due to Prof. Young to give this extract, though I confess I do not see that the suggestions are similar, nor do I see anything similar in the letter referred to, though I have lighted upon this passage which I had forgotten, which shows the great advance that has been made. Prof. Young last year wrote* "It is not impossible that the so-called corona may be complex. Some portion of its radiance may, *perhaps*, originate in our own atmosphere, although I do not yet find myself able to accord with the conclusions of Dr. Gould and Mr. Lockyer in this respect, and am strongly disposed to believe that the whole phenomenon is purely solar." His present views were given to the readers of NATURE three weeks ago, as in the main concurring with my own.

With reference to Prof. Young's article, I am anxious to say one word on the "sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality," witnessed by himself and Mr. Pye. I have seen this *once*, and only once, during all my observations, and Professor Young (who enjoys better atmospheric conditions than I do) has never seen it when working with the new method. Now, I hold that the new method is competent to pick up such an envelope as the one referred to by Mr. Longley, if it can pick up an uprush similarly composed; and although of course the vapours competent to give such lines are not far off, as the ordinary observations prove, I do not think they are ordinarily high enough above the level of the photosphere to be seen in this manner. That the number of lines is largely increased when the atmospheric glare is withdrawn, was proved during the American Eclipse.

But to return to the Corona, the main point of attack during the last Eclipse. Since my last article was written I have had an opportunity of inspecting copies of the beautiful photographs taken by Mr. Brothers at Syracuse, and also one of the photographs taken by the Americans in Spain. These, compared with the sketches taken at the respective stations, are very curious. In the Spanish photograph there is a very distinct "rift," or dark space in the coronal region, extending, I believe, almost to the sun, and fainter indications of two other such rifts in another region, not extending so low down in the Corona. So far as the facts have yet been before me, only one of these rifts was sketched. Now, at Syracuse Mr. Brothers also photographed rifts—three rifts; but the sketches did not record a single one. In Prof. Watson's drawing, a copy of which I have now in my possession, there is no indication whatever of them. But there is a much more important fact behind. Of course, if these rifts had been in the same positions in the two photographs, taken at stations so wide apart as Spain and Sicily, the presumptive evidence in favour of the solar nature of the Corona for a distance outside the sun equal to its diameter would have been overwhelming, and feeling that here was a crucial test to apply to a question which has so long been debated, but never with such interest among the workers as recently, it was with some excitement that I found myself before these two photographs some little time ago with two American astronomers of eminence, for the purpose of endeavouring to settle the question. Suffice it to say that we came to the conclusion that the rifts were not identical, that the two cameras had *not* photographed the same phenomenon, although at first there appeared to be sufficient similarity to make the matter appear doubtful, and, unfortunately, the photographs vary so much in size, and the margin of the American one is so limited, that it will be scarcely possible to make a final comparison until they are brought to a common scale, and superposed the one on the other. I do not think it is surprising that rifts should appear in both photographs, supposing a non-solar cause were at work, for the Corona between the rifts on Mr. Brothers' photograph looks like a very wide ray.

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* NATURE, vol. i p. 533

Assuming then for the present that the photographic evidence goes the way of all the other evidence—that in short, the solar corona, including all its fantastic boundaries, has been probably reduced from one, two, or three solar diameters, to six, eight, or ten minutes,—I care not which,*—let us examine some of the details of the various observations.

In Professor Watson's drawing, the intimate connection between the higher and lower levels of the chromosphere (including the portions not at present observed by the new method), comes out in a very striking way. Mr. Seabroke, at my request, made careful maps of the positions of the prominences before the totality commenced, and Professor Watson made his drawing of the Corona, independently of the positions of the prominences. On the homeward journey the map was compared with the sketch, and to use Professor Roscoe's words, "On comparing the two drawings thus independently made, a most interesting series of coincidences presented themselves. Wherever on the solar disc a large group of prominences was seen in Mr. Seabroke's map, there a corresponding bulging out of the Corona was chronicled on Professor Watson's drawing, and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the sun's limb." We may remark that these coincidences show the excessive fidelity of the drawing, and make it one of the most valuable of the products of the Expedition.

On former occasions the Corona has been stated to assume a roughly four-cornered form. This was also observed in Spain last December, and seems at last explained by three drawings made by one of the American party there.

At the commencement and end of totality, when the moon unequally covered the sun, the photographs have recorded an excess of light on the Corona on the side where the limbs occur nearest in contact. I am told that this effect in one of Lord Lindsay's photographs is very striking; it is certainly so in one of Mr. Brothers'. In the drawings we have a slightly different effect. At the commencement of totality, when the western or right hand limbs were in contact, we get Fig. 1, at the end of totality the appearance recorded was Fig. 2; the picture at the middle of totality compounding both these appearances, and being roughly represented by Fig. 3, in which the rectangular appearance comes out in its full strength.

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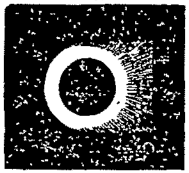

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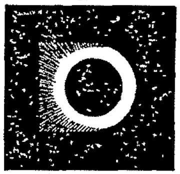

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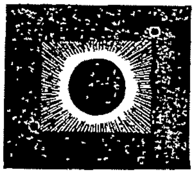

A word now about the polariscopic observations. I may remark on this that it is much more easy for us to explain slight polarisation which might be atmospheric, than it is to explain, if we content ourselves with laboratory experiments, strong *radial* polarisation which must take place at the sun. If we assume that gas or vapour of considerable tenuity does not reflect light (although I think this is to assume very much for the gas or vapour *at the sun*, at all events), what is it that reflects light to us at the sun, and reflects it apparently only *above* the level of the intensely incandescent hydrogen? Certainly not solar spray. If we deny reflection to gases altogether, may it not be the continuous portion of the spectrum of the gas itself to which the light is due. But this question of polarisation is certainly one in which very much remains to be done, and it is consoling to know that the results obtained now will much facilitate the planning of the next polariscopic campaign, which, we may add, should not be deferred beyond the end of this year.

J. NORMAN LOCKYER

PHYSICAL LABORATORIES

IN an excellent article in a late number of NATURE, Prof. Pickering has drawn attention to the importance of the practical teaching of Physics, and has shown how this is being done on an extensive scale in America. It may be interesting to trace the similarity between the methods employed by different teachers, and to show what opportunities are and have for some time past been open to students in London for the practical study of every branch of Physics.

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* I beg here to give the actual words employed by Dr Frankland and myself in the communication to the Royal Society on the subject. Speaking of the chromosphere, it was remarked "the tenuity of this incandescent atmosphere is such that it is extremely improbable that *any extensive atmosphere*, such as the corona has been imagined to indicate, lies outside it"—Proc. R.S., Feb. 11, 1869 I never imagined that all the Corona was non-solar. Again, Proc. R.S., No 116, 1870, discussing the American Eclipse, I state that the chromosphere includes the "radiance" observed in the American Eclipse, of which radiance Dr. Gould wrote as follows:—

"An examination of the beautiful photographs made at Burlington and Ottumwa . . . and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the moon in the pictures made during totality is not the corona at all, but is actually the image of what Lockyer has called the chromosphere. This interesting fact is indicated by many different considerations. The directions of maximum radiance do not coincide with those of the great beams of the corona; they remain constant, while the latter were variable There is a diameter approximately corresponding to the solar axis, near the extremities of which the radiance upon the photographs is a minimum, whereas the coronal beams in these directions were especially marked during a great part of the total obscuration. The coronal beams stood in no apparent relation to the protuberances, whereas the aureole seen upon the photographs is most marked in their immediate vicinity. . . . . Whatever of this aureole is shown upon the photographs was occulted or displayed by the lunar motion, precisely as the protuberances were The variations in the form of the corona, on the other hand, did not seem to be dependent in any degree upon the moon's motion. The singular and elegant structural indication in the special aggregations of light on the eastern side may be of high value in guiding to a further knowledge of the chromosphere. They are manifest in all the photographs by your parties which I have seen, but are especially marked in those of shortest exposure, such as the first one at Ottumwa. In some of the later views they may be detected on the other side of the sun, though less distinct; but the very irregular and jagged outline of the chromosphere, as described by Janssen and Lockyer, is exhibited in perfection."

Professors of Physics at different Universities have usually selected their best students to assist them in their private laboratories, to the mutual advantage of professor and student, but I believe that Prof. Clifton was the first to propose, more than three years ago, that a course of training in a physical laboratory should form a part of the regular work of every student of Physics.

This system was adopted and at once put in action at King's College, on a very considerable scale for a college with no endowment whatever, and has been working for now nearly three years. Two large rooms adjoining the Museum of Physical Apparatus were fitted up for a Physical Laboratory, and a third room was built for a store and battery room. Fixed tables in both large rooms are supplied with water and gas, and with pipes passing to gasholders containing oxygen and hydrogen, also with thick copper wires insulated from one another passing to the battery room, so that in electrical work the fumes from batteries are entirely got rid of.

The principal instruments have their fixed places on the tables, and a description of the measurement to be made is given to each student, and while in progress his work is examined by the professor or demonstrator. The course of study includes the subjects of pneumatics, heat, light, electricity, and magnetism, and with the regular class, a definite order in each subject is kept to as nearly as possible. When, as has sometimes been the case, there are twelve or more students beginning their laboratory course at the same time, it is necessary to deviate from the regular course, and to set some to begin with heat, some with light, and others with electricity. For some experiments, such as the determination of the relation between the pressure and volume of a gas, or the measurement of the expansion of a gas for given changes of temperature, requiring the use of the manometer and cathetometer, it is found better to have two students working together, each student making in his turn and so checking every part of the measurement or determination.

The accuracy of the results obtained has been very great, and is an evidence of the interest taken in the work by the student, and of the value of such a course of study as a mental training, to say nothing of the actual knowledge gained. Every student is required to produce fair results, and to give an account of the methods which he has employed, before he is allowed to proceed to another part of the subject. Besides the students pursuing the regular course there are several who wish to devote their attention to some one branch, such as Electricity. In this subject, after making determinations of Resistance, Strength of current, and Electromotive force with simple galvanometers, they pass to more delicate measurements with Thomson's Galvanometers and Electrometers, such as the experimental determination of equi-potential lines on a conducting surface uniting two poles of a battery, and perform all the tests and measurements required in connection with Telegraph lines and cables.

The more advanced students carry on investigations in the Laboratory, such as the measurement of the effect of heat in altering the magnetic polarity of dia-magnetic bodies, or in altering the rotation of the plane of polarisation of a beam of polarised light as it passes through sugar solutions.

Students are encouraged to combine their work in the Physical Laboratory with their work in the Mechanical Workshop, and are enabled to design and construct apparatus, and their inventive powers are exercised often with great success. From experiments with Attwood's Machine one student has designed and made for himself a new form of governor for an engine, another has designed an Inductometer for measuring the time required to produce the maximum induced current in a wire by the action of another current.

The success of the Laboratory system of teaching may be judged from the quality of the work done by the students, who are mostly from sixteen to eighteen years of age, and are always eager for the work, as well as from the fact that in the last term there were twenty-three students in the Laboratory, and the numbers are steadily increasing.

It will be seen that to the student of Electricity or any branch of Physics, every opportunity is offered of pursuing the object which lies before him, and that the advance which may be made by him is dependent only on his own exertions.

W. G. ADAMS

MORELL'S GEOMETRY

The Essentials of Geometry, Plane and Solid, as taught in French and German Schools, with shorter Demonstrations than in Euclid; adapted for Students preparing for Examination, Cadets in Military and Naval Schools, Technical Classes, &c. By J. R. Morell, formerly one of Her Majesty's Inspectors of Schools. (London: Griffith and Farran, 1871.)

A WORK with this attractive and somewhat ambitious title cannot fail to attract attention, appearing, as it does, at a moment when very commendable efforts are being made to improve the teaching of Geometry in our schools, and to prepare the public mind for an important reform which will necessarily involve the adoption of text-books more suited to modern habits of thought and inquiry than the Elements of Euclid. It will, no doubt, therefore be expectantly read by many, and the fact that the author is already widely known as a writer on philosophy and grammar, and was formerly one of Her Majesty's Inspectors of Schools, will tend to raise expectations, and will lend it an authority to which, as we shall presently see, it is by no means entitled. Our duty to students preparing for examination compels us, in fact, to warn them that this book can render them no essential service whatever, but, on the contrary, may do them incalculable mischief; and our sympathy with the praiseworthy efforts above alluded to—our desire to prevent undeserved discredit from being attached to those efforts—obliges us to dissociate this one from them, and to criticise it with all due severity.

The plan and general arrangement of the book are open to the severest criticism; we deem it unnecessary, however, to dwell thereon, for the work is so destitute of the most essential of all "Essentials of Geometry"—accuracy and clearness—that no possible rearrangement of its materials could redeem it. We shall consequently draw attention solely to the deplorable looseness of expression, indicative of hopeless inaccuracy of thought with which almost every page is disfigured.

We shall commence with quoting a few definitions, and simply italicising their most salient incongruities. Further comment will rarely be needed.

"Extension is the *space* occupied by a body" (p. 1).

"A straight line is that which has all its *points* in the same *direction*" (p. 1). Direction is, of course, not defined, nor is it stated whence the points of the line are supposed to be viewed.

"A plane angle is the *greater* or *less* inclination of two straight lines *to a common point*" (p. 5). In a note hereto the author complacently observes that "our definition accords with Euclid, Bk. i., n. 8."

"The angle A O B increases continually *in proportion* as the straight line O B takes the direction O C, O D, &c." (p. 6).

"The name adjacent angles is given to those angles that have one side common" (p. 6). The author appears to have felt that there was some insufficiency here, but instead of expunging the passage as wholly useless, he tries again in small print, and almost, though not quite, succeeds. That some haziness still clings around his conception of adjacent angles is obvious; for when we come, in the Geometry of Space, to angles between two planes, dihedral angles, we are told (p. 90) that "adjacent dihedral angles are those which have a common plane, *and whose other two sides are in one plane*." The condition here italicised is wholly unessential, and the really essential one is again omitted; viz., that the two angles should be on opposite sides of their common plane. This misconception of the nature of adjacent dihedral angles leads the author, naturally, to the following absolutely false definition:—"If two adjacent dihedral angles are equal, each one is named a dihedral right angle." We may here observe that there is deplorable confusion in this part of the work between polyhedra and polyhedral angles. Trihedral angles are generally, though not always, termed *trihedra*, dihedral angles occasionally crop up as *dihedra*, and uncouth entities such as *polyhedrals*, *dihedral sides* of a polyhedral angle (p. 94), and *polyhedral triangles* (p. 95), not unfrequently stop the way.

Leaving definitions, however, let us glance at Mr. Morell's enunciations of theorems, and the "improved shorter demonstrations" of them with which he supplies us.

On p. 3 we find the following short paragraph, into which a theorem and its demonstration are supposed to be condensed:—"Any diameter whatever divides a circumference into two equal parts; *therefore* if on superposing its two halves they did not agree, the radii of the same circumference would be unequal, which is absurd." The word *therefore* would imply that the assertion which precedes it is not itself the theorem to be demonstrated, but one which is to be employed in demonstrating something else. But what is this something else? Long reflection failed to furnish any answer to the question; it seemed but to give to the last three words a more extended significance than the author could have contemplated.

The entire paragraph remained a mystery to us, in fact, until we reached p. 25, when Mr. Morell's happy arrangement enabled him to return to the subject in these slightly modified words:—"Every diameter divides the circumference into two equal parts; *for* if on placing one *half* on the other they did not coincide, the radii of one and the same circumference would be different in length, which is absurd." Here the word *for* being substituted for *therefore*, we can no longer doubt that what precedes it *is* the theorem to be proved; the moment one *part*, however, is termed a *half* the whole question is begged; and even if this gross blunder had been avoided, the demonstration would still have been worthless, so long as the *mode* of placing one part on the other was left wholly unexplained.

The enunciation and demonstration of the two fundamental theorems of parallels are thus given on p. 21:—

"If two straight lines are parallel, and are cut by a secant, the alternate angles are equal, and also the corresponding angles. Conversely, if two straight lines cut by another form equal alternate or corresponding angles, the said lines will be parallel."

"For two alternate internal, or alternate external, or corresponding angles are both either acute or obtuse, and *consequently* equal in the case of parallels. But, if of two internal or external angles on the same side, one is acute, and the other obtuse, these angles are, *therefore*, supplementary."

Let the reader picture to himself for a moment the perplexity of the misguided student, "preparing for his examination," as he vainly strives to extract a meaning from this sheer nonsense. Distrustful of his own powers, the poor fellow will probably attribute his failure to his own incapacity, and in despair commit the precious passage to memory for the purpose of reproduction when his hour of trial shall come. The consequences of such rashness need not be stated. And yet Mr. Morell cites a respectable French geometer as an authority for this "shorter demonstration!" It was with no small curiosity that we turned to the pages of Amiot's *Éléments de Géometrie* to see how an author, who usually writes with admirable clearness, though not always with desirable rigour, could have been made responsible for such absurdity. The process was simpler than we expected; editorial scissors had simply clipped away everything worthy of the name of demonstration, and the editorial pen had garbled the feeble residue into the chaotic sentences above reproduced. We know of no epithet too severe to apply to editorial transgression of the kind here exposed.

To proceed with the painful task we have imposed upon ourselves, we have next to draw attention to two blunders, not in geometry, but in simple logic. On p. 20 we read as follows:—

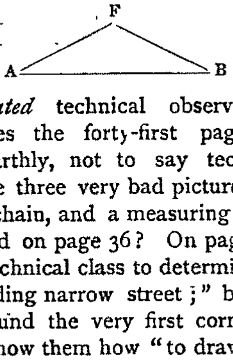
"It is often assumed as a self-evident proposition that through a given point only one parallel can be drawn to a straight line," and, in a foot-note hereto, we are told that "this is the *opposite* of Euclid's 12th axiom, similarly assumed to base upon it his theory of parallels." Had Mr. Morell read Amiot, even, with anything like attention he would have seen that this is not the case. In forming the opposite of a theorem, the new hypothesis and predicate must be made to contradict, respectively, the original hypothesis and predicate. The opposite of Euclid's 12th axiom, therefore, as Mr. Morell may now convince himself by trial, is very different from the one above quoted.

On p. 44 again, we have this marvellously short enunciation and demonstration of Euclid's Prop. 8, Book I.;
"Let A B C, D E F be two triangles, having A B = DE,

A C = D F, and B C = E F. Then angle A = D; *for if they were unequal, sides B C and E F would be unequal. Therefore* A = D." In violation of the most obvious of all logical rules, Mr. Morell here professes to prove a theorem by the simple assertion of it in its contra-positive form! To assert that B C and E F would be unequal if A and D were unequal is *logically equivalent* to saying that A and D would be equal if B C and E F were equal, which is precisely the proposition to be proved. The truth of either of these statements involves that of the other by the necessary laws of thought, and neither can be said to require geometrical demonstration more than the other. It is true that Euclid himself (*vide* Props. 7 and 9, Book iii.) overlooked this necessary relation between contra-positive theorems. But Euclid's error was very trivial in comparison with Mr. Morell's. Euclid proved *both* theorems geometrically when a proof of one would have sufficed, but Mr. Morell unwarrantably *asserts* the truth of one, and imagines he has thereby proved the other.

Notwithstanding the fact that Mr. Morell devotes a considerable part of his book to the approximate calculation of the ratio of the circumference of a circle to its diameter, we fear that he must henceforth be classed amongst the "circle squarers of the period." For, as is well known, the solution of this vexed problem is at once reducible to that of the rectification of the circumference; and on page 62 we find it stated that "the graphic construction which *gives us the length of the circumference in a straight line* is the following." For obvious reasons we forbear to reproduce it.

The "technical classes" to whom Mr. Morell professes to have "adapted" his text-book, will, we fear, derive as little profit from its pages as the "students preparing for examination." How can it profit them, for instance, to be told that "many public buildings have as façade a front elevation like A F B?" Yet it is



with this *isolated* technical observation that Mr. Morell terminates the forty-first page of his book. Again, what earthly, not to say technical use can they make of the three very bad pictures of a surveying rod, a Gunter's chain, and a measuring tape, with which they are favoured on page 36? On page 37, Mr. Morell starts with his technical class to determine a straight line through "a winding narrow street;" but he fails utterly to bring them round the very first corner. On page 38 he proposes to show them how "to draw *a* perpendicular bisector of a given straight line in nature," but we will not disfigure this page of NATURE with a reproduction of his unintelligible method. Although readers might be amused by it, we are not in a humour to extract amusement from a work which was certainly not written for the purpose of providing it, but with the serious intention of teaching Geometry in an improved manner.

We have by no means exhausted the incongruities of the book; but we have, we think, sufficiently shown that the teaching of Geometry cannot but be vitiated by its use; that, in fact, this text-book of Her Majesty's ex-Inspector of Schools will itself bear no inspection whatever.

RODWELL'S DICTIONARY OF SCIENCE

A Dictionary of Science: comprising Astronomy, Chemistry, Dynamics, Electricity, Heat, Hydrodynamics, Hydrostatics, Light, Magnetism, Mechanics, Meteorology, Pneumatics, Sound, and Statics. Edited by G. F. Rodwell, F.R.A.S., F.C.S. (London: E. Moxon and Co., 1871.)

THERE are Dictionaries and Dictionaries. We have had occasion to expose the shortcomings of some books that are called by this name; it is a far pleasanter task to direct attention to the merits of a work like the one before us, which really deserves its title. Mr. Rodwell's "Dictionary of Science" is a repertory of facts connected with physical science, which will be invaluable to the student. From Chemistry to Chladni's Figures; from Thermo-dynamics to Turacine, scarcely a term will be met with in scientific works, of which the learner will not here find an explanation. The articles have the great advantage of being short, and presenting the salient points of each subject at a glance before the reader's eye; and that their scientific accuracy may be relied on, is guaranteed by such names (amongst others) as those of Mr. Crookes, Prof. Guthrie, and Mr. Wormell in the list of contributors. To illustrate the style of the book, we cannot do better than select two of the shorter articles. The first is on a subject which has recently been discussed in these columns:—

"*Mass.*—Mass is a term for the quantity of matter in a body. In order to measure mass, we assume that the attraction of the earth on all particles of matter is the same, and is not dependent on the nature of the matter attracted. This assumption seems to be justified by the fact that bodies of all kinds fall with equal velocity in the exhausted receiver of an air-pump. Hence we measure the mass of a body by its weight, and can only define the mass as a quantity proportional to the weight. If, then, at the same spot of the earth's surface, one body is twice as heavy as another, the mass of the first is twice that of the second. Suppose, however, that the body be weighed by a spring-balance at a certain place, and weighed again by the same instrument at another place nearer the equator, it will be found that the body is lighter at the latter place. It is found also that the acceleration due to the attraction of the earth is also less at the second place than at the first, in the same proportion. This illustrates the fact that when the mass remains the same, the weight varies as the acceleration of gravity. Hence the weight varies as the product of the mass and the acceleration of gravity, and, consequently, when suitable units are chosen, the mass of a body is equal to its weight divided by the acceleration due to gravity."

Our next extract is chemical:—

"*Alcohol.*—By this name, when standing by itself, is usually understood the second term of the series of ordinary alcohols, or vinic alcohol. It is a transparent, colourless, mobile liquid, of a specific gravity 0·7939 at 60° F.; it boils at 74·4°C. (173·1 F.); its vapour density is 1·613; its formula is C_2H_6O; it is the spirituous principle of wine, beer, and spirits, and is produced by the fermentation of sugar, which is split up into alcohol and carbonic acid. In the diluted state alcohol is sometimes called spirits of wine. It is difficult to render anhydrous; distillation alone will not produce an alcohol containing less than 9 per cent. of water, and this remaining quantity must be removed by adding something which unites with the water chemically, as quick-lime. By oxidation it is converted into aldehyde, and then into acetic acid, but other products of oxidation are obtained in less quantity; these are formic acid, acetal, acetic ether, saccharic acid, gloxal, glyoxilic acid, and glycolic acid, the final products being water and carbonic acid. When the elements of water are removed from absolute alcohol, ether is formed."

Now for a few words of criticism, or rather of suggestion. The book is not quite free from mistakes, probably in most cases printers' errors, which ought by all means to be avoided in a Dictionary, and will doubtless be corrected when a second edition is called for, which we hope may be very soon. But what we find most fault with is the cross-references, which are needlessly complicated, and often misleading. Being anxious to see what was said about the latest discoveries in Spectrum Analysis, we turned to the article under that heading, which we found to be very clear as far as it went. At the end we were referred "for further information" to five other articles; of these, "Spectra of the Elements" and "Metallic Spectra" are not to be found; "Spectra, Metallic," does occur, but simply refers us again to "Coloured Flames," under which we found only two lines relating to spectroscopy. Neither of the articles referred to gave us any further information of importance on the subject.

One other criticism on another sentence in the preface. We observe with pleasure that this is to be the first of a series of similar dictionaries, which shall embrace the other departments of Science. One, it is stated, will have for its subject the "classificatory sciences" of Botany and Zoology. Now we must protest against the use of this term as applied to the two sciences named. Zoology and Botany have their physiological as well as their systematic side, and far the more important of the two. We trust that the forthcoming dictionaries will be framed on no such narrow basis as that implied in the denunciations by Mr. Emerson and Mr. Ruskin of the pursuit of botany as "a mere science of names." When these volumes are published, some confusion may possibly arise from the title "Dictionary of Science" having been given to this work, when it should have been more correctly "Dictionary of Physical Science."

The book is however an indispensable addition to the library of every student, and we cordially recommend it to the notice of our readers.

A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Ocean Currents

I do not altogether agree with Mr. Johnston in concluding that his suggestion as to the influence of barometric differences on Ocean Currents was stated with insufficient clearness. His words were:—"The waters which lie under the high pressure area have a tendency to escape from under the excessive weight, towards the space over which the pressure is less;"—and about the meaning of this sentence there can be no dispute. It seems to me rather that Mr. Johnston, in his letter in NATURE for Feb. 2, is changing his ground; and whilst he formerly distinctly suggested the probability of the currents being due to mean differences of pressure stationary over certain oceanic areas, he now wishes to attribute them to the *motion* of centres of low pressure. This is quite a different thing, and is so far admissible, that what Piddington aptly called "storm-currents," attendant on tropical cyclones, are undoubtedly caused in such a way. But these storm-currents are very exceptional; within the tropics, where their excessive development renders them most noticeable, they occur only in the hurricane months, and even then only at intervals; they have clearly no connection with the continuous current which flows west all through the year, and besides the great depressions of the barometer which give rise to these, there are no others. The barometer in tropical latitudes is remarkably constant, and shows no centres of low pressure passing continually towards the west. in the direction of the Trade-winds or the Equatorial Currents. The Equatorial Currents are thus clearly not due in the slightest degree to differences of barometric pressure, either to those that are stationary, as Mr. Johnston now seems to admit, or to moving ones which exist only at rare intervals.

It is however certain that centres of low pressure, round which the air circles, do very frequently pass along in temperate latitudes, driven (it seems to me) by the prevalent west wind. In our own latitudes. and more especially in the winter months, these succeed each other at intervals of a few days, and though their action is intermittent, and for the most part peculiar to winter, I see no reason to doubt that they carry with them a species of storm-current, which does occasionally modify and even intensify the prevailing easterly drift. But it is in the highest degree improbable that the *formation* of these centres of low pressure can give rise to any appreciable currents. Even towards the centre of a cyclone, the barometric gradient does not exceed one inch in fifty miles; which, so far as its effect on the ocean is concerned, is equivalent to a difference of water-level of one-fourth of an inch in a mile; and even this can only exist if we suppose the barometric depression to be formed almost instantaneously. There is no evidence that it is so formed; and the longer it takes to arrive at its maximum, the more gradually does the water rise into the central space, and the more infinitesimal is the velocity with which it does so.

Mr. Johnston rightly corrects the slip which appeared in my former letter, of twenty miles *an hour*, instead of *a day*, and thinks that even twenty miles a day is too large an estimate of the velocity of the equatorial current of the North Atlantic; but the Admiralty chart to which he refers shows many instances of a velocity still greater; and whatever force produces the current must clearly be adequate to the production of the greatest velocity it attains. This, however, is of no consequence to my argument. The barometer decreases very steadily, regularly, and gradually, from the patch of permanent high pressure to the line; and the effect of the formation of this patch would therefore be equivalent—as I said before—to a difference of water-level of about four inches in 1,800 miles; a difference which could no more generate a current of ten miles a day than it could one of twenty; but as whatever effect it was capable of producing was produced, once for all, many ages ago, the consideration of it has no direct bearing on the currents of the present day.

I would, therefore, repeat that neither as permanently existing nor as changing with the seasons, neither in their continuance, nor in their formation, nor in their fluctuations, can the areas of high and low pressure, which Mr. Buchan has delineated, and which formed the subject of Mr. Johnston's first letter, produce an appreciable effect on the Ocean Currents; and that, since the Equatorial Currents in the several oceans follow the course of the Trade-winds, and are not affected by transitory differences of pressure which do not exist in the Trade-wind region, the movement of these differences of pressure where they do exist, cannot be considered as causing the current, or as necessary to its flow, though it may occasionally give it an increased velocity. But any effect so produced is due not to the mere existence or formation of a centre of low pressure, but to its *onward movement*; and to urge that this onward movement exercises an influence on the currents, is merely to urge, with respect to one not very important detail, the application of that principle which, in its widest sense, I have already maintained through a long chapter in my Physical Geography, and which Mr. Croll is now maintaining in the *Philosophical Magazine*—that the Ocean Currents are due entirely to the action of wind.

J. K. LAUGHTON

Royal Naval College, Portsmouth, Feb. 5

IN reference to the exchange current found to exist between the Mediterranean and Atlantic, at the Straits of Gibraltar, it seems a matter for inquiry how far the rise of the ocean bed, just outside the Straits, is natural; and how far it may have been formed or increased by deposits left during the successive ages, by this undercurrent of out-set-water, from the Mediterranean Sea into the Atlantic Ocean.

A. H.

Jan. 19

The Frost

Having seen notices in your journal about the severity of the late frost, I beg to state that its duration and severity have been most remarkable here, and unequalled, as far as my knowledge extends, for many years back. My instruments are standard ones, which have been recently compared at Kew, and placed in my garden quite detached from buildings, and facing the N.E. at 4ft. elevation. I append a table of the observations, which may be interesting to some of your readers:—

Dec. 1870.	Shade max.	Shade min.	Exposed min.	Jan. 1871.	Shade. max.	Shade. min.	Exposed. min.
20	47·8	30	30	1	27·5	19·8	18·2
21	39	22·8	22	2	26	24	24
22	26·8	15	15	3	33	21·5	21·5
23	29	1·5	0	4	31	14	13
24	24	−2	−3·5	5	39	23	23
25	28	+3	+3	6	42·2	31	31
26	33	24	23	7	45	30	29·5
27	31	24	23·5	8	40	28·3	28·3
28	32·2	27	27	9	40	25·3	25
29	32·8	10·5	10	10	40	28	28
30	31·6	15	14·5	11	36	30·5	29
31	32·8	15·2	14·3	12	35·8	20·5	20
				13	35·6	26·5	26

The frost was succeeded by a heavy gale of wind and a deluge of rain; in four days 2¼ inches fell, one inch being between 9 A.M. 17th, and 9 A.M. on the 18th. This, together with the melting of the snow, inundated the valley of the Medway round us for miles. The greatest cold I ever registered here was on January 4, 1867, being 5° below zero. The highest shade temperature I have recorded was 100°·5 on July 22, 1868, which was the hottest summer ever experienced.

Tunbridge

G. H. Fielding

Caves near St. Asaph

It will interest archæologists to know that new caves are being opened by Mr. Townshend Mainwaring in the neighbourhood of St. Asaph, and that already we have much additional evidence brought to light as to the early inhabitants of that part of the country. In one which appears to run downward into the cliff at Carregwen, near Galltfænan, remains of various animals have been found in brown cave-earth, among them one which has been determined to be that of a reindeer, by Mr. Dawkins, who is further of opinion that it has been gnawed by a wolf or hyæna. This is very interesting, as the cave is high up in the face of a precipice, and with the present physical geography the larger animals could not get into that cave except by being carried there; so that we have here either cave-earth containing remains of such a remote antiquity that the gorge below has been considerably altered since its accumulation, or we have the ancient abode of carnivorous beasts able to carry the large animals into their den.

In Brysgill, Mr. Mainwaring has met with greater success. From the rubbish and tumble under the rock shelter outside the mouth of a large cave, he has obtained a fine bone scraper ground to a sharp edge, several flint flakes and bones of man, horse, ox, sheep, hog, &c. Inside the cave, immediately under the recent mould, there is a broken stalagmite floor, associated with which were human bones and the flint flakes, and cores. At about two feet below the broken stalagmite floor, the bones of a horse were found in undisturbed brown earth. Here we have evidently the home of some of our troglodytic ancestors who manufactured their flakes in the cave from flint which they may have procured from the drift not far off.

This is only one of a number of most promising looking caves to which Mr. Dawkins some time ago called attention, and it is to be hoped that, with so many residents in the neighbourhood interested in scientific investigation, we may have them all systematically explored, and not lose any bit of important evidence from the want of observation at the time of discovery.

T. McK. Hughes

The Primary Colours

One more proof that violet is a primary. Place a hand prism between the eye and the sunlight so as to show the prismatic colours. Then hold a sheet of yellow glass between the prism and the light, and observe the result. The reds and yellows are scarcely altered, the greens are very greatly intensified, the blues and violets are altogether extinguished. If violet had really any red in it, the yellow glass, which does not stop the red rays, would change the violet to red, or would show at least some *trace* of red where the violet had been. Instead of this, the violet is totally stopped out, and the space which it occupied left dark. Wherever the secondary *pink* appears, this is changed to red by the stopping of the violet rays. The increased strength and brilliancy of the green shows clearly also the primary character of this colour. It is usually much weakened in the spectrum by mixture with the far-spreading violets; when this is removed it comes out in full splendour. I commend this little experiment to amateurs; it is simple and interesting. The same effect is produced by throwing the coloured spectrum on to a white wall, and holding the yellow glass between the prism and the wall.

Leicester, Feb. 20

Frederick T. Mott

Californian Oaks

In Nature, No. 68, p. 313, you did me the honour to quote a paper of mine in reference to the edible qualities of some of the Californian oak acorns. You will, however, allow me to state that, though this is true of some species, such as *Quercus lobata* Nee, which was the one I chiefly referred to in the passage quoted, yet that the acorns of others have a decidedly injurious effect, or are inedible. For instance, it is very commonly believed by the *rancheros* that the acorns of *Q. Kelloggii* Newb. give rise among pigs to a peculiar disease of the kidneys, while the acorns of a new species from Southern Oregon—which I shall shortly describe in a work now in the press—(*Q. echinoides* mihi) are so very bitter that no animal but the black bear (*Ursus Americanus*) will eat them, and it only when pressed by hunger. On the other hand the acorns of *Quercus Orstediana* (mihi), another as yet undescribed species, are so nutritious, that though the species never grows to a greater size than a small shrub, the produce of forty or fifty such bushes will fatten a hog. Again, there is a difference of quality among the edible species. The "digger" Indian, who is quite a *connoisseur* in acorns, makes a difference; for while the interior tribes prefer those of *Q. lobata*, those living near the coast chiefly affect *Q. sonomensis* Benth. Though pigs fatten freely on the acorns of *Quercus Garryana* Dougl., and in California on those of its ally, *Q. Douglasii* Hook., yet I never knew the Indians either in Vancouver Island or in California eat the acorns of either species, while those of *Q. agrifolia* Nee, *Q. chrysolepis* Liebm., *Q. densiflora* Hook. and Arn., *Q Sadleriana* mihi (nondescript species), &c., are not, so far as I am aware, eaten by any animal but squirrels. The fruit of *Castanopsis chrysophylla* Doug., a plant allied to the oaks and chestnuts, is, however, in great favour with the black bear. I have eaten the acorns cooked in the manner described in the extract, and—I suppose in common with other naturalists in the less explored parts of North-west America—have been forced by hunger to search for the acorns which *el carpentero* (*Melanerpes torquatus*) stores away for its use in the spongy bark of *Torreya*, *Sequoia*, *Pinus*, and various other trees, yet notwithstanding the sauce which famine gave to my appetite, I must confess that they were by no means palatable. This may, however, have been prejudice, for the Ancient Britons—who were by no means savages in the ordinary acceptation of the term—ate the acorns of *Quercus robur*, the common oak of this country. How they cooked them we are not informed. I presume, however, that it was not in so *recherché* a style as practised by some aboriginal friends of Mr. Paul Kane, the artist—a full description of which those curious in North American Indian *cuisine* will find in that gentleman's book descriptive of his journey across the American continent.

Robert Brown

Edinburgh, Feb. 20

THE ECLIPSE PHOTOGRAPHS

The accompanying woodcut is a copy of a drawing made from the negative No. 5, taken at Syracuse during the Eclipse of the sun on Dec. 22 last. When viewed by transmitted light, the negative shows chiefly the portions indicated by the unshaded parts, and the red

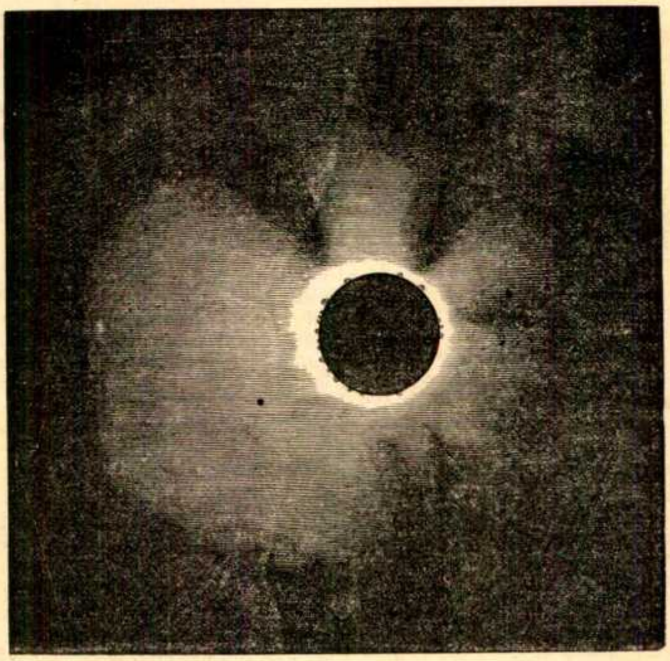
prominences; some parts of the first light shade can be seen, but the outer rays are altogether invisible. When, however, the plate is viewed by reflected light, the whole of the detail is distinctly seen. The negative was the last one taken; four others were exposed for the corona, but owing to the presence of cloud very little detail is visible.

It will be noticed that there is more of the corona shown on the west side of the moon than on the east, north, or south. This feature is shown on all the plates, so that there can be no question that there was more coronal light on the west side of the moon than at the other points. In explanation of the great display of the outer rays (I use the term *rays* for want of a better—perhaps *outer light* would be more correct, for there is no indication of lines or rays on any of the plates), I had supposed that the east side might have been partially covered with cloud; but in conversation with Prof. Eastman I found that he was observing for the reappearance of the sun, and he is quite certain that there was no cloud at the time the photograph was taken—that is, at about thirteen seconds from the end of totality. Mr. Fryer also is equally certain that there was no cloud. The plate was exposed eight seconds. It will be noticed also that the prominences are more numerous on the side where the corona is brightest.

Various opinions have been expressed as to the quality of the light of the corona. The effect we saw was that of moonlight, but not of the *full* moon, excepting the brilliant light close to the moon's limb, which is equal to the brightest moonlight, and I think its action on the sensitive plate confirms this opinion.

A point of much interest to be noticed is, that the light of the corona had been considered to be much less active than it really is; eight seconds were sufficient to produce on the plate an effect of light extending beyond the moon's limb, at least one and a half millions of miles.

I leave it to others to account for the cause of the great gaps or rifts in the corona; also their identity in position with those shown in the photograph taken by the American photographers at Cadiz. The identity of one of the rifts is absolutely fixed by the two prominences between which it appears in the photographs, and this one gives the relative places of the others.



THE LATE ECLIPSE, AS PHOTOGRAPHED AT SYRACUSE

When the two photographs are compared, there is an apparent difference in the places of the rifts with respect to their angular position on the moon's circumference. How this difference arises I am not prepared to say, as I have no information as to how the American picture was taken, and there is no mark on the transparency which has been lent to me by Prof. Young, to indicate the north point. In the engraving from my photograph the top is the north.

It is perhaps necessary to say that it is quite impossible to represent in an engraving on wood the delicate detail of the corona. The cut fairly gives the main features, but it is *hard* when compared with the original; the contrast should not be so great; the ground should not be perfectly black; and the effect should not be produced by *lines*. No woodcut has ever yet accurately represented the phenomena of the eclipsed sun.

When the photograph No. 5 is combined in the stereoscope with the one taken about one minute earlier, stereoscopic relief is produced—the corona is distinctly seen beyond the moon. It may be thought that this is merely the effect of contrast, but I believe it is really due to the change in the position of the moon. No such relief is seen when two copies of the same photograph are combined stereoscopically.

In order to see the woodcut with the best effect, it should be placed at a few feet distance from the observer, so as to lose all trace of the lines of the engraving; the effect is then very accurately given of the corona as seen by the unaided eye.

A. BROTHERS

THE LATE EAST INDIA COMPANY'S MUSEUM—A ZOOLOGIST'S GRIEVANCE

THE late East India Company in their former palace in Leadenhall Street were in possession of a valuable Zoological Museum. It contained specimens in all departments of science, received from the Company's Oriental dominions. These had been contributed by public servants, attached as naturalists to various missions, or had been given by gentlemen of the civil and military services to the Court of Directors. Amongst the contributors to the East India Museum, it will be sufficient to mention the names of Dr. F. Buchanan Hamilton, Dr. Horsfield, Sir T. Stamford Raffles, Col. Sykes, Dr. Wallich, Mr. M'Clelland, Dr. Falconer, Mr. Griffith, and Mr. Hodgson, to prove that the collection was one of no ordinary merit. The Zoological importance of the East India Company's Museum was further augmented by the preparation and publication by, or under the superintendence of, the late Dr. Horsfield, of several catalogues. Of these may be particularly mentioned that of the Mammalia, published in 1851, and that of the Birds in 1854 and 1858, the second part of which bears likewise the name of Mr. Frederick Moore, then assistant-keeper of the Company's Museum, as joint author, on its title-page.

When the East India Company became extinct, and the premises in Leadenhall Street were vacated, the Museum was removed to Fife House, Whitehall, but was very imperfectly exhibited there, a large portion of the contents (the more bulky specimens in particular) being kept stowed away in boxes. When naturalists who wanted to consult specimens remonstrated at their inaccessibility, they were told that this was a mere temporary arrangement, and that when the magnificent buildings of the new India Office were completed, special accommodation would be assigned to the Museum, and there would be ample space for everything. At length the time arrived. The new India Office, with its suites of *salons*, assembly rooms, waiting rooms, and apartments of every description, was finished and opened. Fife House was demolished, and everything that it contained was removed to the new establishment. But when space was required for the Museum it was discovered that the only rooms assigned to this purpose were three or four chambers in the uppermost story, which would not contain a tenth part of the collection. Dr. Forbes Watson, the present chief of this department, has thought it right to devote these to the exhibition of a fine series of specimens illustrative of the arts and manufactures of British India, and we are by no means disposed to find fault with his decision on this subject. But it is the duty of the Government, we maintain, either to provide proper space for the Zoological collections also in the New India Office, or to transfer them to some other Institution, where they may be at least accessible to the scientific student. These Zoological collections contain a large number of typical specimens, without reference to which it is impossible in many cases to ascertain the identity of the species. Some of these typical specimens have, we believe, been handed over to the British Museum, but a number of them still remain in the collection, packed away, we are told, in the same cases in which they were originally removed from Leadenhall Street. This is, we maintain, a great and crying scandal, though as only a few working Zoologists are injured thereby, it is difficult to excite popular feeling upon the subject. In taking over the goods and chattels of the former Company, the India Office must certainly be held to have accepted the corresponding liabilities. Amongst these, it cannot be denied, was that of keeping, at least safe from destruction and in a state accessible to the scientific student, the specimens which the servants of the former Company amassed at such an expenditure of time and toil. If, as we are told, the new India Office is already so short of space that it is not possible to find room for them within its precincts, it is very simple to obtain the necessary accommodation elsewhere. We have good reason to know that Naturalists working on various branches of Indian Zoology are frequently brought to a standstill by the impossibility of access to this important collection, and we trust, therefore, that some steps will be taken to remedy the evil

P. L. S.

*THE METAMORPHOSES OF INSECTS**

THIS very handsomely got-up volume is illustrated by 40 full-page engravings, many of which are exquisite landscapes as well as representations of insects in their various stages; and by about 200 excellent woodcuts in the text, from which we have selected a few specimens as samples of the rest. The subject of insect transformations presents us with so many curious examples of instinct, and such strange eccentricities of structure and habits, as to be especially adapted to attract the attention of the young, and to lead them to study this most fascinating branch of Natural History. The name of M. Emile Blanchard, and the high scientific reputation of Prof. Duncan, are a sufficient guarantee that the facts are accurately stated. In the introductory portion of the work, the main features of the external structure and internal anatomy of insects are exhibited by such large and clear illustrations as to be easily comprehended, the changes in the nervous system, from the larva to the perfect insect, being particularly well shown. The nature of metamorphosis and its different kinds are then explained, and a series of chapters is devoted to each order of insects, beginning with the Lepidoptera and ending with the Crustacea.

Among the more remarkable forms in the first-named order are the Psychidæ, small moths the females of which are not only without wings, but have neither legs nor antennæ. The female *Psyche* is, in fact, a mere helpless egg-bag, which never quits the case or covering in which it was bred. The males are small delicate moths with bodies covered with long silken hairs, and with dusky semi-transparent wings. The larvæ live in cases made of silk or vegetable tissue, bits of straw, stick, or leaves, and they carry these cases just as snails do their shells.

The ravages of the Tineidæ and the curious cases of *Coleophora* and *Gelechia* are illustrated by figures after Stainton; while the cut on p. 331 represents the beautiful pink or violet net-work cocoons in which some Brazilian species suspend themselves by slender threads.

The parasitic Hymenoptera forming the families Ichneumonidæ, Chalcididæ, and Proctotrupidæ are well described, and a quotation from this chapter will exhibit the style in which the book is written:—

"These parasites are very pretty and elegantly-formed insects when in the adult form, and are gifted with great agility and restlessness; but in their early condition they cannot move, having no locomotive organs, and their structures are so soft that they are destroyed with the greatest ease. The larvæ look like worms or maggots, and do not attain a great perfection of development during their growth. All the parasites seek out a caterpillar, a larva, or an insect which suits their purpose, in order to lay an egg within its body. The larva which is born from this egg is nourished by the blood and fat of the victim, whose vital organs it does not touch or injure in any way; for were it to die, the parasite would come to an end also. It is only when the larva is nearly full grown, and is about to undergo its metamorphosis into a pupa, that it appears to know that the life of the victim is not likely to be of much further use. It then devours the internal organs of the unfortunate insect, and undergoes its transformation. The skin of the victim protects some of the pupæ of its destroyers after all the inside has been eaten. Nearly all, if not quite all, insects are subject to the attacks of parasitic Hymenoptera. Fine, smooth, and brightly coloured caterpillars often have a black spot upon their skin, and this is the healed wound of the ovipositor of one of the parasites. Sooner or later the creature is sure to die, and it never reaches the stage of growth when it can lay eggs or reproduce its kind, for before this time the growing larvæ within destroy it, as it were, by slow consumption. Some affected caterpillars die soon, others nearly reach their full growth, and a few undergo their transformation into the chrysalis state before death. It is, therefore, not an uncommon thing for a butterfly-collector, who hopes to see a fine moth disengage itself from its pupal covering, to be disappointed by the appearance of several little parasitic Hymenoptera that had been living within the chrysalis he has been keeping."

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* "The Transformations or Metamorphoses of Insects (Insecta, Myriapoda, Arachnida, and Crustacea)." Being an adaptation for English readers of M. Emile Blanchard's "Metamorphoses, Mœurs, et Instincts des Insectes." By P. Martin Duncan, F.R.S., Professor of Geology in King's College, London. (Cassell, Petter, and Galpin)

One of the most curious recent discoveries among beetles is that which was published by Schiödte, in 1864, of viviparous Staphylinidæ. These are about the tenth of an inch long, and are found in the nests of the *Termites* of Brazil. They are distinguished by the swollen development of the abdomen, which is carried in a most peculiar manner, being turned up and allowed to rest on the back of the insect. The enormous distension of this part of the body is due to the fact that the beetles do not lay eggs, but produce living larvæ, and they are the only beetles that do so. It is supposed that the hairs which cover some parts of the abdomen are furnished with a peculiar secretion that is liked by the white ants among which they live.

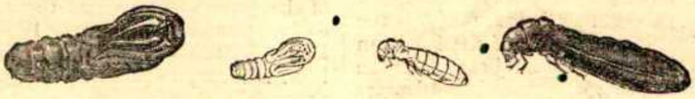

FIG. 1.—THE CHRYSALIS AND THE FEMALE PERFECT INSECT OF *Psyche graminella*.

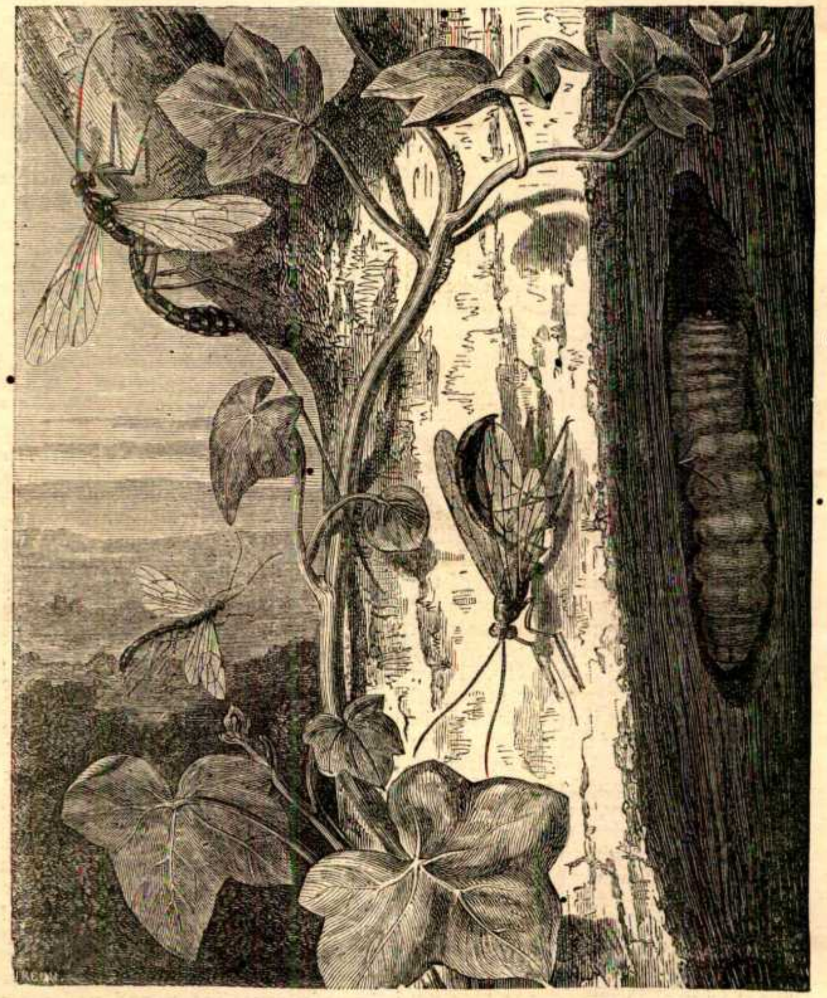

FIG. 2.—ICHNEUMONS

Ephialtes manifestator. The male is flying on the left, and the female is introducing an egg into the body of a larva. Another *Ephialtes, Rhyssa persuasoria,* a female, is on the branch to the left hand.

The extraordinary economy of many of the Diptera or flies is exhibited in a variety of beautiful cuts, one of the best of which represents the metamorphoses of *Stratiomys chameleo.* This fly frequents flowers in order to prey upon other insects, but its larva lives in stagnant water, and is a long, hard-skinned creature, whose small head is furnished with two minute hook-like mandibles. The terminal segments of the body are gradually narrowed, and can be elongated like a sliding telescope, and the slender extremity terminates in two small orifices crowned with hairs. This larva swims about in shallow water, and when it wants to breathe it sticks up the end of its body and respires through the two small holes at the apex. When the larva is mature it floats on the surface, the pupa being formed within the skin, which serves at once as a cocoon and as a boat, from which in due time the brightly-coloured and active fly escapes to its aërial existence.

In the last chapter a short but clear account is given of the recent discoveries as to the metamorphoses of Crustacea, from the works of Spence Bate, Fritz Müller, Darwin, and others.

In the original work reference is chiefly made to common French or Continental insects, some of which are natives of our own country, while many are not found here. In adapting the work for English readers, it would have been well to have stated in every case whether the insect mentioned was an English one. The great carpenter bee (*Xylocopa violacea*), for example, is described as "not uncommon;" but the reader is not told that, although common in France, it is unknown in England. More simple and familiar language might also have been occasionally used; but these are small defects in so useful and attractive a work, which is just the thing for a present to an intelligent and inquiring country schoolboy.

ALFRED R. WALLACE

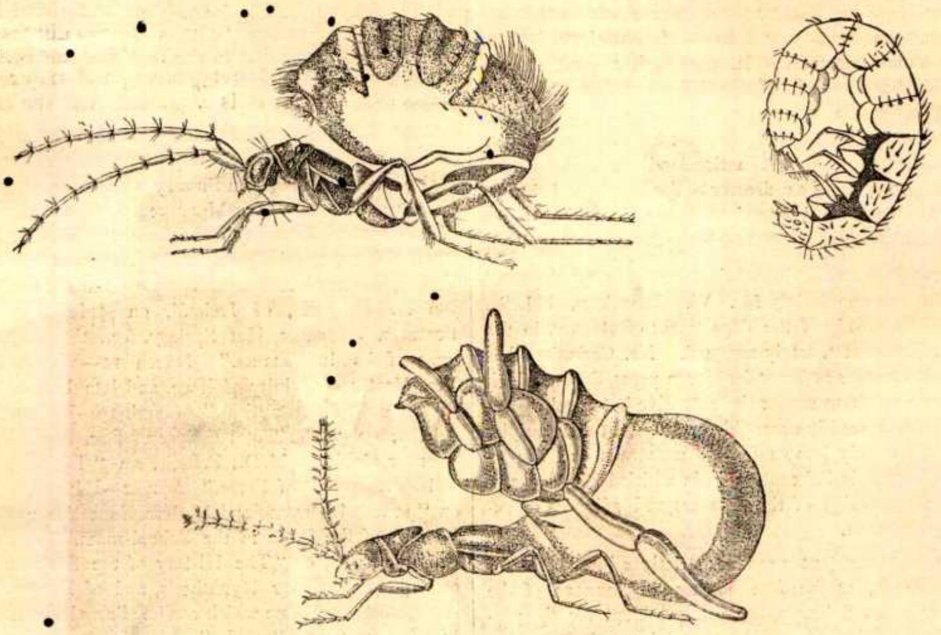

FIG. 3.—VIVIPAROUS *Staphylinidæ*. (After Schiödte.)

Corotoca melantho and larva. *Spiractha Eurymedusa*.

The upper figures are those of *Corotoca*. The turning up of the hinder parts of the body is very evident in the engraving.

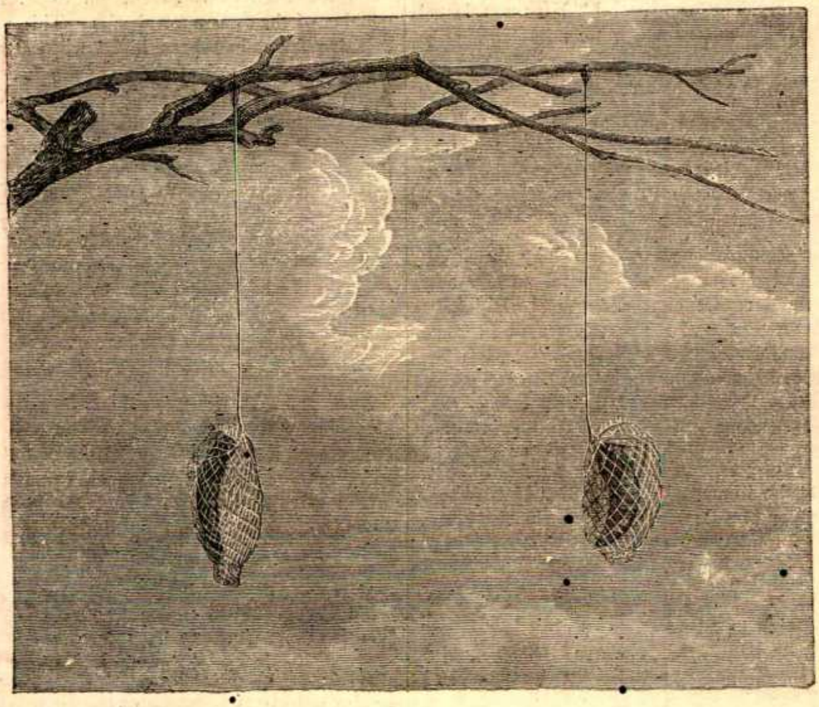

FIG. 4.—COCOONS OF BRAZILIAN *Tineina*.

NOTES

We understand that the Home Secretary has intimated to Dr. Lyon Playfair, member for the Edinburgh University, that he will submit the name of Mr. Geikie, F.R.S., to the Queen as first professor in the new Chair of Geology in that university. This has been done at the express recommendation of Sir Roderick Murchison, who, as already announced, has given the sum of 6,000*l.* to found the chair. In spite of her great mineral wealth, Scotland has no school of applied science like the State-supported establishments in London and Dublin. It is matter for congratulation, therefore, that the first appointment to the first Chair of Geology established in Scotland should have been given to one whose position as director of the Geological Survey in that part of the United Kingdom will enable him to act effectively in the teaching of the practical applications of geology.

The trustees of the British Museum have wisely not departed from precedent in appointing Mr. W. Carruthers, F.L.S., F.G.S., to the Keepership of the department of Botany, in the room of Mr. J. J. Bennett, lately resigned. Mr. Carruthers has been Mr. Bennett's senior assistant for eleven years, during which time he has done much to render the extensive botanical collections of the Museum readily accessible and useful to the public. Mr. Carruthers is widely known for his numerous and important papers on Vegetable Palæontology, in which he has greatly contributed to a knowledge of the structure and affinities of extinct forms of vegetation.

According to a constant practice strictly adhered to for almost two centuries, at the first sitting in January of the French Académie des Sciences, the vice-president for the preceding year fills the office of president, and keeps it for twelve months. But M. Coste having left Paris before the siege, the change in the presidency could not take place. The members present, to the number of thirty-one, elected the vice-president for 1871, who will be president for 1872. M. Faye was elected and proclaimed by M. Lionville, who was then in office. The election took place with the thermometer at – 6° C. No fire was allowed to be lighted, in consequence of the scarcity of fuel.

Dr. W. B. Carpenter recently delivered his Lecture, "On the Temperature and Life of the Deep Sea," at the Hulme Town Hall, Manchester, and it is reprinted as one of the penny series of "Science Lectures for the People," to which we recently referred in terms of commendation.

The report of the commissioners appointed by the Lord-Lieutenant and General Governor of Ireland in October 1868 to inquire into and report on the Artificial Cultivation and Propagation of Oysters, and the methods used in some parts of Great Britain and France for these purposes, has just been printed. It forms an octavo blue-book of nearly 200 pages, and is illustrated with woodcuts and ten plates. We hope in a future number to give a short notice of the recommendations offered in this report.

We understand that the library of the late Dr. Matthiessen, including a large selection of valuable works on chemistry and physics, is to be sold to-morrow, at Puttick and Simpson's Auction Rooms, in Leicester Square.

Prof. Karl Koch, of Berlin, proposes the establishment in that city of a Dendrological Garden, to be specially devoted to the cultivation of species and varieties of all kinds of foreign trees and shrubs. Plans of the proposed garden were shown at the last meeting of the Council of the Royal Horticultural Society.

M. Janssen is now at Bordeaux preparing his memoir on the Eclipse, for presentation to the French Académie des Sciences.

The Astronomer Royal for Ireland has just published the first part of a series of astronomical observations and researches made at Dunsink, the Observatory of Trinity College, Dublin. This part contains results of observations made with the South Refractor from June 1868, to October 1869, and consists of 88 quarto pages and three plates, printed at the expense of the Board of Trinity College. We purpose, in an early number, to give a short account of the College Observatory and of the equatorial erected for the fine 11¾in. object glass presented by Sir James South to the college.

The following is a list of the third series of lectures arranged by the Sunday Lecture Society to be delivered at St. George's Hall, Langham Place, on Sunday afternoons, at half-past three o'clock :—Feb. 26—Mr. Moncure D. Conway, on "The Past and Present of New England: its Early History, Physical Features, Literary and Religious Development, and Sketches of leading thinkers—Emerson, Theodore Parker, &c." March 5—Jon A. Hjaltalin, of Iceland, on "Iceland: its Physical Features, Volcanoes, Hot Springs, &c., the Manners and Customs of its Inhabitants." March 12—W. G. Clark, M.A., Vice-Master of Trinity College, and late Public Orator, Cambridge, on "Protestantism." March 19—J. Norman Lockyer, F.R.S., on "The Total Eclipse of Dec. 22." March 26—T. Spencer Cobbold, M.D., F.R.S., on "The General Structure and Development of Ferns." April 2—Edward Maitland, B.A., on "Jewish Literature and Modern Education; or the Use and Mis-use of the Bible in the School-room." April 16—W. K. Clifford, M.A., on "The History of the Sun: an Explanation of Laplace's Nebular Hypothesis, and of Recent Controversies in regard to the Time which can be allowed for the Evolution of Life." April 23—Prof. J. S. Blackie, on "War; its Causes, Character, and Consequences."

At the sitting of the 23rd January, the French Academy learned the death of M. H. Regnault, son to M. Regnault, the learned member of the section for physics. This young man, a painter of rising fame, had been killed when attacking the woods of Buzenval on the 19th of the same month. A deliberation of the assembly was taken to congratulate M. Victor Regnault, the father, then at Geneva. The body having been found was buried in the Père La Chaise before a deputation from the Académie des Sciences, Académie des Beaux Arts, and Académie Française. The proceedings were recorded in the *Comptes Rendus*.

During the week ending 18th February upwards of 3,500 British objects, consisting of sculpture, pottery, woollens, and educational works and appliances, were delivered at the buildings of the International Exhibition of 1871, besides foreign objects from Bavaria, Belgium, and Saxony.

We are informed that public meetings on the subject of Science and Art Education have recently been held in many places in the West of England. The well-known Royal Albert Museum at Exeter, with its Science and Art classes, museum, and free library, has done much towards attracting attention to the value of scientific knowledge in this part of England. The Plympton Grammar School, which has just been reorganised under a new scheme, will include the teaching of chemistry and drawing as a regular part of the school course.

Prof. Phillips is busily engaged at Oxford upon his new work on the "Physical Geography and Geology of the Thames Valley." This volume will contain numerous plates of fossils, illustrative of the various beds to be met with in the district, all of which are drawn by the learned Professor himself. In addition to these the numerous drawings of the magnificent series of Ceteosaurian remains lately acquired by the Oxford Museum will cause this volume to be eagerly looked forward to by every geologist and palæontologist

The offer to build a Museum at Hereford in connection with the Woolhope Naturalists' Field Club has been very gratefully accepted by the Corporation of that town, and there is every prospect of Mr. Rankine's generous scheme being fully carried out.

The British Museum has lately acquired two fine specimens of the most gigantic of known crabs. This species measures 10 feet between the tips of the claws, but has a comparatively small body, triangular in shape, somewhat convex. The claws are thin, and about 6 feet in length, including the pincers. Its habitat is Japan, where it is, we believe, eaten for food, and it seems strange that specimens should be so scarce in Europe. It was described and figured by Kæmpfer, in 1763, in his account of Japan, and is now named *Macrochira Kæmpferi* (Dettaan) in commemoration of that eminent naturalist. One specimen has also been acquired for the Edinburgh Museum.

At the sitting of the 30th January the French Academy learned the death of M. Gustav Lambert, who was engaged in preparing an expedition to the North Pole when war broke out, and who had been badly wounded at Montrelaut when fighting at the head of a company of National Guards which he commanded. M. Élie de Beaumont read a memoir sent four years ago by the enterprising intended explorer. Before leaving Paris for his last fight, he had sent to the Défense Nationale a memoir on the means of communication between Paris and the Provinces. This memoir will be published when peace shall be restored.

The large series of fossils in the Oxford Museum is in process of re-arrangement, and the space formerly occupied by the Physical Science Apparatus, removed into the Physical Laboratory, is now devoted to large collections of fossils hitherto undisplayed for want of room. Chief among these is to be noticed the magnificent series of Ceteosaurian remains from Enslow Bridge, which is the finest collection of gigantic reptilian remains to be seen in any museum in the world. The greatest care and ingenuity have been displayed in reconstructing these immense bones, many 4ft. or 5ft. long, from the broken and fragmentary state in which they were originally found.

The splendid meteor noticed by Mr. Wilson, in our last number, as having been observed at Rugby, was also very clearly seen in the neighbourhood of Worcester a few minutes past nine o'clock. It is described as falling towards the S.W., and bursting like a rocket, when it left a trail of light behind it for some time after its disappearance. No audible explosion was heard. The atmosphere was clear, and there were only a few clouds in the sky.

M. Bienaymé has published in the *Comptes Rendus* a rectification of a catalogue of the articles written by Cauchy, the celebrated mathematician, formerly a member of the French Institute. Some of the articles attributed to Cauchy were written by Cournot. This list is of some importance, as Cauchy's works are under publication at the expense of the French Academy, according to a vote which was taken a few months before the war broke out.

We have received the first report of the Cheltenham College Natural History Society, which we commend to the notice of similar youthful societies, for the modest earnestness that pervades it. The achievements of the society for the first year of its existence are summed up as follows:—"Botany—Decidedly well worked; best point of the Society. Entomology—Also well worked; sadly deficient in *Coleoptera*. Zoology—Practically reduced to Ornithology; so far, so good. Geology—Little attempted; less done." In so rich and interesting a county, geologically speaking, as Gloucestershire, we hope the Society will have next year a very different tale to tell. We notice that in order to encourage the pursuit, the Council offers a prize for the best original collection of local fossils; but trust this will not tend to promote the habit of collecting for mere collecting's sake—the bane of young naturalists. From the general tone of the pursuit of Natural History in the College, however, we hope for better things. It is interesting to find that such subjects are discussed at their meetings as the theories respecting the Aurora, and the causes of the November Showers of Meteors; and although the papers on "The Flowers of Virgil's Eclogues" and "The Flowers of Virgil's Georgics" are stated to have been "scarcely suited to the requirements of a scientific meeting," we doubt not they will have invested the old poems with a new interest in the eyes of both the writer and his hearers. Some of the papers are, as might be expected, crude and inaccurate; the one on "Butterflies at Home and Abroad" requires revision, and the statement that the Killarney bristle-fern, *Trichomanes radicans*, has been gathered by one of the members in Cornwall, must be received with caution. Although this would not be "the only spot in England where that fern has yet been found" (there being old records of its having been gathered in Derbyshire, and it having recently been unquestionably met with in North Wales), and there is no inherent improbability in its growing in Cornwall, yet the specimen stated to have been gathered "not half an inch high," can scarcely have been satisfactorily determined to be this rare fern. Seedling ferns of all kinds are extremely difficult to distinguish, and when growing in damp places, frequently simulate the filmy appearance of the Irish fern. We shall watch with great interest the progress of the Cheltenham College Natural History Society.

Dr. C. L. Spiegel's "Cultur-historische Tafeln," published at St. Petersburg, gives in parallel columns the men distinguished in each epoch from the earliest times to the present in politics and war, technical or mechanical discoveries, medicine, divinity, natural science and mathematics, philosophy, law, history, belles-lettres, and the fine arts; in Greece, Italy, Spain and Portugal, France, Great Britain, America, Sweden, Norway and Denmark, Holland, Germany, Russia and Turkey, Asia and Africa. It is printed in German.

The register taken by M. Renan at Montsouris Observatory shows that the minimum temperature in December was $-11{\cdot}7°$ C. in the garden, and $-12{\cdot}8°$ on the roof. This minimum is not exceptionally low for the district. The feature of the temperature for the month was not so much the number of degrees under freezing-point as the continuity of cold. Nine days only exhibited a mean temperature above the freezing-point. The temperature for the whole month was about 8° F. lower than the mean temperature of Paris for December.

Willkomm and Lange's "Prodromus Floræ Hispanicæ," published at Stuttgart, is now brought down to the third part of the second volume, including the natural orders to the end of the Gamopetalæ or Corollifloræ.

Freiherr Ludwig von Hohenbuhel-Heufler has reprinted from the Proceedings of the Zoological and Botanical Society of Vienna an interesting contribution to botanical history, "Franz von Mygind, der Freund Jacquins." Mygind was born at Broust, in Jutland, in 1710, and after his education at the University of Copenhagen, and a short stay at St. Petersburg, took up his residence at Vienna, and devoted his attention to botany. He speedily became an intimate friend of Jacquin's, and corresponded with the most eminent scientific men of the day, including Priestley, Sir Joseph Banks, L'Héritier, and Gmelin. He did great service in investigating the flora of Austria, paying the expense of two Alpine expeditions by Wulfen. He died in 1789.

THE MICROSCOPE

IMPROVEMENTS IN THE LENSES OF MICROSCOPES

FOR some time, people in England have been content to let the improvement of the optical powers of the microscope remain entirely in the hands of the makers, believing, apparently, that Mr. Lister had effected all in his suggestions and improvements that could be desired. Dr. Royston Pigott, an able mathematician, formerly Fellow of St. Peter's College, Cambridge, and a Doctor of Medicine of that University, was not, however, inclined to look at the matter in this way, and for many years has been working and experimenting with a view, first, to test the accuracy of our best object-glasses, and, secondly, to suggest means for their improvement. It should be remembered that Oberhauser, Nachet, and especially Hartnack, on the Continent, not satisfied with the old system of combinations for object-glasses, and not having the benefit of Lister's researches, have made excellent objectives on a totally different system, and during the last few years the last-named maker has carried his system of "immersion lenses" to such a point of excellence as really to surpass the best glasses on Lister's system, in definition, penetration, working distance, and illumination. Those who do not admit the excellence of these objectives, which are now used by nearly all German histologists, have probably seen older glasses, made at a time when Hartnack had not reached his best. It is worth stating, now that the Parisian opticians are inaccessible, that Gundlach of Berlin has succeeded in making excellent glasses of high power at astonishingly small prices, some of his 1-12ths and 1-16ths immersion 1-16ths (so-called), being admirable in their performance. They are not, however, equal to Hartnack's glasses, which, though costing far less than what similar English glasses cost, yet are more expensive than Gundlach's. It is only fair to all parties concerned to state that the terms 1-8th, 1-12th, 1-16th, &c., as now applied to an object-glass, appear to have no definite meaning, but depend on the caprice of the maker, since the magnifying power of glasses, with the same fraction assigned to them, differs enormously.

To return to Dr. Royston Pigott. He found the usual means of testing an object-glass by trying if it gave some particular appearance with a "test object," such as the Podura-scale, very unsatisfactory, since we have no certainty to begin with as to what is the true appearance of such an object. He therefore examined minute images of objects of which he knew the true form, such as a watch-face or thermometer-scale, forming these images by aid of mercurial globules and the condenser properly adjusted below the microscope-field. By this means he has found that object-glasses corrected so as to show dark, sharply marked spines (like ! ! !) on the Podura-scale—a favourite test-object with our microscope-makers—give false, blurred, and distorted appearances with his known images, and on making such corrections of the objective as to show the known images in their true form, he finds that the Podura-scale, examined with the corrected objective, is not really marked at all, as supposed, but is beset with a series of bead-markings, which by intersection, when improperly defined, give the curious appearance like notes of exclamation. This important discovery of the falsity of our high powers (1-8th to 1-16th), has led Dr. Royston Pigott to pay more attention to the lower powers, and he finds that though you may not get so much actual amplification, you yet get a truer effect, and greater clearness of detail, by employing very carefully made low powers (1-2nd to 1-5th) and increasing the magnifying power at the other end of the microscope, *i.e.*, the eye-piece. We have in this way seen the beaded structure of the scales of Podura more satisfactorily than with very high objectives even when corrected so far as they would admit, and we may say the same of some Diatom-valves *e.g.*, *Pl. formosum*. It would be most important to know how far such a change of combination would be useful in histological work.

The general upshot of Dr. Royston Pigott's investigations appears to be that it is desirable to shift the burden hitherto cast almost wholly upon the objective, to the other parts of the instrument. We should be content with an objective as high as a fifth, or even less. A very deep eye-piece is to be used; and to correct residuary aberrations of the objective, and at the same time amplify, Dr. Pigott has introduced an important adjustable combination *between the eye-piece and the object-glass*. There seems to be considerable reason for the step proposed by Dr. Royston Pigott. Just as great results were obtained in passing from the single lens or combination to the compound microscope of eye-piece and objective, so by adding distinct integral factors to these two, such as Dr. Pigott's "aplanatic searcher," we may obtain excellences quite impossible by any amount of attention bestowed on the objective alone, or only with difficulty reached by long labour, leading to very high price for high powers.

Dr. Pigott has, during the past year, published some account of his researches in the *Quarterly Journal of Microscopical Science*, and has communicated papers to the Royal Society, one of which is about to appear in the Philosophical Transactions.

Naturally at first the makers in London and the Microscopical Society were sorely tried by Dr. Pigott's exposure of the Podura-scale, but we hear, as one good result already obtained, that Messrs. Powell and Lealand have constructed a new 1-8th both dry and immersion, with great care, which is declared to be the best glass yet made. It has been proposed to form a committee for the purpose of examining carefully as to penetration, definition, and angular aperture, the best glasses of our English makers, the best American glasses, and the best of Hartnack's, Gundlach's, and others; the glasses being mounted similarly, with private marks only for recognition, so as to prevent all possibility of prejudice on the part of the committee. Were this done, the result, whichever way it tended, would be eminently satisfactory. Of this the writer is sure, that many persons—even eminent microscopists—have made up their minds about the qualities of foreign objectives, without having seen any, or only very poor examples, and then when a really fair specimen of such a glass is placed before them, they exclaim with astonishment "Why this is the finest glass I have ever seen." We shall be glad to receive suggestions or assistance, in carrying out the proposed comparison of objectives. Dr. Royston Pigott has expressed his willingness to aid in such an undertaking.

E. R. L.

REPORT ON DEEP-SEA RESEARCHES

Carried on during the months of July, August, and September, 1870, in H.M. Surveying Ship "Porcupine" *

By W. B. CARPENTER, M.D., F.R.S., AND J. GWYN JEFFREYS, F.R.S.

THE equipment of the *Porcupine* for the previous Expedition had been found so complete and satisfactory, that nothing more was considered necessary to prepare her for the work of the present season than the overhauling of her gear, and the manufacture of new dredges, sieves, and other apparatus, on the patterns of those which had already proved most serviceable. We had the advantage of the same excellent commander, now promoted to the rank of staff-captain, with his able staff of officers; and we would take this opportunity of again expressing our deep sense of obligation to them all for their hearty co-operation in our scientific work, and for the unvarying personal kindness by which our voyage was rendered a most agreeable one. A considerable part of the crew, also, consisted of the same steady and experienced men. The Meteorological Department supplied eight of the protected Miller-Casella thermometers, including the two with the performance of which we had been so thoroughly satisfied last year; and we usually employed one of these in conjunction with one that had not been used in the previous Expedition.

At the request of the Committee, Mr. Siemens undertook to devise an apparatus for testing the depth of sea-water to which light, or at least the actinic rays, can penetrate. The foundation of the apparatus which he constructed for this purpose is a horizontal wheel with three radii, each of them carrying a glass tube in which a piece of sensitised paper is sealed up. The rotation of this wheel round a vertical axis brings each of the tubes in succession out of a dark chamber in which it ordinarily lies, exposes it to light in an uncovered space, and then carries it into darkness again. This movement is produced by a spring; but it is regulated by a detent that project from the keeper of an electro-magnet, which is made and unmadeby the completion or breaking of a circuit that connects it with a galvanic battery. When the magnet is made, it lifts the keeper with its projecting detent; and this allows the wheel to be carried by the spring through one-sixth of its rotation, whereby the first of the tubes is brought out into the open space. There it remains until the circuit is broken, whereby the magnet is unmade; the keeper then falls, and the wheel is allowed to move through another sixth of a rotation, so as to carry on the tube into the dark chamber. A repetition of the making and unmaking of the magnet brings out the second tube, and shuts it up again; and another repetition does the like with the third tube. This apparatus, with a deep-sea lead attached to it, is suspended by an insulating cable that contains the wires whereby it is connected with the battery in the vessel. Being lowered down to any desired depth, the circuit is completed, the magnet made, and one of the tubes exposed for as long a time as may be wished; the circuit is then broken, the magnet unmade, and the tube shut up again. The second tube may be exposed for a longer time in the same place, or the apparatus may be lowered to a greater depth, at which the experiment may be repeated; and the third tube may then be dealt with in like manner.—The committee having been satisfied with the performance of Mr. Siemens' apparatus, it had been arranged that trial should be made of it, and also of his Differential Thermometer, now provided with an improved Galvanometer; and he had undertaken to send out a qualified assistant to take charge of these instruments during the Mediterranean Cruise. The declaration of war between France and Germany, however, unfortunately interfered with this arrangement; the assistant (a German) being recalled to his own country, and no other competent person being available on a short notice. Under these circumstances it was thought better that the Differential Thermometer should not be sent out; but it was hoped that such a trial might be given to the Photometric Apparatus as should at any rate determine whether satisfactory results might be anticipated from its use, or whether any modifications in its construction might be needed. The apparatus was sent out to Gibraltar under charge of Dr. Carpenter, and was got into working order by his son and himself in Gibraltar Harbour. It proved, however, that the action of sea-water on the bearings,—increased as this was by the galvanic current arising out of the contact of iron and brass in them,—so embarrassed its mechanical arrangements, that no fair trial could be made of its photometric efficiency. But the experiment served the important purpose of showing the weak points of the apparatus; and neither Mr. Siemens nor Dr. Carpenter entertains any doubt that it may be so reconstructed as to answer the purpose for which it was devised.

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* Extracted from the Proceedings of the Royal Society.

The work of this year's Expedition was divided, according to the plan originally marked out, into two Cruises: the First to examine the Deep-sea bottom between Falmouth and Gibraltar; the Second to make the like examination of the western basin of the Mediterranean between Gibraltar and Malta, and to determine its Physical and Biological relations to the Atlantic,—with special reference to the Gibraltar Current. The First Cruise was under the scientific direction of Mr. Gwyn Jeffreys, who was accompanied by a young Swedish naturalist, Mr. Josua Lindahl, of the University of Lund, as Zoological Assistant; whilst Mr. W. L. Carpenter, as before, took charge of the Chemical department,—his special work, on this occasion, being the determination, by volumetric analysis, of the proportion of chlorine in samples of Atlantic water taken from the surface, the bottom, and from intermediate depths, so as to serve as a basis of comparison with similar determinations of Mediterranean water. In the Second Cruise it had been arranged that Dr. Carpenter and Prof. Wyville Thomson should co-operate as before; but the latter being unfortunately prevented by serious illness from taking part in it, the whole charge of this Cruise rested with Dr. Carpenter. He was fortunately able to retain the assistance of Mr. Lindahl; and the chemical work was continued (as in the Third Cruise last year) by Mr. P. H. Carpenter; Mr. Laughrin throughout acted as dredger and sifter.

[For the exceedingly interesting and valuable zoological details of the various dredging-hauls, we must refer our readers to the report itself.]

Throughout the whole of this cruise the temperature of the *sea-bottom* was taken by the protected Miller-Casella Thermometers in nearly every sounding. As, for the reason already mentioned, no extreme depths were sounded, and as the general rate of the diminution of temperature on the margin of the North-Atlantic basin seemed to have been established by the serial soundings taken in the expedition of the preceding year, it was not thought necessary to repeat these; more especially as the variety of depths at which the *bottom*-temperature was ascertained gave adequate data for comparison with the results then correlated. It will be shown hereafter that this comparison leads to some very interesting conclusions, fully confirming the view advanced in the last report as to the slow northward movement of an upper stratum of warm water 700 or 800 fathoms in depth, and of the southward movement of the whole deeper stratum, bringing water of an almost icy coldness from the Arctic basin into the temperate and even the intertropical zone.

During the whole of this expedition the temperature of the *surface* of the sea was ascertained and recorded every two hours, both by day and by night; as were also the readings of the Dry and Wet-bulb Thermometers, which were placed in a small penthouse on deck, in which they were freely exposed to the surrounding air, but secluded from direct or reflected solar heat. The temperature of the *surface-water*, from the time of our leaving the British Channel in lat. 48° N. to our turning the corner of Cape St. Vincent in lat. 36° 50′ N., increased at a rate which bore a pretty regular proportion to the Southing. Thus, at the "chops of the Channel," it averaged 62° for five days; whilst, by the time we approached Cape St. Vincent, it had gradually risen to above 69°. After passing that point, however, we found both the *surface* and the *bottom*-temperatures to present certain variations, which, though not considerable in themselves, proved to be of great interest when taken in connection with the peculiar condition of the *embouchure* of the Strait of Gibraltar. These points, however, will be more fitly discussed hereafter; and we shall now only notice a sudden *rise* in surface-temperature of about 3° which showed itself as we turned the corner of Cape St. Vincent and entered the *north side* of the embouchure; and a sudden *fall* of nearly 6° (to 66°·4) which was encountered when we entered the mid-stream of the narrower part of the Strait as we proceeded towards Gibraltar.

In the course of the first portion of the cruise between Falmouth and Lisbon (beyond which point Mr. W. L. Carpenter was unable to proceed), thirty-six quantitative determinations were made by volumetric analysis, of the amount of chlorine in as many samples of Atlantic water, taken (1) from the surface, (2) from the bottom at various depths, and (3) from various intermediate depths. The greater part of these, as will be shown hereafter, indicated a very close conformity to a uniform standard of density, as indicated by a specific gravity of 1·0268, and a Chlorine proportion of 11·84 per 1000;[*] the chief departures being observable in the *lower* density of the *deepest* waters, and in the occasional *excess* of density in the surface-waters. The former is doubtless attributable to the fact that the *deepest* water is essentially *Polar*, and therefore derives its more dilute character from that source. The latter we are inclined to attribute to the influence of slight concentration by evaporation.

Second Cruise.—Leaving Gibraltar early in the morning of Monday, Aug. 15, we steamed out into the middle of the Strait, for the purpose of commencing our experiments on the Gibraltar current. The point selected by Capt. Calver (Chart of Strait of Gibraltar, Station 39) lay midway between Point Carnero, which forms the south-eastern boundary of Gibraltar Bay, and Jebel Musa or Apes Hill, which lies opposite to it, at a distance of only 8 geographical (9¼ statute) miles, on the African coast, the Strait being here nearly at its narrowest; and it was also that at which the greatest depth (510 fathoms) was indicated by the soundings marked on the Chart. With this depth our own sounding, which gave a bottom at 517 fathoms, agreed very closely; and having thus at once found the position most advantageous for our work, that position was precisely determined by angles taken by sextant from the ship between conspicuous objects on the shore. The *bottom*-temperature obtained in the first sounding was between 5° and 6° *higher* than that which had been met with at corresponding depths on the bed of the Atlantic about 100 miles to the westward; whilst the *surface*-temperature was *lower* by from 1°·3 to 2°, as will be seen by the following comparative statement:—

	Station.	Depth.	Surface temperature.	Bottom temperature.
			°	°
Strait of Gibraltar	39	517	70·0	55·5
Atlantic	31	477	71·3	50·5
Atlantic	32	651	71·5	50·0
Atlantic	33	554	72·0	49·7
Atlantic	34	414	71·7	50·0

This striking difference led us to take a set of *serial* soundings at intervals of 50 fathoms, and these gave a result, which, *though* it appeared anomalous at the time, was afterwards fully explained, and proved to be of unexpected import. The temperature fell, at 50 fathoms from the surface, to 56°; at 100 fathoms it was 55°·7; at 150 it was 55°·5; and from that depth to the bottom, at 517 fathoms, there was *no further descent.* Now, it will be shown hereafter that the thermal condition, which here so much surprised us by its contrast with that of the Atlantic waters, is that universally met with in the Mediterranean, the temperature of which, whatever may be its surface-elevation, falls to within 1° Fahr. above or below 56° at a depth of 50 fathoms, to a degree lower at 100 fathoms, and then remains uniform down to the greatest depth (1,743 fathoms) at which we examined it. And it thus appears that whilst the *surface*-water in this part of the Strait is certainly derived from the Atlantic, the *deeper* water, partaking of the thermal condition which so remarkably characterises that of the Mediterranean basin, may be fairly regarded as belonging to the latter.

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[*] The proportion here adopted,—the number of Grammes of Chlorine to 1000 Cubic Centimetres of water, is that employed by Prof. Forchhammer in his elaborate Memoir on the Composition of Sea Water (Phil. Trans. 1865).

This inference is in harmony with another fact ascertained on the same occasion, viz., the great excess in salinity shown by water brought up from the depth of 250 fathoms over the water of the surface. Whilst the specific gravity of the latter was found to be 1·0271, that of the former was 1·0293; and whilst the proportion of chlorine in the latter was 20·034 per 1,000, it was 21·775 in the former. Now in these particulars the surface-water agreed well with what had been found to be the condition of the water of the Atlantic, whilst the water at 250 fathoms agreed equally well with what proved to be the condition of the water at the like depth in the adjacent part of the Mediterranean. We were not a little surprised, however, to find that the water taken from the *bottom* (517 fathoms) was of much *less* density, as indicated both by specific gravity and by chlorine percentage, than that of the *intermediate* stratum; its specific gravity being 1·0281, and its proportion of chlorine 21·465. This apparent anomaly (the existence of which was confirmed by observations made on our return voyage) pointed to the existence of an out-current in the *intermediate* stratum as the probable explanation of the overlaying of the lighter by the heavier water. The specific gravity of the *bottom*-stratum closely corresponded, as we subsequently found, with that of the bottom-water over the deepest part of the area of the western basin of the Mediterranean.

These data having been obtained by the examination of the several parts of the vertical column at one and the same point, and this point being in the centre nearly of the narrowest part of the Strait, and at the deepest part of the channel, we proceeded to test the *actual movement* of water on the surface and at different depths beneath it.

The rate of *surface*-movement was easily determined. The precise position of the ship having been ascertained in the manner already stated, a small flat basket, presenting no such elevation above the water as would cause it to be influenced in any considerable degree by a moderate wind,* was sent adrift, so as to be freely carried along by the current; it was allowed to float for a determinate time, throughout which it was followed by the ship, and when it was taken up at the expiration of that time, the place of the ship was again ascertained as before. The space between the two points being then determined trigonometrically, the rate of the flow per hour, and its precise direction, could be readily calculated. Thus on the morning of August 15th the float was followed by the ship for fifteen minutes, during which it was found to have moved 4,377 feet in the direction E. by S. ¾ S., or at the rate of 2·88 miles per hour.

For the determination of the *movement of the water at different depths below the surface*, a current-drag had been constructed by Capt. Calver on a plan suggested by his previous experience; which had led him to the conclusion that a submerged basket lined with sail-cloth, which of course fills itself with water, presented a better resisting surface than any vessel of wood or metal. Such a basket being made the basis (so to speak) of the apparatus, its resisting surface was augmented by fixing two pairs of arms at right angles to one another across its upper end, and stretching a piece of sail-cloth between each arm and the side of the basket, which device caused a uniform resisting surface to be presented to the current whatever the manner in which the sails might meet it. To the lower part of this "drag" a couple of sinkers, of 112lb. each, were attached; and the whole apparatus was supported by cords meeting in a ring above it, to which the suspending line was secured.

This "current-drag" having been transferred to a boat, was lowered down by a couple of men placed in her, to the desired depth; and the boat was then left entirely free to move, being lightened by the return of the men into the ship. The motion of the boat would be the composite result of (1) the action of wind (if any) upon the tranverse section of the part of the boat above the water; (2) the action of the surface-current upon the transverse section of the immersed part of the boat; (3) the action of the upper current upon the suspending line; and (4) the action of the current in which the "drag" is suspended, upon the drag itself. Putting aside the *first* of these agencies, which will be of very little account if (as in the experiment now narrated) the boat be small and the breeze be light, it is obvious that the relative influence of the *second* and *third* to that of the *fourth* will depend upon the proportion between the surfaces presented by the boat, the line, and the "drag" respectively, and the strength of the current acting upon each. The surface given to the "drag" being larger than that of the boat and line taken together, the force acting on the "drag" will dominate, if it hang in an opposing current superior, equal, or even somewhat inferior in rate to that which acts on the boat and line, so that the boat would be carried along by the drag *against* the surface stream at a rate proportioned to the excess. If, again, the rate of the under-current should be greatly inferior to that of the surface, its action upon the "drag" might still be sufficient to neutralise that of the surface-current upon the boat and line, and the boat would then remain *stationary*, or nearly so. A still further reduction in the rate of the opposing under-current would make its action upon the "drag *less* powerful than that of the surface-current upon the boat and suspending-line, and the boat would then move *with* the surface-current but at a rate of which the great retardation would indicate an antagonistic force beneath. Supposing again, the water of the stratum in which the "drag" is suspended to be stationary, the action of the surface-current upon the boat and line would be opposed by the resistance offered by the deeper water to the movement of the drag; and the retardation of the movement of the boat would be less, though still considerable.—If, again, the stratum in which the "drag" is suspended should itself be moving in the direction of the surface-current, but at a reduced rate, there will still be a resistance to the movement of the "drag" at the more rapid rate of the surface-current; and this resistance will produce a proportional retardation in the motion of the boat. Finally, if the stratum in which the "drag" is suspended, with the intermediate stratum through which the suspending line passes, move at the same rate with the surface-current, the motion of the boat with the whole suspended apparatus will have the same rate as that of the simple float.

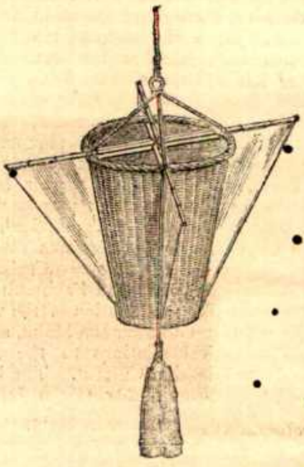

FIG. 1—CURRENT DRAG

Putting these respective cases conversely, it may be affirmed (1) that if the boat, having the "current-drag" suspended from it, should move *with* the surface-current and *at the same rate*, the stratum in which the "drag" hangs may be presumed to have a motion nearly corresponding with that of the surface-current; (2) that if the rate of movement of the boat *with* the surface-current should be *retarded*, a diminution of the rate of the stratum in which the drag hangs to a degree *exceeding* the retardation of the movement of the boat, may be safely predicated; (3) that when this retardation is so considerable that the boat moves *very slowly* in the direction of the surface-current, it may be inferred that the stratum in which the drag is suspended is either stationary, or has a slow movement in the opposite direction; (4) that if the boat should remain *stationary*, a force must be acting on the "drag" which is equal, and in the *contrary direction*, to that of the upper current upon the boat and suspending line; so that the existence of a counter-current is indicated, having a rate as much *less* than that of the surface-current as the resisting surface presented by the "drag" is greater than that offered by the boat and upper part of the suspending line; (5) that if the boat should move in a direction *opposed* to that of the surface-current, a motion is indicated in the stratum in which the "drag" hangs, which will correspond in *direction* with that of the boat, and which will *exceed* it in *rate*, the effect of the "drag" upon the boat being partly neutralised by the antagonistic drift of the surface-current.

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* It is obvious that the movement of the ship itself would be liable to be considerably affected by even a slight breeze, on account of the large surface of resistance presented by its transverse section (especially by its paddle-boxes) above the water. This would cause its drift to be *more* rapid than the current, if the direction of the wind should be *with* that of the current, and *less* rapid if the wind should be opposed to it.

Now our first set of experiments with the "current-drag" gave the following results:—

(1.) The surface-movement being first tested in the manner already described, its rate was found to be 2·88 nautical miles per hour, and its direction E. by S. ¾ S. The wind was W. by N., with a force of 4.

(2.) The "drag" having been lowered down to a depth of 100 fathoms, the rate of movement of the boat from which it was suspended was reduced to 1·550 mile per hour, or *rather more than half* the surface-movement. Its direction was E. ½ S. Taking into account the action of the wind and surface-current on the boat, it may be safely affirmed that at 100 fathoms the rate of the current was reduced to *less than one half.*

(3.) The drag having been lowered down to a depth of 250 fathoms, the boat remained nearly stationary, its *rate* of movement being reduced to 0·175 mile per hour, while its direction (S.E. ¼ E.) was slightly altered to the southward, though still easterly. From this we felt ourselves justified in inferring that the 250-fathoms' stratum had a movement in the *reverse direction*, acting on the current-drag with a force almost sufficient to neutralise the action of the upper stratum on the boat and suspending line. And this inference is strengthened by the extraordinary density of the water of this stratum, and is fully justified by the results of the experiments which we made on our return-voyage.

While these experiments were in progress, we had the pleasure of seeing the Channel Fleet, which was expected to meet the Mediterranean Fleet at Gibraltar, come in sight beyond Cape Tarifa; its approach having been indicated, long before even the tops of the masts of the vessels composing it showed themselves above the horizon, by the number of separate puffs of smoke which the experienced eye of our Captain enabled him to distinguish. As soon as all possibility of doubt was removed by the appearance of the masts, Capt. Calver communicated "Fleet in sight" by signals to the Admiral in Gibraltar Harbour; our position being such that *we* could be seen by him, though the Fleet could not. In due time, the massive hulls of the ironclads rose above the horizon, and whilst we continued at our work, all passed us in sailing order, at a distance of not more than a couple of miles, the ill-fated *Captain* being the chief object of interest. A few hours later, the *Monarch*, which had been detained for repair, but whose passage had been made in a shorter time by the free use of her steam-power, came in sight, and passed on in solitary grandeur to join the fleet, now united in Gibraltar Bay.

The whole of our first day having been consumed without our being able to work the "current-drag" in the deepest stratum, we anchored for the night near Point Carnero, with a view to resuming our experiments on the following morning. We then ran out to a spot almost precisely identical with that which had been our starting-point on the previous day; and commenced as before, by testing the rate and direction of the surface-movement. Its rate proved rather slower, being 2·40 miles per hour, instead of 2·88; and its direction was E. by N., instead of E. by S. ¾ S. Both differences seemed to be accounted for by the difference in the force and direction of the wind; which, having been W. by N. with a force of 4 on the previous day, was now W. ½ S. with a force of only 2. The "drag" was then lowered to a depth of 400 fathoms; but our expectation that it would there encounter a westerly (or outward) current sufficiently strong to carry the boat in that direction in spite of the antagonistic movement of the easterly (or inward) surface-current, was not verified on this occasion; for the boat slowly drifted in an E. ½ N. direction, its rate being 0·650 mile per hour. Whether this result should be taken to indicate a stationary condition of the deep stratum, or a slight movement in either direction, can scarcely be affirmed with positiveness; but from the indication afforded by the specific gravity of the water taken up from this depth, it would seem probable that the general movement of this stratum was at this time rather *westerly*, or in conformity with that which we attributed to the intermediate stratum, though at a slower rate. It will be shown hereafter that a decisive proof of such a movement was obtained on a subsequent occasion.

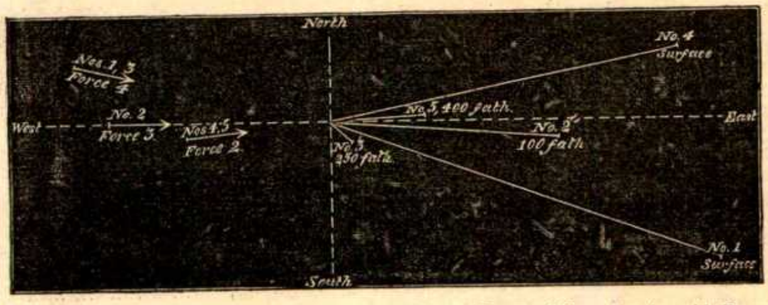

FIG. 2—Rate (per hour) and Direction of Movement of Surface-Float, and of Current-Drag at different Depths; with Force and Direction of Wind

Thinking it expedient to postpone the further prosecution of this inquiry until our return-voyage,—when we should be able to repeat our experiments, not only at this narrow end of the Strait where it enters the Mediterranean Basin, but also at that shallowest portion to the westward, where the Strait opens out into the Atlantic,—we put steam on before mid-day, and entered the basin of the Mediterranean, directing our course in the first instance to the spot (lat. 36° 0′ N., long. 4° 40′ W.) at which the sample of bottom-water had been obtained by Admiral Smyth, which, when analysed by Dr. Wollaston, was found to possess the extraordinary specific gravity of 11·288, and to yield a percentage of 17·3 of salt.* As we were within sight of both shores, and could distinguish several remarkable mountain-summits which were accurately laid down on our charts, the bearings of these enabled the situation of the ship to be determined with great precision; and Captain Calver undertook to place her within a mile of the point at which Admiral Smyth's observation had been taken. Having reached this, we took our first sounding in the Mediterranean, and awaited the result with no little interest. The depth proved to be 586 fathoms, or 84 fathoms less than that given by Admiral Smyth's sounding; but as the latter was not taken on the improved method now adopted, and as its correctness may have not improbably been affected by the strength of the easterly current which is here very perceptible, the discrepancy can scarcely be considered as of any real account as showing that the two points were otherwise than nearly coincident.† The specimen of bottom-water brought up by our bottle was found to have a specific gravity of 1·0292, whilst that of the surface-water was 1·0270. The volumetric determination of the chlorine gave 21·419 per 1,000 for the bottom-water, as against 20·290 per 1,000 for the surface-water. A decided excess of salt is thus indicated in the bottom-water, as compared on the one hand with the surface-water of the same spot, and on the other, with the bottom-water of the Atlantic, which had been generally found to show a rather smaller proportion of chlorine than the surface-water. But this excess is extremely small in comparison with that indicated by Dr. Wollaston's analysis. For, assuming his factor of ·134 as representing, when multiplied by the excess of specific gravity above that of distilled water, the total per-centage of salt, that per-centage is only 3·91, instead of being 17·3, as stated by Dr. Wollaston.

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* Phil. Trans. for 1829, p. 29; and Admiral Smyth's "Mediterranean," pp. 128—130.

† Thus Admiral Smyth states ("Mediterranean," p. 159) the depth in mid-channel between Gibraltar and Ceuta to be 950 fathoms; whereas it is now known to be but little more than 500 fathoms. "A little farther to the eastward," he says, "there is no bottom with 1,000 fathoms of line up-and-down (*upwards of* 1,300 *payed out*);" whereas the greatest depth as far east as Malaga Bay is now known not to exceed 750 fathoms. These errors are noticed in no invidious spirit, but merely to prevent their perpetuation. Admiral Smyth doubtless made the very best use of the means at his disposal; but a far more satisfactory method has now entirely superseded that formerly adopted.

This result accorded so closely with that obtained by Dr. Wollaston himself from the analysis of two other samples of bottom-water taken up by Admiral Smyth, the one in long. 1° E. from a depth of 400 fathoms, and the other in long. 4° 30′ E. from a depth of 450 fathoms, as well as with our own determinations of the specific gravities and chlorine per-centages of a great number of samples taken in different parts of the Western basin of the Mediterranean, that we cannot hesitate in regarding it as representing the *ordinary* condition of the bottom-water at this spot. And it seems to us far more probable that the sample furnished by Admiral Smyth to Dr. Wollaston had been concentrated by evaporation in a badly-stopped bottle in the three years during which it had remained in Admiral Smyth's possession, than that any extraordinary discharge of salt from a brine-spring at the bottom (a sort of *Deus ex machinâ* invoked by Admiral Smyth to account for the occurrence) should have given rise, in the spot at which his sounding was taken, to an exceptional condition of which no indication whatever was presented in our own.

The temperature-phenomena presented at this Station proved of singular interest. The *surface*-temperature, 74°·5, was higher than any that had been encountered on the Atlantic side of the Straits, even in a latitude half a degree farther south; and the observations which had been regularly taken every two hours showed that it had increased nearly *ten degrees* as we proceeded eastwards from Station 39, between 10 A.M. and 2 P.M. A part of this increase was doubtless due to the heating effect of the mid-day sun; but as the temperature of the air had not increased quite *six* degrees during the same time, and as it will be shown hereafter by a comparison of the diurnal averages of the surface temperature of the Mediterranean with those of the Atlantic, that the latter are at least four or five degrees higher than the former, it may be fairly assumed that at least half the increase was due to the passage from the colder Atlantic water of the mid-channel into the warmer water of the Mediterranean basin, the temperature of the latter being even here somewhat reduced by the inflow of the former.—The *bottom*-temperature was found to be here 55°; and this corresponded closely with that which we had met with in the Strait, while it was at least 5° higher than had been obtained at corresponding depths on the Atlantic side. Being desirous of determining the rate of its diminution, we took *serial* soundings at intervals of 10 fathoms down to 50, and then at 100 fathoms; with the following remarkable result:—

	°		°
Surface	74·5		
10 fathoms	69·3	diff.	5·2
20 ,,	65·0	diff.	4·3
30 ,,	63·0	diff.	2·0
40 ,,	61·7	diff.	1·3
50 ,,	59·7	diff.	2·0
100 ,,	55·1	diff.	4·6
586 ,, (bottom)	55·0	diff.	0·[illegible]

Thus there was a fall of 9°·5 in the first 20 fathoms; of 5°·3 in the next 30 fathoms; of 4°·6 in the next 50 fathoms; whilst from 100 fathoms to the bottom at 586 fathoms there was no further descent.

Whilst we were prosecuting these inquiries, we found ourselves surrounded—the surface of the sea being extremely calm—by great numbers of the beautiful floating *Velellæ*, which are occasional visitors to our own coast, accompanied by the *Porpitæ*, which are more exclusively restricted to warmer seas. With these was a great abundance of a small species of *Firola*, about 0·4 inch in length, the extreme transparence of which enabled every part of its organisation to be readily studied microscopically, its nervous system being specially distinguishable. Of this very interesting *Heteropod*, a full description will be hereafter published by Dr. Carpenter.

The result obtained by our first temperature-sounding in the Mediterranean, was fully borne out by that of the temperature-soundings taken during three subsequent days; which show an extraordinary uniformity of *bottom*-temperature at depths from 162 to 845 fathoms*:—

Station. No.	Depth, in faths.	Surface-temp. °	Bottom-temp. °
41	730	74·5	55·0
42	790	74·0	54·0
43	162	74·7	55·0
44	455	70·0	55·0
45	207	72·7	54·7
46	493	73·5	55·5
47	845	69·5	54·7

It will be observed that the surface temperature varied between 69°·5 and 74°·5; and that whilst the highest temperatures were shown at Stations near the African coast, the lowest presented itself between Cape de Gat and Cartagena. Now the Gibraltar inflow is very sensibly felt at Cape de Gat, where the current usually runs at the rate of a mile an hour; and of the strength of this current we had unpleasant experience. For on the 19th of August, as we were crossing from Station 46 towards the Spanish coast, we encountered a strong N.E. breeze, which, meeting the current, worked up a considerable swell; this prevented us from taking even a temperature-sounding on that day, and gave our ship a peculiar twisting or screwing movement, from which we were glad to escape by the subsidence of the breeze during the following night. During this day the surface-temperature of the sea came down from the average of 72°·2 which it had maintained on the 18th, to 66°·9. Had the weather been calm, we might have attributed this reduction to the colder Gibraltar in current; but as the average temperature of the air also fell from 73°·8 to 69°·8, and as the strong N.E. breeze must have had a cooling effect upon the surface of the sea, we should have deemed it probable that the reduction of surface-temperature was due at least as much to the latter as to the former of these causes, had it not been that a set of serial soundings which we took at Station 47 showed that the reduction extended very far down, as will be apparent on comparing the following results with those previously given:—

	°Fahr.
Surface	69·5
10 fathoms	59·0
20 ,,	57·5
30 ,,	56·5
40 ,,	55·7
50 ,,	55·3
100 ,,	54·7
845 ,, (bottom)	54·7

It will be seen hereafter that the observations made on our return voyage gave more distinct evidence of the cooling influence of the Gibraltar in-current.

At most of these stations we explored the bottom by means of the dredge, with results much less profitable than we had anticipated. Except near the coast, on either side, where the ground was rocky and unequal, the bottom was found everywhere to consist of a tenacious mud, composed of a very fine yellowish sand mixed with a bluish clay—the former predominating in some spots, the latter in others. Large quantities of this mud were laboriously sifted, often without yielding anything save a few fragments of shells, or a small number of *Foraminifera*; and in no instance was it found to contain any considerable number of living animals of any description. Our disappointment at this unexpected paucity of life was not small; and it was destined, as will hereafter appear, to continue through the whole of our dredging exploration of the deeper portions of the Mediterranean basin. The operation of dredging in the shallower portions nearer shore was rendered difficult by the rocky nature of the bottom, on which the dredge continually "fouled;" and after the loss of two more dredges and a considerable quantity of rope, Capt. Calver came to the conclusion that the "tangles" only should be used where the inequality of the soundings indicated danger to the dredge. Now the "tangles," whilst gathering

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* This uniformity, as we have since learned, had been previously observed by Captain Spratt, in his Soundings in the Eastern Basin of the Mediterranean; but owing it seems probable to the want of "protection" in his thermometers, he had set the uniform temperature too high, namely 59°. (See his "Travels and Researches in Crete," vol. ii. Appendix II.)

Polyzoa, Echinoderms, Crustacea, and the smaller corals, sometimes even better than the dredge, pick up but few shells; and hence our collection of Mollusca is altogether a scanty one. Nevertheless, many of the types we did obtain were of considerable interest. Thus at Station 45, at a depth of 207 fathoms, we got *Turbo Romettensis*, Seguenza, MS. (Sicilian fossil); *Scalaria plicosa* (Sic. foss.); *Odostomia obliquata*, Ph.; *Philine*, two undescribed species; and an interesting coral (*Dendrophyllia corrugosa*).

SCIENTIFIC SERIALS

THE *Geological Magazine* for January (No. 79) commences with an article by Mr. A. H. Green, on a subject which has lately attracted much attention, namely, the "Geological Bearings of the recent Deep-Sea Soundings." Mr. R. Tate contributes an article on the Invertebrate Fossils of the Lias, which includes a useful table of the known Liassic genera, showing the number of species in each found in Britain and on the Continent, with observations on the distribution of the genera, and descriptions of some new species. Mr. J. Clifton Ward has a paper on the Development of Land, illustrated by references to the geological history of Italy and England. In an article on the Transport of Wastdale Crag Blocks, Mr. Croll ascribes the distribution of those puzzling boulders to the action of land-ice, and adduces evidence in support of the opinion that the great ice-covering of Scotland overlapped the high grounds of the North of England. Mr. Hopkinson characterises a new genus of Graptolites, under the name of *Dicellograpsus*; it includes five species hitherto referred to *Didymograpsus*.

The February number of the same journal opens with a note on the Diamond-fields of South Africa, by Prof. T. Rupert Jones, in which the author endeavours to correlate the scattered and scanty information that we possess on the geology of that region. MM. Brady and Crosskey furnish lists of fossil Ostracoda obtained from the post-tertiary deposits of various localities in Canada and New England, with descriptions of six new species, illustrated by an excellent plate. From the Rev. Osmond Fisher, we have some interesting notes on phenomena connected with denudation observed in the so-called coprolite pits near Haslingfield in Cambridgeshire; and from Mr. Woodward a note on the new British Cystidean, *Placocystites Forbesianus* of De Koninck, which is identified, on the authority of Mr. Billings, with his *Ateleocystites Huxleyi*. The numbers also contain the usual notices, reviews, and smaller communications.

OF the *Wurttembergische Naturwissenschaftliche Jahreshefte* we have received the twenty-sixth volume, published in 1870, and including, besides the ordinary general notices of the Proceedings of the Natural History Society of Württemberg, some important papers in various departments of science.—Dr. Samuel Bartsch contributes a notice of the Rotifera observed by him in the neighbourhood of Tubingen, which he preludes with remarks on the anatomy and physiology of those animals. The species are not described, but the genera are characterised, and the author contributes valuable remarks on some of them. He establishes two new families: the Longisetæ, for the genera *Distemma*, *Rattulus*, *Furcularia*, and *Monocerca* of authors, and a new genus, *Monommata*, including *Notommata tigris* and *longiseta*; and the Loricata, for the reception of Ehrenberg's *Euchlanidota* and *Brachionæa*.—Prof. Fraas describes the progress of the geological investigation of Württemburg, and M. C. Deffner, the very curious structure of the Buchberg, an outlier of the Jura, near Bopfingen.—The most important geological paper is a notice of the fauna of Steinheim, relating principally to the remains of Miocene mammals and birds from that locality. This fauna includes, together with numerous well-known species some new forms of great interest, among which may be noticed especially, a species of *Colobus*, described by the author as *C. grandævus*; a new generic type allied to the badgers, and described as *Trochotherium cyamoides*; and species of *Chæropotamus* and *Tapirus*. The fauna is considered by the author to have its nearest existing representative in South-Eastern Asia and the great islands of the Indian Archipelago, and he regards the deposit as following in order of time, the "Langhian stage" of Carl Mayer, which includes the deposits of Weissenau, Oppenheim, Radoboj, and others in Germany; and of Arquato, Superga, Malta, and others in the South of Europe. This valuable memoir is illustrated with six plates.—Prof. C. W. Baur reports on recent geodetic surveys made in Württemberg, for the purpose of a European measurement of a degree.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 9.—"On the Problem of the In-and-Circumscribed Triangle," by A. Cayley, F.R.S.—The problem of the in-and-circumscribed triangle is a particular case of that of the in-and-circumscribed polygon; the last-mentioned problem may be thus stated—to find a polygon such that the angles are situate in and the sides touch a given curve or curves. And we may in the first instance inquire as to the number of such polygons. In the case where the curves containing the angles and touched by the sides respectively are all of them distinct curves, the number of polygons is obtained very easily and has a simple expression; it is equal to twice the product of the *orders* of the curves containing the several angles respectively into the product of the *classes* of the curves touched by the several sides respectively, or say it is equal to twice the product of the orders of the angle-curves into the product of the classes of the side-curves. But when several of the curves become one and the same curve, and in particular when the angles are all of them situate in and the sides all touch one and the same curve, it is a much more difficult problem to find the number of polygons. The solution of this problem when the polygon is a triangle, and for all the different relations of identity between the different curves, is the object of the present memoir, which is accordingly entitled "On the Problem of the In-and-Circumscribed Triangles;" the methods and principles are, however, applicable to the case of a polygon of any number of sides, the method chiefly made use of being that furnished by the theory of correspondence.

Anthropological Institute of Great Britain and Ireland, February 14.—Sir John Lubbock, Bart., M.P., F.R.S., President, in the chair. The President stated that this was the first meeting of the "Anthropological Institute of Great Britain and Ireland," since the union of the late Ethnological and Anthropological Societies under that name, and having vacated the chair in favour of Professor Huxley, proceeded to read a paper, "On the Development of Relationships." After some preliminary observations on the character of the family among the lower races of men, and the preponderance of the tribal tie, Sir John proceeded to discuss the conclusions drawn by Mr. Morgan from the valuable schedules of relationships collected by him and published by the Smithsonian Institution, especially with reference to his theory that the similarity between the Mohawk and Tamil systems indicated any Ethnological affinity between those races, a conclusion which Sir John was unable to accept. He then proceeded to show how in his opinion that similarity had arisen, and traced up the gradual development of correct ideas on the subject of relationships from the system of the Sandwich Islanders, which is the lowest on record, step by step to that of the Karens; showing that in each system there are points which can only be explained on the hypothesis of its development from a still ruder condition. He then compared these actually existing systems with those which would be produced by a retrogression of social customs, and showed that the systems of the lower races all indicate progress, and there are no instances of the existence of such a system as would arise in the case of degradation. He also laid stress on the fact that the social system is invariably in advance of the nomenclature of relationships, another evidence of progress as opposed to degradation. He showed that even in some European nations we have traces of earlier lower condition, and that therefore in the systems of relationships we have an interesting proof of the social progress of man, and the gradual development of family ties.—Mr. Hodder M. Westropp exhibited a Worked Flint Implement, said to have been found in a barrow, Ashby Down, near Ventor, Isle of Wight.—The Chairman announced that the meetings of the Institute would, during the remainder of the Session, be held on Mondays, commencing on Monday, 6th March.

Chemical Society, February 16.—Prof. Williamson, F.R.S., President, in the chair. The following gentlemen were elected fellows:—W. D. Herman, W. W. Houlder, G. Lockyer, jun., W. J. Lockyer, J. E. Mayer, R. Meldola, M. M. P. Muir, W. J. Reynolds, W. Smith, T. E. Thorpe. The following papers were read:—"On the production of wood spirit," by E. T. Chapman. The author began by remarking that the difficulties of obtaining correct information regarding this production are very great. The manufacturers are too jealous to disclose their “secrets.” The woods chiefly employed in wood distilling are oak, beech. Great pr[illegible], birch, thorn, crab or apple, hazel, alder, and ash and [illegible] preference is given to holly and yew, whilst poplar, elm, [illegible] the whole of the coniferous order are avoided. The ovens or retorts are of various forms, and either of cast or of wrought iron, protected outside by brickwork against the dire effects of the fire. The condensers of iron and copper are constructed so as to permit the ready passage of very large volumes of gas, and to admit frequent cleaning. The temperature at which the distillation is conducted influences the amount of the products; as a rule, greater heat yields more of the so-called naphtha, and a lower temperature more acetic acid. The liquid products of distillation which form two layers, are differently treated according to the object the manufacturer has in view. After having given a lengthy description of the various products obtained in wood distilling, the author went on to say that some easier and more exact methods of estimating the commercial values of the various products ought to be introduced.—“On the effects of pressure on the absorption of gases by charcoal,” by John Hunter. Very numerous experiments lead to the observations—first, that the amount of absorption increases with the pressure to which the gas is exposed; and secondly, the same change of pressure produces about the same amount of increase in the quantity of each gas absorbed.—“On the solubility of the phosphates of bone-ash in water holding carbonic acid,” by E. Warington.

Cambridge

Philosophical Society, February 13.—“On the Great Trigonometrical Survey of India,” by Colonel J. T. Walker, R.E. The author carefully described the process of carrying out the survey and the instruments in use, pointing out the various difficulties which were experienced, and the mode in which they were overcome. He also gave an account of the earlier efforts in this field and pointed out the importance of careful survey, and the mode by which accuracy in geodelic investigations was secured. In conclusion he discussed the mathematical difficulties which presented themselves, and a theorem which had been found useful in the reduction of observations.

Norwich

Naturalists' Society, January 3.—The chairman read a paper by Mr. W. M. Crowfoot, on Spontaneous Generation. After distinguishing between the *origin* of life and the *nature* of life, two very distinct subjects, which have at all times been more or less confounded with one another, Mr. Crowfoot proceeded to review briefly the history of the theories concerning the nature of life as propounded by Hippocrates, Paracelsus, &c.; he then gave a condensed history of the views concerning the origin of life, and of the experiments of Redi, Needham, Pasteur, Huxley, and Bastian, and a *résumé* of the discussion which has been recently carried on in the columns of Nature and elsewhere; concluding with some practical remarks on the nature of epidemic diseases, and the important results which may arise from such discussions. The thanks of the meeting were voted to Mr. Crowfoot for his very interesting paper, which was followed by an animated discussion.

Dublin

Royal Geological Society, January 11.—Rev. Maxwell Close in the chair. Edward Hull, F.R.S., read a paper on the Geological Age of the Ballycastle Coalfield, and its relations to the Carboniferous rocks of the west of Scotland.—Mr. John Leech read a paper on the moving bog of Castlerea, Co. Roscommon.

February 8.—Dr. Reynolds in the chair. The annual report of council, and statement of accounts for the year 1870 were submitted, and the following were elected as officers and council for 1871:—President, the Earl of Enniskillen, F.R.S.; Vice-presidents, Colonel Meadows Taylor, J. Emerson Reynolds, Sir Robert Kane, F.R.S., Rev. H. Lloyd, Provost T.C.D., F.R.S.; Sir Richard Griffith, Bart., LL.D.; Treasurers, William Andrews, and Samuel Downing, LL.D.; Secretaries, Rev. S. Haughton, M.D., F.R.S., and Alexander Macalister, M.D.; Council, Alphonse Gages, B. B. Stoney, W. Frazer, George Dixon, Alexander Carte, M.D., W. H. S. Westropp, C. R. C. Tichborne, Rev. Maxwell Close, Francis M. Jennings, Ramsay H. Traquair, M.D., R. Callwell, John Barker, M.D., John Ball Greene, Edward Hull, F.R.S., William H. Baily.—Mr. J. Scott Moore read a paper on a moulded piece of quartz, and exhibited a remarkable specimen of dendritic markings in granite.—A paper was read from Mr. G. H. Kinahan on foliation.

Perthshire

Society of Natural Science, February 2.—Dr. Buchanan White, president, in the chair. Mr. C. Fleckstein read a paper upon the Zoology and Botany of the Ancients. The paper, which was of general interest, was confined almost entirely to a consideration of the knowledge of natural history possessed by those nations termed *par excellence* the *ancients*, viz., the Greeks and Romans, and related chiefly to the researches of Aristotle and Pliny. Mr. W. Herd read a paper upon the Lepidoptera of Moncrieffe Hill and its neighbourhood. It was illustrated by specimens of the insects mentioned, and contained the results of Mr. Herd's own observations of the habits of the less common species found in the district selected.

DIARY

THURSDAY, February 23.

Royal Society, at 8.30.—On the Mutual Relations of the Apex-Cardograph and the Radial Sphygmograph Trace: A. H. Garrod.—On the Thermo Electric Action of Metals and Liquid: G. Gore, F.R.S.

Society of Antiquaries, at 8.30—On the Topography of Jerusalem, with special reference to the results obtained by the Palestine Fund Committee: Thomas Lewin, M.A., F.S.A. (Second paper.)

Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

London Institution, at 7.30—On the Action, Nature, and Detection of Poisons: F. S. Barff, M.A., F.C.S.

FRIDAY, February 24.

Royal Institution, at 9.—On Rumford's Scientific Discoveries: W. Mattieu Williams.

Quekett Microscopical Club, at 8.

Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, February 25.

Royal Institution, at 3.—Socrates: Prof. Jowett.

MONDAY, February 27.

Royal Geographical Society, at 8.30.

London Institution, at 4.—On the first Principles of Biology: Prof. Huxley. (Educational Course.)

Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

TUESDAY, February 28.

Royal Institution, at 3.—Nutrition of Animals: Dr. Foster.

WEDNESDAY, March 1.

Royal Microscopical Society, at 8.—Notes on the Microscopical Examination of Waters for Domestic Use: J. Bell.

Society of Arts, at 8.—On the Patent Laws and their Administration, with a view to the Adoption of Practical Amendments: A. V. Newton.

Royal Society of Literature, at 8.30.

London Institution, at 4.—On the first Principles of Biology: Prof. Huxley. (Educational Course)

Royal College of Surgeons, at 4—On the Teeth of Mammalia: Prof. Flower.

THURSDAY, March 2

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Linnean Society, at 8.—On the Tamil names of Plants: Rev S. Mateer.—Contributions towards a knowledge of the *Curculionidæ*: H. P. Pascoe.

Chemical Society, at 8.

Royal Institution, at 3—Davy's Discoveries: Dr. Odling.

London Institution, 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers

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Erratum.—Page 266, second column, lines 15, 16, for “three times” read “*g* times.”

THURSDAY, MARCH 2, 1871

THE SMALL-POX EPIDEMIC

THE present epidemic of Small-pox in London is the most destructive, we are told, that has occurred in London during the present century. This is a very painful disclosure, when it has been almost demonstrated that, of all contagious and epidemic diseases, it is the one over which man has the most control. It is a well-known fact that when persons have once had the small-pox they seldom or never take it again, and that the disease known as cow-pox is a modified form of small-pox, and that persons who have had this modified form of small-pox are as little liable to take this disease as those who have had the small-pox itself. This was the great discovery of Jenner, and the practice of vaccination has more than realised the hopes of its discoverer and his friends. Where vaccination has been carried out with energy, and communities by wise laws or individual action have seen that every child is duly vaccinated, there small-pox has not spread. It appears that where communities are all properly vaccinated, there, even if an isolated case of small-pox does occur, it has no pabulum to feed on, and it does not spread. It is only when the small-pox contagion is communicated to unvaccinated persons that the disease is set up, and has sufficient vitality to spread through a community. Forty-five millions of persons died in Europe from small-pox in the century preceding the introduction of vaccination, whilst it is calculated that it has not killed more than two millions of persons in Europe since the introduction of vaccination. In London, during the last century, one death in every fourteen was due to small-pox. Up to the present time in this century not more than one-fiftieth of the persons who have died in London have died of this disease Greater differences than even this have been observed in some of the cities and towns of the Continent of Europe. At Trieste the deaths from small-pox have been seventy-five times less than before vaccination; in Moravia, twenty-one times less; in Silesia, twenty-nine times less; in Westphalia, twenty-five times less, and in Berlin, nineteen times less.* These instances might be indefinitely increased, but we are anxious to show to what extent this disease is really controllable.

Supposing even that it is not demonstrated that small-pox can be eradicated by vaccination alone, carefully collected statistics show that when small-pox is taken by the vaccinated, it is much less fatal than among the unvaccinated. In the epidemic now prevailing in the metropolis, it is found that not more than six per cent. of persons who have been vaccinated die of small-pox, whilst about thirty-six per cent. of those who have not been vaccinated die. This is the proportion of deaths observed at the Small-pox Hospital from 1836 to 1851, so that it will be observed that the small-pox has neither lost nor increased in malignity.

There is a question which we ought to allude to here, and that is, Does vaccination lose its protective power? The best observers are of opinion that when vaccination has been properly performed, and the system brought thoroughly under the influence of the cow-pox, a person has no more liability to take the small-pox than if he had had the small-pox itself. But unfortunately, from various causes, vaccination is either improperly performed, or the disease is only imperfectly developed, and in such cases it is desirable that re-vaccination should be effected. It is not however, possible to say by looking at an arm, whether the operation has been properly performed, or the disease has perfectly developed. Under these circumstances it is no doubt desirable that every person should be re-vaccinated at least once in his life. The best time for the performance of this operation, where persons have been vaccinated in infancy, is between the twelfth and twentieth year. But in times of epidemic every person in the household should be re-vaccinated who has not been so before.

Thus much with regard to our knowledge of one means of averting this disease. That this means has not been adopted, arises partly from the ignorance of our population, partly from the perversity of our vestries and boards of guardians, and partly from the feebleness of our legislators.

The ignorance of our population of the means of preventing ordinary diseases is astounding, and if left to themselves with regard to vaccination, they will do nothing, from sheer ignorance of the nature of small-pox or the nature of its great antidote. Our coroners' courts bear testimony to this, where poor people have excused themselves for not having their children vaccinated, by not being aware of its value or of the measures to be adopted to get the operation performed. Surely this is one of the subjects to be embraced by the "relations of man to the universe," recommended by Professor Huxley in his programme of the education of children under the new School Board.

The perversity of vestries and boards of guardians has much to do with the present unvaccinated state of London. This epidemic has not come on London unanticipated The medical officers of health in many districts warned their vestries of the coming evil, but, unfortunately, from our wholly exceptional legislation in sanitary matters, the power of looking after small-pox and vaccination is given to the Poor Law Board. The vestries have therefore thrown on the guardians the burden of providing for an attack of small-pox; and in many instances little or nothing has been done, either in the way of looking up the unvaccinated, putting in force the vaccination laws, or treating the first cases of small-pox with those precautions which the fearful nature of the disease imperatively demands.

With a bundle of Acts of Parliament passed at different times with different objects, and giving authority variously to the Privy Council, the Poor Law Board, the Home Secretary, and other bodies, it is no wonder that our Government acts feebly in sanitary matters. There have been no vigorous attempts on the part of the Government or the Legislature to meet the present outburst of small-pox. If cows and sheep had been attacked, it is probable that something new would have been done. The old machinery has, it is true, been kept in motion. There is the Vaccination Act, which threatens every person with a fine who does not have his child vaccinated, but this law is regularly set at defiance. Besides this, even if boards of guardians and vestries are disposed to carry it out, they have no means of finding out unvaccinated children. There is, it is true, the Registration Act, but that Act does not make registration compulsory, and in some districts of London it has been stated that twenty-five per cent. of the population are not registered. There is no machinery yet set on foot to enable the inspectors of vaccination to lay hold of these children. Then there is the emigration of families from one parish to another. There is no plan of action for discovering these unvaccinated children in these families. It is vain for one parish to look after its vaccination, if other parishes do not. This is an Imperial question, and ought not to be left to Boards of Guardians.

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* Further facts of this kind are recorded in Dr. Ballard's Essay on Vaccination, 1868.

But what can we do at once, so as, if possible, to avert the further course of the epidemic? We can hardly hope, from past experience, that the epidemic will cease at present, if nothing further be done. The measures which a knowledge of the nature of the disease would suggest are as follows:

1. There should be a vigorous attempt made at once to secure the vaccination and re-vaccination of all persons who have not yet undergone these operations. It is of no use to wait till such persons seek this for themselves. They must be found out, and found out at once. At the rate at which inspectors are now finding out the unvaccinated, the epidemic will have spent its force, and little, if any, life will be saved. But if the Legislature would at once interfere, and insist on a house-to-house visitation with a body of men armed with vaccine virus, and ability and power to vaccinate, the whole of London might be visited, and every inhabitant inspected or vaccinated, in the course of the next month. These agents must be medical men or medical students, who should be paid so much a day for their trouble. It is this question of paying that constantly hampers vestries and boards of guardians. They would rather see any number of people sick than pay to prevent their sickness. The Government must do it, and do it at once, or it will not be done at all. A few hundreds of pounds will do it, and it will save thousands that the small-pox would cost.

2. Vigorous efforts should be made to stamp out each case of small-pox where it occurs. Every case of the disease should be reported to the sanitary authority of the district, on pain of fine and imprisonment. The person affected should be either removed to a hospital or isolated. If the latter, the isolation should be complete. Satisfactory evidence of the isolation should be given to the medical officer of health, and unless he is satisfied with the means taken, some method of punishing the erring parties should be devised. Contagious diseases of all kinds may be thus arrested. It is the difficulty of discovering the first cases that makes the spread of contagious disease so rapid and extensive.

3. Disinfection should be insisted on. This subject requires more thought and attention than it has yet received. All possible means by which the poison can be conveyed from one person to another should be prevented. The poison of small-pox retains its vivaciousness or reproductive power more tenaciously, apparently, than any other animal poison. It can be conveyed in clothes, paper, thread, string, everything that it is possible to use in the sick room. The doctor may take it to his patients, the lawyer to his clients, or the clergyman to his congregation if he has been visiting the sick. The Levitical laws against leprosy would be hardly too severe to prevent the spread of small-pox. Rules of the most stringent kind ought to be laid down for the guidance of nurses and all persons entering the sick room. Above all, in every district where small-pox prevails, there should be a disinfecting apparatus. This should be an oven not heated by gas, but by a stove. The oven should be long enough to receive beds and all kinds of bed-clothes and wearing apparel. These things should be conveyed to the store in a covered van, which could at once be placed in the oven without opening it to remove its contents. Filthy rags and beds of straw and shavings should be burned in the stove. Such an apparatus is at present at work in the parish of St. Giles. It should be forthwith erected in every district in London. Even when the small-pox has killed its utmost, such ovens will be useful for a future war with the demon of contagion in some other form.

Will some philanthropic member of the House of Commons draw up a Bill embracing these suggestions, and get it passed into law as quickly as possible, so as to save the lives of some thousands of our population, and the faces and purses of many thousands more?

E. LANKESTER

GÜNTHER'S CATALOGUE OF FISHES

Catalogue of Fishes in the British Museum. By Albert Günther, M.A., M.D., LL.D., F.R.S., F.Z.S., &c. Eight volumes. (London, 1859-1870.)

THE recent issue of the eighth and last volume of Dr. Gunther's Catalogue of Fishes brings to a completion one of the most laborious and important zoological works of the present epoch. For Dr. Gunther's Catalogue is not a mere catalogue in the ordinary sense of the word, but rather a more or less complete history of all the known members of the class of fishes. Not merely the higher divisions of the class, but the genera and species are all fully characterised, and to each species is appended a list of the specimens of it contained in the British Museum. References to other species, either doubtful or not yet acquired by the national collection, are also added. When it is stated that our national collection of fishes now contains 29,275 specimens, some idea may be formed of the labour that has been involved in naming, arranging, cataloguing, and describing such a vast mass of materials. Each of these specimens has to be carefully examined, in many cases internally as well as externally, and to be compared with its brethren of the same and allied species, before it can be satisfactorily determined. Let our readers go through this process in the case of one fish, and they will be able to form some sort of idea of the amount of toil involved in repeating this experiment some thirty thousand times over. Dr. Gunther has in fact expended about thirteen years of unremitting labour on this great work, and has had the good fortune to bring it to a felicitous conclusion. No general account of fishes has been published since Lacépede and Schneider's edition of Bloch about the beginning of the present century, as the celebrated "Histoire Naturelle des

Poissons" of Cuvier and Valenciennes, like too many other French publications, has never been finished.

When Dr. Gunther commenced his labours in 1859, his work was apparently intended to be confined to the Acanthopterygian order of fishes, and the volumes were entitled accordingly. On the completion of this great order with the third volume in 1861, the scope and title of the publication were extended so as to embrace the whole class of fishes. The fourth volume, issued in 1862, was devoted to the Pharyngognathi and Anacanthini of Müller, whose system Dr. Günther has generally followed. Here, however, he has made a slight change in the nomenclature used by the great German Anatomist, considering that the structure of the fins is of more importance than that of the pharyngeal bones, and therefore changing Müller's name, "Pharyngognathi acanthopteri" into "Acanthopterygii pharyngognathi.' '

In the fifth volume of his work, issued in 1864, Dr. Günther commenced the order Physostomi, treating first of the Siluroids and allied forms, which were formerly associated with the Salmonidæ; in the sixth, published two years later, the Salmonidæ themselves were handled. This group, Dr. Günther informs us, both on its own account and from the large amount of literature involved in its investigation, offers such great difficulties to the ichthyologist, that "as much patience and time are required for the investigation of a single species of it as in the case of other fishes for that of a whole family." The ordinary method followed by naturalists in distinguishing and determining species is here utterly inadequate; and Dr. Günther does not hesitate to assert that "no one, however experienced in the study of other families of fishes, will be able to find his way through this labyrinth of variations without long preliminary study, and without a good collection for constant comparison. Sometimes forms are met with so peculiarly and so constantly characterised, that no ichthyologist who has seen them will deny them specific rank; but in numerous other cases one is much tempted to ask whether we have not to deal with a family which, being one of the most recent creation, is composed of forms not yet specifically differentiated."

Dr. Günther's preliminary remarks (vol. vii. p. 3), before he commences the discussion of the true *Salmones*, well merit perusal by any naturalist engaged on the differentiation of species. In the Salmonidæ, characters such as the proportion of one part of the body to another, and the number of fin rays, which in other groups of fishes are generally employed for the separation of species, fail entirely, and another set of characters has to be relied upon. To add to the confusion, some of the species at least *interbreed*, and "it is probable, although at present not yet confirmed by direct observation, that such hybrids mix again with one of the parent species, thereby producing an offspring more or less similar to the pure breed." The difficulties thus added to the correct determination of the Salmonidæ, may be easily understood.

The seventh volume of Dr. Gunther's work, published in 1868, continues the history of the order Physostomi, and is devoted mainly to the extensive families Cyprinidæ and Clupeidæ, and to smaller groups nearly allied to them. In the eighth and last volume, published last year, Dr. Günther concludes the Physostomi with the eels and their allies, and then treats of the Lophobranchii and Plectognathi, which form the two last orders of the Teleostian subclass of fishes. The small subclass *Dipnoi*, embracing only the two Lepidosirens, comes next, and to them is appended a short notice of the recent discovery of the new Australian Mud-fish, which Mr. Krefft has referred to the Agassizian genus *Ceratodus*.* Dr. Gunther states his inability at that period to determine whether it "should be referred to the Dipnoans or to the Ganoids, or should form the type of a separate subclass." But it is well known that he has since received perfect examples of this wonderful fish, and has in preparation a memoir which will, no doubt, put at rest all questions upon its structure and its position in the natural series.

The Ganoids (of which, formerly multitudinous, subclass Dr. Günther only recognises *six* existing species) and the Chondropterygians, or Sharks and Rays, follow next in order, and the eighth volume concludes with the two undoubtedly lowest forms of the class of fishes—the Lampreys and the Lancelet, an *invertebrated* vertebrate.

The total number of specimens of fishes in the collection of the British Museum at the period of the close of this great work was, as we have already said, 29,275. These are referred by Dr. Günther to 5,177 species. Besides these, 1,666 other species are recognised as valid, of which the national collection has not yet obtained examples, and 1,682 more are referred to as doubtful. "Assuming, then," says Dr. Gunther, "that about one half of the latter will ultimately be admitted into the system, and that since the publication of the volumes of this work, about 1000 species have been described elsewhere, we may put the total "number of fishes at present known as about 9000."

In the preface to the last volume, Dr. Günther, besides giving us a general *résumé* of the extent of the collection under his charge, enters very fully into several other questions which are well worthy of attention, particularly at the present moment, when the relations between Government and Science are undergoing investigation by a Royal Commission. Whilst expeditions, fitted out by Austria, Prussia, and Italy, are despatched round the globe, accompanied by a staff of naturalists, and bringing back large collections of fishes to the national museums, our navy, it appears, is almost inert on this subject. Except from the Magellan Straits Surveying Expedition, to which Dr. Cunningham was attached as naturalist, no contribution from our Admiralty, which has so many ships always afloat, has reached the ichthyological department of the British Museum of late years. Yet it cannot be doubted that a very few words of encouragement from my Lords of the Admiralty would induce some of the many naval officers whose time must hang heavy on their hands at foreign stations, to turn their attention to collecting the common objects of the element on which they pass their lives. We assume, of course, that the expense of attaching a competent naturalist to any foreign expedition would be so great that "*my Lords would not feel justified in incurring it!*" Yet even the economical government of the United States thinks differently, and "each exploring American expedition was and still is accompanied by collectors, employed solely for the benefit of public museums."

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* See NATURE, vol. ii. p. 106.

We should also, did not space fail us, like to call special attention to Dr. Gunther's remarks on the importance of the study of the class of pikes as regards the elucidation of some of the most perplexing problems of Biology. "No other class of vertebrates," says our author, "is of equal importance to the geologist and palæontologist: the materials for comparing the living with past creations being so numerous and so diversified that we cannot help thinking that the relation of the various epochs to one another will be solved in the fields of ichthyology. Although fishes are mostly hidden by the elements in which they live, so that the knife of the anatomist generally first reveals new facts connected with their life, we have sufficient evidence to show that the phenomena of life are more varied in their different groups than in any other of the higher vertebrata, and that their study will form a solid basis for the solution of those general biological questions which, perhaps rather prematurely, agitate the minds of many zoologists."

OUR BOOK SHELF

On the Relations between Chemical Change, Heat, and Force. By the Rev. H. Highton, M.A.

WE should not have noticed this paper, though it has been sent to us for review, had it not been marked as "Reprinted from the *Quarterly Journal of Science*." This is its sole claim on our attention.

To put the contents in their simplest form before the reader, we may at once say that the Rev. Mr. Highton is a Perpetual-motionist. Not, perhaps, consciously—rather the reverse—but he belongs *in fact* to that singular class, though he would probably deny the charge with indignation. A short extract or two will, however, be sufficient to prove it to the satisfaction of any one acquainted with modern physics. Take the following:—

"Does there not, then, exist a power in nature for force to multiply force—even in the same way as life is multiplied by life through successive generations, and one living being may in due time become a thousand without losing its own vital energy?"

"We cannot . . . . produce heat without at the same time producing virtually in some shape or other an equivalent of cold."

"Cannot skill, mere skill, produce a less or greater disturbance and restoration of equilibrium, and so more or less force?"

If these extracts, which are perhaps not the richest which a careful search may discover, be not sufficient for the reader's amusement, we refer him to the original work. If they be not sufficient to prove to him the justice of our remarks, we refer him at once to some good scientific text-book, for he will have amply proved his need of instruction.

It would be an insult to our instructed readers to suppose that the fallacies of this paper require to be exposed *seriatim* for their benefit. *Ex uno disce omnes.* One will be given presently. But before giving it, we must strongly protest against the way in which many of the early, and some even of the later, discoveries of Joule are ignored throughout, while the attempts made to verify them by inferior experimenters, are put forth as original researches. When, however, Joule *does* happen to be referred to, the description of his experiments is wonderful indeed. Here is an example:

"He (Joule) churned various liquids in a calorimeter, and measured the increase of temperature. But in this kind of motion, as, perhaps, in all cases of friction, there is a pulling exertion of force, as well as one of pushing. Behind the arms of the paddle-wheel in the churn the liquid is pulled, and is pushed before them."

Comment on this sort of thing would be thrown away. When men like Helmholtz, Rankine, and Thomson vouch for the accuracy of a proposition, the world may well be indifferent to the criticisms of a Heath or a Highton. The grand founders of a rapidly progressing science cannot turn aside from their labours to answer frivolous objections. And it is strange and sad, indeed, that such excellent journals as the *Philosophical Magazine*, the *Quarterly Journal of Science*, and the *Chemical News* should diminish the space at their disposal for facts, by affording facilities for the dissemination of palpable nonsense and error.

As regards Electro-dynamics, Mr. Highton follows in the track of several better-known men, and is thus to a certain extent relatively excusable for his blunders, though they are quite as grave as those he commits with reference to the general theory of Conservation of Energy. The subject is by no means a very easy one, and it would certainly be somewhat hard to explain in a thoroughly popular manner the causes of his error. His difficulties, however (so far as we have had patience to investigate them), are such as have been met and overcome long ago by Joule, Faraday, and Thomson. (See, especially, Thomson, *British Association Reports*, 1852.) They are due, in great part, to his having confined his attention to the *zinc* alone of a galvanic cell.

Our Feathered Companions. Conversations of a Father with his Children about Sea birds, Song birds, and other feathered tribes that live in or visit the British Isles, their habits, &c. By the Rev. Thomas Jackson, M.A. (London: S. W. Partridge and Co.)

Dogs and their Doings. By the Rev. F. O. Morris, B.A. (London: S. W. Partridge and Co.)

THE first of the books of which we give the titles above, is one we cannot take up without pleasure, because of the memories that its numerous illustrations bring to those who are shut up amongst bricks and mortar, of some wild sea-shore, sweet and tender woodland, or moor with gorse and fern; all of which are the homes of birds who would rather be free from the companionship of man than seek it, and which we love all the more for their wild freedom. "Our Feathered Companions" is full of information about birds, telling many well-known things about their habits and lives, of which children will never tire as long as there are children. With regard to the children in this book, we wish our author had drawn them from nature; we do not often meet with little boys who quote Greek out of school, and should be sorry if little girls were always moralising about birds being useful to man; we cannot read the narrative without feeling that their lives must have some other purpose beyond this, and we are glad that our children should be reminded by some of the little poems in the book, that they should love birds for their beauty, and learn all they can about them, rather than kill them to gratify a selfish desire for possession. The pictures of sea-birds are almost all charming, and there are many more of our favourite birds which, with a few exceptions, are very good.

"Dogs and their Doings" appears to be rather a compilation of old stories of wonderfully wise and clever dogs than the result of fresh observation,—the most extraordinary is of one which could pronounce words, and of another which understood the use of money; there are others which show wonderful affection and faithful memory for years after death. The book will be intensely interesting to children who love animals, they will recognise the tricks and sagacity of some of their friends in the dogs described; for dogs who know Sunday, and show great evidence of memory and reasoning powers, are to be met with in almost every family where they are cherished and loved. Some of the numerous illustrations are by Harrison Weir, others after Landseer; they are very attractive, though few of them are new.

Both volumes are got up in that attractive style with which we are familiar from Mr. Partridge, and make very pretty books for presents to young people.

Class Book of Inorganic Chemistry. By D. Morris, B.A. (London: George Philip and Son, 1870.)

THIS text-book is specially designed for pupils preparing for the Oxford and Cambridge Middle-class examinations and for the matriculation examination of the University of London. How far it will answer its purpose will be seen best by the number of students who use it as their guide and succeed in passing such examinations. Written for a special purpose, it is exempt from the criticism to which a general text-book would be subject, and we shall therefore amply point out how the book fails, in our opinion, to accomplish its purpose.

Nothing is more important for a student than that the definitions he learns should be clear and precise; yet those in this work are almost uniformly bad. Absolute *weight*, for example, is defined as "the amount of weighable matter in a body," which might have been simplified by saying, "weight is weight." Again, *solution* is defined as the "perfect union of a solid with a fluid," thus making the hydration of caustic lime an example of solution, and excluding the solution of gases in liquids.

It would be unjust to the author to state which nomenclature he has adopted; he has shown his impartiality by adopting nearly every one proposed; "potass," "potash," "potassium hydrate," and "caustic potash," are used indifferently; "sulphate of potassium" and "dipotassic sulphate" are used as synonyms for the substance having the formula $HKSO_4$. But the most objectionable feature in the book is a certain looseness of expression, leading to positive errors, which is certain to perplex the student. Examples of this abound; we need only quote a few. The solution of ammonia in water, we are told, "has all the properties of the gas;" again, "strontia forms with water a hydrate which has all the properties of baryta water," and "lime-water has all the properties of solutions of potash and soda."

Several statements occur in the book which are so absurd that they can only be traced to careless revision; but although they will scarcely mislead the merest tyro in the study of chemistry, they are none the less objectionable. We thought it very absurd to read that "combustion in air and in oxygen is exactly the same thing," and we thought it more absurd to read that "ammonium and sodium are distinguished by the smell of ammonia on the addition of caustic potash," but we only arrived at the climax of absurdity when we read that "nitrogen increases the volume of the atmosphere; and in this way provision is made for winds and other things useful for man's well being." Surely after this the text-books will cease to tell us that nitrogen is a very inactive substance.

It is only fair to say that the latter portions of the book are tolerably free from such errors as we have noticed; and the chapters on the heavier metals are the best in the book. Questions selected from the examination papers of Oxford, Cambridge, and London Universities, and the Science and Art Department, are given in several places, and the book concludes with tables for the analysis of simple salts. It is doubtful if a work of the size and scope of the one before us was at all needed, when there are so many excellent small manuals; but if such a one is really required, it must be much more carefully compiled and edited before it can be either useful or instructive.

F. J.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Measurement of Mass and Force

"W. M. W." seems to regard units of mass and force as things which are to be talked and philosophised about, rather than to be actually used for the measurement of concrete qualities. It is true that the force of gravity on a standard pound at a specified locality is a definite unit of force; but instead of specifying a locality to which reference is by common consent to be made (and without which consent we shall have as many different units as there are localities), he says, "The assumption of a hypothetical force of gravity not dependent on latitude seems to stand on the same footing as the employment of a mean solar day."

The average length of the apparent solar day is the same at all places on the earth, and is called a mean solar day. I am curious to see "W. M. W.'s" definition of a hypothetical force of gravity not dependent on latitude. It appears to me to be such stuff as dreams are made of.

As your readers are probably nearly tired of this discussion, I propose to conclude it on my side by the following summary of the whole question:—

Mass and *quantity of matter* are generally identified, and were identified by Newton in the opening paragraph of the Principia; but I am disposed to agree with "W. M. W." so far as to allow that the comparison of quantity of dissimilar kinds of matter is to a certain extent conventional, though there is, perhaps, no other convention which has so strong a basis of reason. If it is the case that all kinds of matter consist of one elementary material differently arranged, this convention must of necessity be no longer a convention, but a simple statement of fact, and portions of dissimilar matter which have equal masses, whether as measured by the inertia test or the gravitation test, consist of equal quantities of the elementary material. The rigorous agreement which is known to hold between these two tests, as applied to the comparison of different kinds of matter, may even be quoted as an argument for the existence of such a common material.

Whatever we may think of this convention or assumption (call it which we will), the phrase *a pound of matter* is legitimate and involves no assumption. A pound of sugar is a pound of matter, and a pound of lead is a pound of matter, independent of all hypothesis.

All authorities are agreed that, in treating kinetical problems, it is expedient to express masses and forces in such units as to satisfy the relation.

$$\text{Force} = \text{Mass} \times \text{Acceleration},$$

which, for a body, falling freely in vacuo, becomes

$$\text{Gravitating force} = \text{Mass} \times g.$$

Now comes the point of divergence, the main question being this: shall we (I.) measure force by reference to a standard of mass, or shall we (II.) measure mass by reference to a certain (or uncertain) standard of force. I showed in my last letter (p. 228) that the pound is practically, in going from place to place, a standard of mass, not of force, and that masses in different localities can be more directly compared than forces. It is surely reasonable to measure force by reference to a universally accessible standard of mass, rather than to measure mass by reference to a standard of force which, on the earth generally, is difficult of identification. I therefore uphold the following system:—

I. Take a pound of matter as the unit of mass; from which it follows that the gravitating force of a pound of matter has the numerical value g, and the unit of force is $\frac{1}{g}$ of the force with which a pound of matter gravitates.

This system was first proposed by Gauss, for the comparison of magnetic forces in different parts of the earth. It is the system which has been always taught by Sir William Thomson, and is adopted in Thomson and Tait's Natural Philosophy, Tait and Steele's Dynamics (except the first edition), the Reports of the Brit. Assoc. Committee on Electrical Standards (see especially the Report for 1863), Balfour Stewart's Elementary Physics, and the later editions of Atkinson's Ganot.

If we adopt the other alternative, we are driven to choose between certain subordinate alternatives, of which, with "W. M. W.'s" help, I may enumerate three.

II. (1.) Localise your pound of force by specifying the standard locality of reference where the imperial pound gravitates with unit force. For this unit to be generally adopted, there must be general agreement as to the locality of reference, or what amounts nearly to the same thing, general agreement as to a definite reference-value of g. If we make this 32·2, the unit of force, at a locality where g has any other value, will be $\frac{32.2}{g}$ of the local gravitating force of the imperial pound, and the unit of mass will be the mass of 32·2 imperial pounds, so that the units will be a definite multiple of those employed in I.

This system is sound because (though in a roundabout way) it acknowledges the pound as a universal standard of mass, which is everywhere to be denoted by the same number, while it denotes the gravitating forces of pounds in different latitudes by different numbers.

If we take as unit of force the gravitating force of half an ounce at a place where g is 32, we obtain a clumsy definition of the Gaussian unit of force, and our unit of mass becomes the pound.

(2.) File or load your pounds so as to make a pound at the pole gravitate with the same force as another and larger pound at the equator. This is the only way of making the pound a direct *standard of force* to the inhabitants of the world generally. The unit of mass will then be g times the mass of one of these filed or loaded pounds, and will everywhere represent the same mass.

(3.) Let every man adopt the local gravitating force of an imperial pound at the place where he happens to be, as his unit of force, and the mass of g imperial pounds as his unit of mass. This system gives a rough and ready unit of force, which is frequently adopted for rough purposes by all physicists; but experimental results stated in terms of it must be accompanied by a statement of the local value of g, to make them comparable with those obtained at other places. In dealing with masses in cases where forces are merely subsidiary, as in buying and selling goods, no one would recommend the adoption of the unit of mass which this system gives. In cases where forces and masses have to be considered in conjunction, this system has no advantage over I. in point of simplicity, and has the disadvantage of requiring us to express both forces and masses in terms of changeable units which tend to confusion of ideas. It is altogether inapplicable to astronomy, and is not even competent to express the mass of the earth; for it would make the mass of a cubic foot of matter at the earth's centre many millions of times greater than the mass of a cubic foot of gold or platinum at the earth's surface.

Writers who, without special explanation, express forces in pounds or grammes, and say that the mass of a body is numerically equal to its weight divided by g, must be classed as adopting this system; for though such expressions are ambiguous in themselves, this is the sense in which they will usually be received and applied. It is indeed the system which was almost universally taught until the publication of Thomson and Tait's Natural Philosophy.

I am not quite clear as to the particular system which "W. M. W." elects to adopt. He began (p. 145) by siding with Deschanel, who seems to adopt II. (3). In answer to my first letter (p. 167), he stated (p. 187) that "if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound." If this be not an adoption of II. (2), it amounts to saying that at the North Pole a pound does not weigh a pound. In the second sentence of his last letter he adopts II. (1) without, however, distinctly committing himself to a definite locality of reference; and as long as this point is left open, every man will make the locality where he happens to be the locality of reference, so that II. (1) in this indefinite form degenerates into II. (3). In the same letter he says, "As a philosophical theory, I am perfectly ready to admit that the standard pound is most appropriately considered as a standard of mass, but the employment of this standard in a text-book for the use of beginners seems calculated to lead to confusion." It rather appears to me that the refusal to accept this real standard of mass leads usually to a confused mixture of the systems II. (1), II. (2), II. (3); and I have shown in this letter that II. (1), which is the best of the three, does, in fact, make the pound a standard of mass.

I would earnestly commend to all teachers of dynamics the practice of Sir W. Thomson in strictly abstaining from the use of the word *weight* in all definitions and specifications relating to mass and force, as its ambiguous use does more than anything else to confuse these subjects. The weight of a body may and most frequently does mean its mass stated in pounds, or it may mean the force with which it gravitates. In the one sense its weight is the same whatever place it is carried to; in the other sense it varies from place to place.

I would also recommend for imitation Sir W. Thomson's practice of discarding the term *accelerating force*, which has been used to denote what ought to be called *force per unit of mass* or *intensity of force*, as distinguished from amount of force. Forces should always be expressed in terms of comparable units. As long as the learned recognise two non-comparable measures of force, they can hardly blame the unlearned for recognising a third and confounding force with energy.

The phrase *absolute force* (of a centre) is for the same reason objectionable. *Strength* of a centre is a better designation, and magneticians already speak in this sense of the strength of a magnetic pole. It would be a great advantage to have a short and handy name for some unit of force properly so called, and I venture to propose that the unit of force defined in I. be called a *kinit*. It may be formally defined as *that force which, acting on an avoirdupois pound of matter for a second, generates a velocity of a foot per second.* If we substitute gramme for pound, and metre for foot, we obtain a different unit which must be called by a different name, and of which 138¼ make one kinit. This numerical relation remains true, even if one of the forces compared be at the earth and the other at the moon. If any etymologist objects to making a word derived from Greek end in *it*, he may adopt the convenient fiction that it is an abbreviation of *kinetic unit.*

J. D. EVERETT

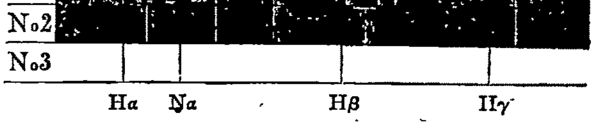
Belfast

The Spectrum of the Aurora

IN the *Philosophical Magazine* for February there is a paper by F. Zollner, on the Aurora Spectrum, in which he points out that, since the light of the aurora is as faint as that of the faintest vacuum-tube capable of being spectroscopically examined, while the mass of incandescent gas is almost infinitely greater, its temperature must be exceedingly low comparatively. He therefore supposes "that the spectrum of the aurora borealis does not correspond with any known spectrum of the atmospheric gases, only because, though a spectrum of our atmosphere, it is one of another order, and one which we cannot yet produce artificially."

I do not know if any other observer has noted the apparent coincidence in position of several of the auroral bands with those of a spectrum which I have occasionally obtained from air at low pressure, and with a feeble discharge. It is sometimes exhibited with great brilliancy by ordinary "lumière" tubes, and is, I believe, in part at least, the spectrum described by Wüllner (*Phil. Mag.*, June 1869), as a new spectrum of oxygen. I have certainly obtained it very vividly in pure electrolysed oxygen, with a feeble discharge; but some perplexing observations make me rather doubtful of its origin.

In the annexed sketch No. 1 represents the spectrum above mentioned, No. 2 that of the aurora, while No. 3 gives the lines of H and Nα for comparison.



I first noticed the coincidence of the yellowish-green line at 41 with the principal line of the aurora in Jan. 1870, and since that time have frequently repeated the observation; once with a spectroscope with a 60° bisulphide prism, and magnifying power of about six times. With this instrument the lines of both spectra appeared nebulous, but perfectly coincident. When the light from the vacuum tube is strong, this band appears shaded off gradually on the most refrangible side, but when it is no brighter than the aurora this is not noticeable.

The faint lines shown in the sketch were never bright enough to be compared with the same accuracy as the bright line, but repeated observations convince me that the positions given cannot be far wrong. Their relative brightness is very variable.

The red line, which was so brilliant in the auroral displays of last October, is very rarely visible, and does not coincide with any line which I have observed. The first red line of the tube spectrum is $H\alpha$, due to a trace of residual moisture.

I think it best to publish these imperfect results without more delay, in the hope that they may be corrected or confirmed by other observers, as I am unable at present to continue the research.

HENRY R. PROCTER

Royal College of Chemistry, Feb. 7

Resemblances of Plants *inter se*

MR. BENNETT, in his very interesting review of Mr. Mivart's "Genesis of Species" (NATURE, No. 66), refers to the close resemblance of an African Euphorbia and a South American Cactus" as an instance of "imitation," and to "the extraordinary resemblance of certain Conifers to flowerless plants" as "opposed to the theory of Natural Selection."

Neither example seems to be well chosen. In every case of supposed "mimicry" or "imitation," the question first arises, whether the resemblance between different organisms be or be not referable to similar conditions of life. Nobody will think a dolphin "imitates" a fish, nor does a climbing Tamus "imitate" a Humulus. These are cases of similar adaptations, but wholly unconnected with anything like mimicry in its true sense. An aquatic mode of life, or climbing habits, will necessitate certain peculiarities of structure, and the influence of an epidermis precluding evaporation will be the same in a Euphorbia as in a Cactus. The consecutive similarities of detail may without straining be referred to Mr. Darwin's "Correlations of Development."

There is still less cause for wonder in the resemblance of Conifers to flowerless plants, since it has been long ago shown by Hofmeister ("Vergleichende Untersuchungen," 1851, cf. Sachs, Lehrbuch der Botanik, 1868, pp. 310 and 384) that there is a close affinity between Lycopodiaceæ and Conifers, in spite of the chief boundary line of our system running between them. The resemblances of these respective orders therefore are scarcely more "extraordinary" than the resemblance of a Juncus to a grass, nor can I comprehend their bearing against the theory of Natural Selection.

Mr. Bennett deals rather strongly with botanists in denying to them, with rare exceptions, "a philosophic spirit." I cannot think this severe criticism to be applicable to the labours of De Bary, Hofmeister, Sachs, Nägeli, Schwendener, Pringsheim, and other contemporaries of ours (confining myself to my own country), labours alike distinguished by comprehensiveness of generalisation and accuracy of detail.

Did Mr. Bentham really come over to "the evolutionists" "only within the last twelve months"? I should consider that he expressed his full assent to the ideas of Mr. Darwin already in his addresses of 1868 and 1869.

D. W.

Frankfort-on-the-Main, Feb. 21

The Genesis of Species

IF this note should meet the eye of the writer on Darwinism in the *North British Review* for June 1867, I should feel greatly obliged if he would explain the following passage, quoted by Mr. Mivart in p. 57 of his "Genesis of Species":—"The advantage is utterly outbalanced by numerical inferiority. A million creatures are born; ten thousand survive to produce offspring. One of the million has twice as good a chance as any other of surviving; but the chances are fifty to one against the gifted individual being one of the hundred survivors."

Is it an assumption or the statement of a fact that "one of the million has twice as good a chance as any other of surviving?" and how are "the hundred survivors" arrived at?

STUDENS

Fertilisation of the Hazel

IN NATURE for April 7, 1870 (vol. i. p. 583), Mr. Marcus Hartog stated, as the result of his observations, that the male catkins and female flowers of the hazel are not simultaneously developed on the same twig, and that therefore a kind of quasi-cross-fertilisation necessarily takes place. Although convinced at the time that my observation did not tally with Mr. Hartog's, it was then too late in the season to submit my impression to a practical test. During the past week I have closely observed the hazel bushes in flower, and have found on every bush which has come under my notice, the female and male flowers in a perfect state of development on the same ultimate twigs, in close proximity to one another, the stigmas being frequently loaded with pollen-grains, apparently from the neighbouring catkin; at all events there appears no provision of nature specially to promote fertilisation from other bushes. We see in fact here a confirmation of the general law suggested in my paper in the first number of NATURE, on "The Fertilisation of Winter Flowering Plants," that when plants flower in the depth of winter, and at a time when no or few insects are about, self-fertilisation is the rule rather than the exception, or in the case of unisexual flowers, as near an approach to self-fertilisation as is possible under the circumstances.

ALFRED W. BENNETT

Feb. 27

Sanitary Tests

DR. LANKESTER recently pointed out in NATURE that the existence and spread of fever were mainly owing to popular ignorance and the neglect of physiological laws. Since the article referred to was written, a greater plague has followed the fever then prevailing; and we are now told that Ireland is much better off than England—vaccination being there almost universal.

This state of things seems to me but a symptom of a more deeply-rooted evil, viz., the general neglect of physiological and sanitary science among the *higher* orders as well as the lower.

In how many large schools are the laws affecting the human body and the relation of man's frame to the destructive and constructive elements which surround us systematically taught? Are there any?

We talk of the gross ignorance and filthy habits of the poor; but both their ignorance and their filthiness are often a part of the very conditions of their existence. Have *we* been faithful stewards in this great matter?

Allow me to draw attention to the fact that, though Government has greatly facilitated and cheapened the acquisition of other branches of science, hygiene seems entirely omitted from the programme. But knowledge must descend from the higher to the lower levels. Ought not the heads of all public schools to undergo a compulsory examination in this subject? I could adduce instances where the greatest mischief has resulted from ignorance in such high quarters.

Aigburth, Liverpool, Feb. 25 SAMUEL BARBER

Morell's Geometry

IN my ignorance of the fact that there are two Morells of very different calibres in the literary world, I find, to my great regret, that I have done serious injustice to the "widely-known" writer "on philosophy and grammar" and scholarly Mr. J. *D.* Morell, by attributing to him the authorship of the work on the "Essentials of Geometry," whose appearance I was compelled to notice so unfavourably in last week's NATURE. Pray give immediate publicity to this confession of error on the part of

March 1 THE REVIEWER

Algaroba

AT p. 313 of NATURE (February 16) is a paragraph on the use of the Algaroba in the province of Catamarca, Argentine Republic, in which it appears to me the writer has confounded two or more plants. Algaroba is a name applied to *Ceratonia siliqua*, to several species of *Prosopis* in South America, and to *Hymenæa Courbaril* in Panama. In Brazil the last-named plant in called Jatai, from which, I presume, the writer of the paragraph in question has obtained his "*Hymenæa Courbaril-Jetaiba*." The sweet fleshy pods of *Prosopis dulcis*, a tree widely spread over Southern and Central America, are used for feeding cattle, and several other species are employed for a like purpose in different parts of the tropics; it is therefore more than probable that *P. dulcis* is meant instead of *Hymenæa Courbaril*. Moreover, the pods of this latter are very thick and woody, and would not be easily "pounded in a wooden mortar;" and the tree cannot be well described as growing "to a height of forty feet, with wide-spread branches, and a rather slender stem," when we know that it frequently attains a hundred feet in height, and sixty feet in circumference.

JOHN R. JACKSON

Kew, Feb. 25

The Exeter Museum

THE paragraph in NATURE of the 23rd ult., respecting the Royal Albert Museum at Exeter, has excited my curiosity. Will you be so good as to mention an instance or two in proof that the museum "has done much towards attracting attention to the value of scientific knowledge in the West of England?"

It is difficult for a stranger to see how the case of the Plympton Grammar School can be ascribed to its influence.

Feb. 25 INQUIRER

Aurora Australis

[Extract from a letter received from Captain H. P. Wright, Ship *Gasfarth*]

Madras, Dec. 5.

I ENCLOSE you an account of the Aurora Australis as we saw it in the South, and I might also state that Mr. Pogson (Madras Observatory) says that the magnetic disturbance in these two days, 14th and 25th October, was so great that his instruments would not register the amount.

(Signed) H. P. WRIGHT

"October 24, 1870.—New moon at 6 P.M., lat. 42° S., long. 39° E.; at 7.30, as the twilight began to fade in the sky, we observed a bright rosy light at first resembling the reflection from a very red fire in the southern heavens. It extended from W. to E., and was visible from 8° to 50° high, being brightest at about 35° or 40°. Bright stars of the first and second magnitude shone through it. This cloud of crimson light had nearly all faded away by 9 P.M., first in the south-western direction, and so on gradually to the south-eastward. It may have been blown along by the wind, which was N.W. by W., but I did not think so. Other light clouds were passing; the sky below was its usual colour, and the stars shining very low down. As soon as this had passed away, there came a yellowish white, or milky white, light in the southern sky, and, as it were, taking the place of the crimson light. I should guess it to be about equal to $\frac{1}{4}$ of the moon's light, and showing a little bank of clouds of a dark-grey colour some 4° or 5° above the horizon underneath. This continued until 10.40 P.M., when it suddenly assumed a grander appearance. There was one long line of the brightest crimson some 8° or 9° broad, reaching up from south towards north, and some 70° high, fading into the normal colour of sky; this rose up a little to the west of the Southern Cross, on from this to the eastward was a great cloud or clouds of this bright crimson light, the bright star Canopus &c. showing through with a deep yellow light, and, passing over all, cumulus clouds carried somewhat quickly by the brisk breeze then blowing. To give, perhaps, a better estimate of the yellowish-white light, we could as long as it lasted only see the stars in the Southern Cross indistinctly. By midnight, or a little after, it all passed away, and we had lightning to the S.W. in the middle watch. The following night was very rainy, but the strong crimson and white light could still be discerned."

Aurora by Daylight

IN NATURE of Dec. 15, a correspondent asks the question, "Can Aurora be seen in daylight?" I answer, yes, beyond a doubt. In the autumn of last year (I cannot give the date nearer than that it was early in Ooctober) my eye was attracted by an unusual motion, in what at the first glance appeared to be a light fleecy cloud, but was in reality a broad ribbon of Aurora of a yellowish white colour, which changed its form and position with the peculiar streaming motion of the Aurora, sometimes almost fading entirely and again recovering its comparative distinctness.

It was about four o'clock in the afternoon when my attention was drawn to it, and I watched until late in the evening, and saw it as the dusk came on, supported by fainter streamers of light, which stole out as the darkness increased, and almost imperceptibly grew into one of those magnificent auroral displays so frequently seen here.

The Aurora, as I first saw it, was about N.W. by N., and I should say 30° above the horizon, and the sky was beautifully clear and free from clouds.

Will any of your correspondents inform me if the intensity of auroral light, as proved by its visibility in daylight, teaches us anything more than is at present known of the Aurora? And I should be much obliged to anyone who will inform me if the spectrum of lightning has ever been obtained, and if so, how it compares with the spectrum of the Aurora.

W. G. THOMPSON

Matapediac, Province of Quebec, Feb. 4

Tigers at Bay

IN NATURE for Feb. 2, p. 275, a doubt is expressed as to whether a tiger when in danger will ever take to a tree. An anecdote related in vol. 2, p. 112 of De Beauvoir's "Voyage Round the World," seems to settle the question:—"Attacked and conquered by the buffalo, the tiger bounded some thirty feet into the air into a cocoa-nut tree. Some twenty natives were in an elevated position amongst the branches of this tree; in one and the same moment they let themselves fall like ripe fruit from a tree that is shaken." G. E. D.

Furzewell House, Torquay

Dr. Donkin's Natural History of the Diatomaceæ

THE reviewer of the above-named work (see NATURE, vol. iii., p. 210) describes the plates as inferior to those in the Synopsis. With this opinion I fancy many will differ; as correct representations of the species described, they are far in advance of those in the Synopsis, for example, compare the figure of *Navicula tumens* in the latter work with *Navicula rostrata* in Dr. Donkin's; with the exception of the outline, the figure in the Synopsis does not resemble that species, and is inferior to Ehrenberg's in the Microgeologie. Also compare *N. Hebes*, *N. palpebralis*, *N. subsalina*, *N. latiuscula*, *N. alpina*, with the corresponding forms in the Synopsis, and I think the superior fidelity of Dr. Donkin's illustrations will be conceded.

Many of the forms in the Synopsis must have been drawn from memory, they are so glaringly inaccurate, *e.g.*, *Amphipleura pellucida* is represented with marginal punctæ, *Nitzschia bilobata* with indistinct distant striæ. The marginal dots on *A. pellucida* existed only in the delineator's imagination; and, as every student of the Diatomaceæ knows, *N. bilobata* has close but distinct striæ.

I agree with "W." that the synonymy might have been more extended. It is, however, next to impossible to identify from figures or descriptions the forms intended by the early observers.

The desirability of giving habitats in full is questionable; three or four localities are sufficient, as with very few exceptions the same species would be found (the localities being similar) in any part of the United Kingdom.

In conclusion, I would remark that the following species are not Ehrenberg's, as stated by "W.," but Gregory's:—*Navicula Smithii* var. *fusca*; *N. Smithii* var. *suborbicularis*; *N. Smithii* var. *nitescens*, and *N. latissima*.

I know the works of Ehrenberg, Kutzing, Rabenhorst, Grunow, and Greville, but who is Cleeve? K.

PROPOSED OBSERVATIONS OF VENUS

THE following circular has just been issued by the Observing Astronomical Society:—

The committee of the society have decided to undertake a series of systematic observations of the planet Venus, during one complete revolution, for the purpose of obtaining results that shall lead to our becoming better acquainted with the markings which are visible on her surface, and a correct knowledge of their form and permanency.

In common with other observers it has been to them a matter of regret that although this beautiful object approaches nearer to us than does any other member of the solar system (our satellite excepted), yet that our knowledge of its superficial condition should be far less than of those planets less favourably situate. In most astronomical works the information concerning Venus is very meagre, whilst the drawings of her appearance exhibit, in the majority of cases, merely a blank crescent.

Yet, in turning to the ancient observations made of this planet, the committee have been struck by the large number recorded, many exhibiting well-defined markings, and when they considered the numerous observations of the same character made of late years, including several important ones from members of the society, it seemed evident that observation of this planet was not so difficult as is generally represented. It was seen further that if a proper discussion and analysis of all recorded observations were made, the result might be a large addition to our knowledge of the planet's surface.

The Committee, therefore, in inaugurating this important movement, divide the work to be done into three branches:—

1. The formation of a sub-committee of astronomical observers (including non-members of the society) for the purpose of continually observing Venus during one complete synodical revolution.
2. The collection of all ancient observations and drawings of the planet.
3. The collection of as much modern data as possible from existing observations, and from public and private records.

At the conclusion of the observations of the sub-committee, the results obtained, together with the ancient and modern observations collected, will be placed in the hands of a competent astronomer for complete analysis and discussion, when the results obtained will be published.

Those observers who are willing to join the "Venus Observation Sub-Committee" are requested to send their names and addresses to the hon. secretary of the Society, Ashley Road, Bristol, before March 10, stating the aperture and power of the instrument they intend to employ.

The observations will commence on March 20, previous to which a circular, containing full instructions, will be issued to every observer who has expressed his willingness to assist in the project.

PROFESSOR DE NOTARIS AND HIS NEW WORK ON MOSSES

In the year 1838 Professor De Notaris, of the Universities of Turin and Genoa, published a Syllabus Muscorum, which he now calls *Un lavoro giovanile* (a juvenile work). Still it is a very useful manual for the young Italian bryologist, for whom it was first written, and to whom it gives not only the exact characters of each plant, but also their localities and the synonymy. From that time, year after year, he went on increasing and improving his work, till he had gathered together sufficient materials for a new work. It was not, however, till last year that he succeeded in persuading the bountiful municipality of Genoa to take his MS. in hand and publish it, which they effectually did at their own cost some months ago. The simple title of De Notaris' work[*] was mentioned by one English scientific periodical; but, except Dr. Braithwaite, the well-known contributor of the "Recent Additions to our Moss Flora," and Mr. C. J. Smith, who recently published a "Moss Flora of Sussex," accompanied by a clear and really instructive paper on the structure and reproduction of mosses,[†] I have hardly heard of any other English bryologist taking account of the newly edited *Atti della Università di Genoa*, the first of which, in large quarto, is entirely taken up by Professor De Notaris' standard work.

This painstaking and well-digested composition represents, as it were, the *résumé*, or, we might say, the quintessence of the long and seldom interrupted labours of the veteran bryologist, who, far from rejecting or disregarding the improvements that bryology has recently made, is quite willing to adopt them wherever he finds them of practical value, but not when, instead of the promised gold, they only give glittering or useless tinsel.

"I really find myself"—thus he writes in his preliminary comments—"in a sort of quandary (*specie di peritanza*) whenever I have to produce any ulterior, though obvious, re-adjustment, which might probably be attributed to some deplorable mania of upsetting and overthrowing the monumental edifice of the European bryology, or even to an ungracious instinct (*parmaloso istinto*) of seizing upon and appropriating the works of others. For, after all, we must acknowledge that the *Bryologia Europæa* (of Schimpfer) is the bible of every student of mosses."

Whilst admitting that any very small and apparently insignificant character may at times be useful to establish a natural group, De Notaris believes that such differential characters occasionally assumed, to complete the diagnosis of some genus or species, cannot be taken as absolute and invariable, and therefore he prefers to stick to the old Linnean canon: *Quæ in uno genere ad genus stabiliendum valent, minime idem in alio genere præstant.*

Thus pointing out the different forms of cells in the epicarp of *Bryum cæspitosum* and *B. erythrocarpum*, and seeing that it is not in the power of anyone to examine all the capsular membranes of each individual plant of these two species, he says: "We must not wonder if now and then an exception to the ruling character of the cells is to be found," observing, at the same time, that a pretty good character for distinguishing certain natural groups might also be found in the specific conditions of the *endochrome*. Another striking remark on this subject he makes, where he says that "the epithet *pachydermic*, applied to the cellule of the epicarps, might easily be a source of error, owing to either a stronger or weaker cohesion of the chromoplasm, in which case the cellules might well be pachydermic, and flocculently membranaceous at the same time."

He further observes upon the variable characters of the inflorescence that "*Mnium medium* is found both monœcious and polygamous, as well as *Bryum torquens* and *Bryum pendulum*. *Catoscopium* is not seldom to be found monœcious as well as diœcious. Even that form, he says, which I have distinguished under the name of *Bryum bimoideum*, may turn out at some time to be merely a diœcious form of *B. bimum*."

The *peristome*, in his view, is but an exudation or, as it were, a hypertrophy in the evolution of the capsule, not an indispensable means for the ripening of the sporules. Hence the reason why he considers it, morphologically speaking, as an apparatus of secondary importance. Thus there are specimens of *Hypnum stellatum*, which bear their ordinary normal capsules, whilst there are others whose capsules are like those of the apothecia, with imperfect and nearly Leskeaseous peristoms.

Other important reflections are to be found in the same "Comments," such as the one relating to the *perichætial* leaves, which serve to distinguish a *Limnobium* from a *Hypnum* better than any other distinction taken from the habit of the stem or the arrangement of the leaves; and the other on the topographical distribution of mosses in Italy, where the great variegation of the "Bryological mantle" mainly depends upon the various constitution, elevation, or undulation of the soil; the heights and the valleys, naked, or well timbered and green with copse and grove, variously contributing to their infinitely varied diversifications.

Among the vagrant species of all regions he points out particularly the *Bryum alpinum*, which, "from the uppermost boundaries of the Alpine zone, spreads itself down to the very brackish pastures that scarcely raise themselves above the sea level in the islets of the Strait of Bonifacio." Again, that gem of mosses *Schistostega osmundacea* (Bridel's *Catoptridium smaragdinum*) which has been till late a *desideratum* in the Italian flora, De Notaris informs us he found himself whilst, in 1865, he was crossing the Morgan on his way to Mont Rosa.

J. GAGLIARDI

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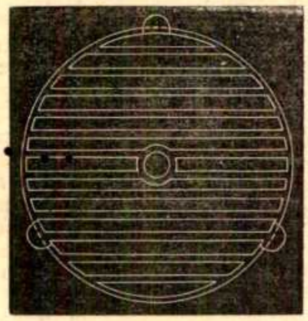
[*] "Epilogo della Briologia Italiana."

[†] "I have seen," writes Mr. Smith, "your friend's *Epilogo*, and am much struck with it. What a fine work, and brought out at the cost of the Municipality! When will such things occur in England?"

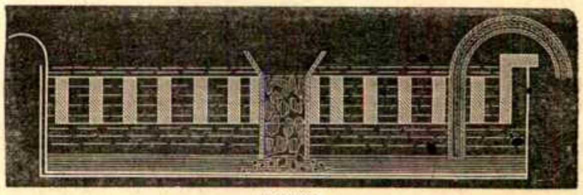
A CONSTANT FORM OF DANIELL'S BATTERY*

GRAHAM'S discovery of the extreme slowness with which one liquid diffuses into another, and Fick's mathematical theory of diffusion, cannot fail to suggest that diffusion alone, without intervention of a porous cell or membrane, might be advantageously used for keeping the two liquids of a Daniell's battery separate. Hitherto, however, no galvanic element without some form of porous cell, membrane, or other porous solid for separator, has been found satisfactory in practice.

The first idea of dispensing with a porous cell, and keeping the two liquids separate by gravity, is due to Mr. C. F. Varley, who proposed to put the copper-plate in the bottom of a jar; resting on it a saturated solution of sulphate of copper; resting on this a less dense solution of sulphate of zinc; and immersed in the sulphate of zinc, the metal zinc-plate fixed near the top of the jar. But he tells me that batteries on this plan, called "gravity batteries," were carefully tried in the late Electric and International Telegraph Company's establishments, and found wanting in economy. The waste of zinc and of sulphate of copper was found to be more in them than in the ordinary porous-cell batteries. Daniell's batteries without porous cells have also been tried in France, and found unsatisfactory on account of the too free access of sulphate of copper to the zinc, which they permit. Still, Graham's and Fick's measurements leave no room to doubt but that the access of sulphate of copper to the zinc would be much less rapid if by true diffusion alone, than it cannot but be in any form of porous-cell battery with vertical plates of copper and zinc opposed to one another, as are the ordinary telegraphic Daniell's batteries which Mr. Varley finds superior to his own "gravity battery." The comparative failure of the latter, therefore, must have arisen from mixing by currents of the liquids. All that seems necessary, therefore, to make the gravity battery much superior instead of somewhat inferior to the porous-cell battery, is to secure that the lower part of the liquid shall always remain denser than the upper part. In seeking how to realise this condition, it first occurred to me to take advantage of the fact that saturated solution of sulphate of zinc is much denser than saturated solution of sulphate of copper. It seems[†] that, at 15° temperature, saturated aqueous solution of sulphate of copper is of 1·186 sp. gr., and contains in every 100 parts of water 33·1 parts of the crystalline salt; and that at 15° the saturated solution of sulphate of zinc is of sp. gr. 1·44, and contains in every 100 parts of water 140·5 parts of sulphate of zinc, both results being from Michel and Krafft's experiments.[‡] Hence I made an element with the zinc below; next it saturated solution of sulphate of zinc, gradually diminishing to half strength through a few centimetres upwards; saturated sulphate of copper resting on this; and the copper-plate fixed above in the sulphate of copper solution. In the beginning, and for some time after, it is clear that the sulphate of copper can have no access to the zinc otherwise than by true diffusion. I have found this anticipation thoroughly realised in trials continued for several weeks; but the ultimate fate of such a battery is that the sulphate of zinc must penetrate through the whole liquid, and then it will be impossible to keep sulphate of copper separate in the upper part, because saturated solution of sulphate of zinc certainly becomes denser on the introduction of sulphate of copper to it. To escape this chaotic termination, I have introduced a siphon of glass with a piece of cotton wick along its length inside it, so placed as to draw off liquor very gradually from a level somewhat nearer the copper than the zinc; and a glass funnel also provided with a core of cotton wick, by which water semisaturated with sulphate of zinc may be continually introduced at a somewhat lower level. A galvanic element thus arranged will undoubtedly continue remarkably constant for many months; but it has one defect, which prevents me from expecting permanence for years. The zinc being below, must sooner or later, according to the less or greater vertical dimensions of the cell, become covered with precipitated copper from the sulphate of copper which finds its way (however slowly) to the zinc. On the other hand, if the zinc be above, the greater part of the deposited copper falls off incoherently from the zinc through the liquid to the copper below, where it does no mischief, provided always that the zinc be not amalgamated, —a most important condition for permanent batteries, pointed out to me many years ago by Mr. Varley. Placing the zinc above has also the great practical advantage, that, even when after a very long time it becomes so much coated with metallic copper as to seriously injure the electrical effect, it may be removed, cleaned, and replaced, without otherwise disturbing the cell; whereas if the zinc be below, it cannot be cleaned without emptying the cell and mixing the solutions, which will entail a renewal of fresh separate solutions in setting up the cell again. I have therefore planned the following form of element, which cannot but last until the zinc is eaten away so much as to fall to pieces, and which must, I think, as long as it lasts, have a very satisfactory degree of constancy.



GROUND PLAN OF NEW BATTERY



SECTION

The cell is of glass, in order that the condition of the solutions and metals which it contains may be easily seen at any time. It is simply a cylindrical or rectangular jar with a flat bottom. It need not be more than ten centimetres deep; but it may be much deeper, with advantage in respect to permanence and ease of management, when very small internal resistance is not desired. A disc of thin sheet copper is laid at its bottom. A properly shaped mass of zinc is supported in the upper part of the jar. A glass tube (which for brevity will be called the charging-tube) of a centimetre or more internal diameter, ending in a wide saucer or funnel above, passes through the centre of the zinc, and is supported so as to rest with its lower open end about a centimetre above the copper. A glass siphon with cotton-wick core is placed so as to draw liquid gradually from a level about a centimetre and a half above the copper. The jar is then filled with semisaturated sulphate of zinc solution. A copper wire or stout ribbon of copper coated with india-rubber or gutta percha passes vertically down through the liquid to the copper plate below, to which it is riveted or soldered to secure metallic communication. Another suitable electrode is kept in metallic communication with the zinc above. To put the cell in action, fragments of sulphate of copper, small enough to fall down through the charging tube, are placed in the funnel above. In the course of a very short time the whole liquid below the lower end of the charging tube becomes saturated with sulphate of copper, and the cell is ready for use. It may be kept always ready by occasionally (once a week for instance) pouring in enough of fresh water, or of water quarter saturated with sulphate of zinc at the top of the cell, to replace the liquid drawn off by the syphon from near the bottom. A cover may be advantageously added above, to prevent evaporation. When the cell is much used, so that zinc enough is dissolved, the liquid added above may be pure water; or if large internal resistance is not objected to, the liquid added may be pure water, whether the cell has been much used or not; but after an interval, during which the battery has not been much in use, the liquid added ought to be quarter saturated, or even stronger solution of sulphate of zinc, when it is desired to keep down the internal resistance. It is probable that one or more specific gravity beads kept constantly floating between top and bottom of the heterogeneous fluid will be found a useful adjunct, to guide in judging whether to fill up with pure water or with sulphate of zinc solution. They may be kept in a place convenient for observation by caging them in a vertical glass tube perforated sufficiently to secure equal density in the horizontal layers of liquid to be tested by the floaters.

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* From the Proceedings of the Royal Society.

[†] Storer's Dictionary of Solubilities of Chemical Substances. Cambridge, Massachusetts: Sever and Francis, 1864.

[‡] Ann. Ch. et Phys. (3) vol. xli. pp. 478, 482, 1854.

An extemporised cell on this plan was exhibited to the Royal Society, and its resistance (measured as an illustration of Mance's method, described in the first of his two previous communications) was found to be ·29 of an Ohm (that is to say, 290,000,000 centimetres per second). The copper and zinc plates of this cell, being circular, were about thirty centimetres in diameter, and the distance between them was about 7·5 centimetres. A Grove's cell, of such dimensions that forty in series would give an excellent electric light, was also measured for resistance, and found to be ·19 of an Ohm. Its intensity was found to be 1·8 times that of the new cell, which is the usual ratio of Grove's to Daniell's. Hence seventy-two of the new cells would have the intensity of forty of Grove's. But the resistance of the seventy-two in series would be 209 Ohms, as against 76 Ohms of the forty Grove's; hence, to get as powerful an electric light, threefold surface, or else diminished resistance by diminished distance of the plates, would be required. How much the resistance may be diminished by diminishing the distance rather than increasing the surface, it is impossible to deduce from experiments hitherto made.

Two or three cells, such as the one shown to the Royal Society, will be amply sufficient to drive a large ordinary turret-clock without a weight; and the expense of maintaining them will be very small in comparison with that of winding the clock. The prime cost of the heavy wheel-work will be avoided by the introduction of a comparatively inexpensive electro-magnetic engine. For electric bells, and all telegraphic testing and signalling on shore, the new form of battery will probably be found easier of management, less expensive, and more trustworthy, than any of the forms of battery hitherto used. For use at sea, it is probable that the sawdust Daniell's first introduced on board the *Agamemnon* in 1858, and ever since that time very much used both at sea and on shore, will still be found the most convenient form; but the new form is certainly better for all ordinary shore used.

The accompanying drawing represents a design suitable for the electric light or other purposes, for which an interior resistance not exceeding $\frac{2}{10}$ of an Ohm is desired. The zinc is in the form of a grating, to prevent the lodgment of bubbles of hydrogen gas, which I find constantly, but very slowly, gathering upon the zincs of the cells I have tried, although the solutions used have no free acid, unless such as may come from the ordinary commercial sulphate of copper and commercial sulphate of zinc crystals which were used.

The principle which I have adopted for keeping the sulphate of copper from the zinc is to allow it no access to the zinc except by true diffusion. This principle would be violated if the whole mass of the liquid contiguous to the zinc is moved toward the zinc. Such a motion actually takes place in the second form of element (that which is represented in the drawing, and which is undoubtedly the better form of the two) every time the crystals of sulphate of copper are dropped into the charging-tube. As the crystals dissolve, the liquid again sinks, but not through the whole range through which it rose when the crystals were immersed. It sinks further as the sulphate of copper is electrically precipitated on the copper plate below in course of working the battery. Neglecting the volume of the metallic copper, we may say, with little error, that the whole residual rise is that corresponding to the volume of water of crystallisation of the crystals which have been introduced and used. It becomes, therefore, a question whether it may not become a valuable economy to use anhydrous sulphate of copper instead of the crystals; but at present we are practically confined to the "blue vitriol" crystals of commerce, and therefore the quantity of water added at the top of the cell from time to time must be, on the whole, at least equal to the quantity of water of crystallisation introduced below by the crystals. Unless a cover is added to prevent evaporation, the quantity of water added above must exceed the water of crystallisation introduced below by at least enough to supply what has evaporated. There ought to be a further excess, because a downward movement of the liquid from the zinc to the level from which the syphon draws is very desirable to retard the diffusion of sulphate of copper upwards to the zinc. Lastly, this downward movement is also of great value to carry away the sulphate of zinc as it is generated in the use of the battery. The quantity of water added above ought to be regulated so as to keep the liquid in contact with the zinc, a little less than half saturated with sulphate of zinc, as it seems, from the observations of various experimenters that, the resistance of water semisaturated with sulphate of zinc is considerably less than that of a saturated solution. A still more serious inconvenience than a somewhat increased resistance has been pointed out to me by Mr. Varley as a consequence of allowing sulphate of zinc to accumulate in the battery. Sulphate of zinc crystallises over the lip of the jar, and forms pendants like icicles outside, which act as capillary siphons, and carry off liquid. Mr. Varley tells me that this curious phenomenon is not unfrequently observed in telegraph batteries, and sometimes goes so far as to empty a cell and throw it altogether out of action. Even without this extreme result, the crystallisation of zinc about the mouth of the jar is very inconvenient and deleterious. It is of course altogether avoided by the plan I now propose.

In conclusion, then, the siphon-extractor must be arranged to carry off all the water of crystallisation of the sulphate of copper decomposed in the use of the cell, and enough of water besides to carry away as much sulphate of zinc as is formed in the use of the battery. Probably the most convenient mode of working the system in practice will be to use a glass capillary siphon, drawing quickly enough to carry off in a few hours as much water as is poured in each time at the top; and to place, as shown in the drawing, the discharging end of the siphon, so as to limit the discharge to level somewhat above the upper level of the zinc grating. It will no doubt be found convenient in practice to add measured amounts of sulphate of copper by the charging-tube each time, and at the same time to pour in a measured amount of water, with or without a small quantity of sulphate of zinc in solution.

As 100 parts by weight of sulphate of copper crystals contain, as nearly as may be, 36 parts of water, it may probably answer very well to put in, for every kilogramme of sulphate of copper, half a kilogramme of water. Experience (with the aid of specific-gravity beads) will no doubt render it very easy, as a perfectly methodical action involving very little labour, to keep the battery in good and constant action, according to the circumstances of each case.

When, as in laboratory work, or in arrangements for lecture-illustrations, there may be long intervals of time during which the battery is not used, it will be convenient to cease adding sulphate of copper when there is no immediate prospect of action being required, and to cease pouring in water when little or no colour of sulphate of copper is seen in the solution below. The battery is then in a state in which it may be left untouched for months or years. All that will be necessary to set it in action again will be to fill it up with water to replace what has evaporated in the interval, and stir the liquid in the upper part of the jar slightly, until the upper specific gravity-bead is floated to near the top by sulphate of zinc, and then to place a measured amount of sulphate of copper in the funnel at the top of the charging-tube.

W. THOMSON

NOTES

AT the Anniversary Meeting of the Geological Society, held on the 17th ult., the Wollaston Gold Medal was presented to Prof. Ramsay in recognition of his many researches in practical and in theoretical geology; and the balance of the proceeds of the Wollaston Donation Fund was given to Mr. Robert Etheridge in aid of the publication of his great stratigraphical "Catalogue of British Fossils." A report of the interesting proceedings on the occasion will be found in another column. Mr. Prestwich was re-elected President of the Society for the ensuing year.

A STATUTE was promulgated last week in Convocation at Oxford enlarging the powers of the delegates of "unattached students," whom it is proposed to allow to admit students after examining them in one of the subjects already permitted (*i.e.*, Classics and Mathematics), "together with some other subject recognised in the schools of the University" (*e.g.*, Physical or Natural Science).

We are indebted to *Harper's Weekly* for some of the interesting notes on science in America, which we are able to furnish this week, in advance of their publication there. This magazine provides its readers with an important and interesting summary in each issue of the progress of science on that side of the Atlantic.

DR. WILLIE KUHNE, at present Professor of Physiology at Amsterdam, has been called to occupy the chair left vacant at Heidelberg by the removal of Helmholtz to Berlin.

IT is with great pleasure that we hear from the Abbé Moigno that the publication of *Les Mondes* will have recommenced before this, on March 1. Though the Abbé has escaped uninjured from the bombardment, a portion of his fine library was destroyed by a Prussian shell. We are still without files of any of the Paris papers.

IN reply to a question in the House of Commons on Friday night last, Mr. Ayrton said that considerable progress had been made in the preparation of the plans for the erection of the Natural History Museum at South Kensington, but until they had been completed it would be impossible for them to receive the sanction of the Government.

THE lectures of the present year for the Royal College of Physicians will be delivered at the College, Pall Mall East, at five o'clock on each of the following Wednesdays and Fridays Gulstonian lectures—Dr. Gee, March 3, 8, 10, "On the Heat of the Body;" Croonian lectures—Dr. Parkes, March 15, 17, 22, "On some points connected with the Elimination of Nitrogen from the Human Body;" Lumleian lectures—Dr. West, March 24, 29, 31, "On some disorders of the Nervous System in Childhood."

THE third course of Cantor Lectures for the Session of the Society of Arts will be delivered by Dr. T. S. Cobbold, F.R.S., and will treat of "Our Food-producing Ruminants, and on the Parasites which reside in them." The course will commence on Monday, the 17th of April, and will be continued on subsequent Monday evenings till completed. These lectures are open to members, who have also the privilege of admitting two friends to each lecture.

MR. ANDREW MURRAY will, during the following season, deliver a short course of lectures for the Royal Horticultural Society on Economic Entomology, especially in its relations to Horticulture and Forestry. Mr. Murray has been mainly instrumental in forming the Society's collection of Economic Entomology exhibited in the South Kensington Museum.

FLORIDA has, this winter, its usual complement of scientific visitors, who are engaged in prosecuting investigations upon its natural history. Mr. E. J. Maynard, of Massachusetts, is exploring the ornithology of the keys and the southern portion of the State; Mr. N. H. Bishop, of New Jersey, and Mr. George A. Boardman, of Maine, are at work with a similar object about Jacksonville. Professor Wyman, of Cambridge, is also making use of the opportunities of his third or fourth visit to the State in the critical examination of the ancient mounds and shell heaps which abound everywhere.

DR. PACKARD has lately announced the discovery, by Prof. Verrill, of a dipterous larva of the genus *Chironomus*, at a depth of 120 feet, in the vicinity of Eastport, Maine. He also describes a mite, or *Acarus*, as occurring at a similar depth. He has not yet ascertained whether, like other species of the genus, the latter lives, in any of its stages, in the gills of the lamellibranchiate mollusca.

A RECENT communication to the State Department from the United States consul at St. Helena, states the fact that the white ants, which have effected a lodgment in the island, are rapidly destroying everything upon it. No wood but teak, and sometimes not even that, escapes their fangs; and numbers of houses in Jamestown have been fairly gutted by them—doors, window-sashes, floors, and roofs, all being eaten up, leaving nothing but the bare walls.

THE following is a list of the German learned men connected with the French Académie des Sciences, copied from the *Comptes Rendus*, although some of them have died since the investment of the city, but their names were not erased from the list for want of official notification:—Four Associates, MM. Ehrenberg, Liebig, Wohler, Kummer: three mathematicians, Neumann, Weierstrass, Kronecker; one mechanician, Clausius; three astronomers, Hansen, Argelander, Peters; three physicists, Weber, Mayer, Kirchhoff; two chemists, Bunsen, Hofmann; three mineralogists, Naumann, Rose, Haidinger; four botanists, Mohl, Braun, Hofmeister, Pringsheim; one anatomist, Siebold; three surgeons, Virchow, Rokitansky, Sebert.

THE special correspondent of the *Times* gives the following account of the effects of the bombardment on the Jardin des Plantes:—No fewer than eighty-three shells had fallen within this comparatively limited area. On the night of January 8 and 9 four shells fell into the glass houses and shattered the greater part of them to atoms. A heap of glass fragments lying hard by testified to the destruction, but the effect of the shells was actually to pulverise the glass, so that it fell almost like dust over the gardens. The consequence was that nearly the whole of this most rare and valuable collection was exposed to one of the coldest nights of the year, and whole families of plants were killed by the frost. Some of the plants suffered the most singular effects from the concussion; the fibres were stripped bare, and the bark peeled off in many instances. All the Orchids, all the Clusiaceæ, the Cyclantheæ, the Pandaneæ were completely destroyed, either by the shells themselves or by the effects of the cold. The large palm-house was destroyed, and the tender tropical contents were exposed to that bitterly cold night; yet, singularly enough, although they have suffered severely, not one has yet died. All through the whole of the fortnight during which these gardens were subjected to this rain of shells, MM. Decaisne, Chevreuil, and Milne-Edwards, remained at their posts, unable to rest, and have since, at their own expense, repaired the damage done, trusting that whatever form of government France may choose, it will not repudiate its debts of honour. M. Decaisne is making out a list of his losses, a large proportion of which might possibly be supplied from Kew, while owners of private collections might also be glad to testify their sympathy and interest in the cause of science by contributing whatever they may be able to spare as soon as the amount and nature of the loss is ascertained. The animals fared better than the plants—not only have none of them been eaten by the population of Paris, as the latter fondly suppose, but although several shells burst among them, they have escaped uninjured. Of course, when food was so scarce for human beings, the monkeys and their companions were put upon short allowance. This fact, coupled with the extreme rigour of the season, increased the rate of mortality among them, and one elephant died, but was not eaten. The two elephants and the camel that were eaten belonged to the Jardin d' Acclimatation, and had been removed in the early stage of the siege from their ordinary home in the Bois de Boulogne, for safety, to the Jardin des Plantes, where, however, it would appear, it was not to be found. The birds screamed and the animals cowered, as the shells came rushing overhead and bursting near them, as they do when some terrific storm frightens them; latterly they seemed to become used to it.

WE have great pleasure in announcing that the Museum of Natural History at Strasburg has escaped the bombardment of the town. One shell entered one of the corridors and destroyed a small collection of chalk fossils, and a few fragments of a shell decapitated two or three birds. The concussion caused nearly all the glass in the cases to be broken. But the fine collections of mammals, of birds, and of fossils, the result of many years of labour of Prof. Schimper, are perfectly untouched. This has not been the case with some of the private collections in the town, and one, a collection of European Lepidoptera, belonging to the Taxidermist of the museum, was scattered into dust.

We learn with great regret that the Société d'Acclimatation of Paris has thought it necessary to pass a resolution erasing from the list of its members all sovereigns and princes of German States engaged in the late war against France, "considérant que la manière dont le bombardement de Paris a été effectué par les armées allemandes constitue un acte contraire au droit des gens, ainsi qu'aux plus simples notions de l' humanité."

Among the experiences of the Mount Washington winter party may be mentioned an exposure to perhaps the greatest cold ever recorded in the annals of science. The temperature of 40° below zero was not in itself unusual; but to this was added a hurricane blowing at the rate of ninety miles an hour. The combination of such a wind with the temperature indicated would probably have been entirely unsupportable but for the means of protection enjoyed by the party in the dwelling which had been fitted up expressly for their accommodation.

Prof. J. Young has recently brought the question of the education of the mining engineers before the Institution of Engineers for Scotland. His proposals for its amelioration are as follows:—"1. Great improvement in secondary schools, especially in teaching arithmetic, geometry, elements of natural philosophy. 2. The establishment in some large towns, such as Liverpool and Birmingham, of colleges on the Scottish model, or on that of Owens College—fairly well endowed—giving chiefly general scientific training, with a few special technical chairs. 3. The practical recognition of the value of scientific training by engineers who take pupils. (a) By giving free pupilships or valuable scholarships; (b) By admitting as pupils only those who have passed certain recognised examinations; (c) By co-operating with colleges as examiners; (a) By inserting in agreements with their pupils that during winter they shall attend certain classes; (e) By giving some privileges, in connection with engineering societies, to graduates."

We regret to learn that the fine specimen of *Pandanus odoratissimus* in the Botanical Gardens at Glasnevin, near Dublin, has been completely destroyed by the attacks of a fungus, in all probability the same that has destroyed the Screw Pine in the Breslau Gardens, as referred to by Prof. Oliver in a late number of this journal. The Glasnevin plant was nearly fifty years old.

Messrs. Bell and Daldy will shortly publish "Outlines of Magnetism and Electricity for Public Schools and Science and Art Examinations," being notes of a course of lectures delivered at the Royal School of Naval Architecture, with an introduction on the First Principles of Physics, by W. F. Barrett.

We learn from the *British Medical Journal* that Dr. Thorne Thorne, who has been very successfully engaged for several years as an occasional inspector under the Privy Council, and who more especially led to the exposure of the causes of the Terling epidemic, has been appointed to a permanent position under the Privy Council.

The *British Medical Journal* states that the rumour that Dr. Liebreich, the distinguished ophthalmologist of Paris, was likely to be appointed ophthalmic surgeon and lecturer at St. Thomas's Hospital, is likely to be realised. The reputation of Dr. Liebreich is more than European, and his services to ophthalmoscopic science and practice are such that he may fairly claim to be considered as almost the founder of our present school of ophthalmoscopy. The services which he has rendered to science are cosmopolitan, and we feel assured that if, under the existing state of affairs in Paris, Dr. Liebreich elects to take up his residence in London, his services, reputation, and personal character will secure for him that welcome which the English profession has always been wont to extend to distinguished men of science of every nation, and which well becomes the members of a liberal profession in a country proud of its freedom and hospitality.

Two natives of the Garrow Hills in Madras are to be trained as vaccinators to practise in their tribe, which suffers severely from small-pox. On the other hand the villages in Kunnool oppose the entrance of vaccinators by force, and hide their children in the jungle.

The Siam papers report the fortunate news of the capture of an albino or white elephant. He had been brought to the capital in state, and will in due time succeed to the highest dignities of state, the chief white elephant ranking next the Queen, and the heir apparent coming next only to this elephant.

Mr. W. King, of the Indian Geological Survey, reports from Ballary, in Madras, that he doubts the reported discovery of coal in that district.

The *Homeward Mail* states that the cold has been so intense at the Mullier in Scinde lately, that on January 24th icicles were found on the works connected with the viaduct in that place. It is hardly possible to believe this phenomenon were it not communicated by a reliable eye-witness.

A paper, read to the Academy of Sciences, Paris, during the siege, gives some very interesting information about the great cold experienced there, and its occurrence in former years. In the fifty years from 1816 to 1866, the average temperature of the month of December has been 3·54° Centigrade above zero, but December, 1870, gave an average of 1·07° C. *below zero*, thus showing how far below the average the cold of last year was. In the *Annales de la Société Météorologique*, vol. v., 1861, is a paper by M. Renou, "On the Periodicity of Great Cold." In this he shows that about every forty years there comes round a series of cold winters, in general five or six together, of which the central one is the coldest of all. His researches extend back to the fifteenth century, but to take recent times he notices the great frosts and cold winters which group themselves round the years 1709, 1748, 1789-90, and again in 1829-30. From these facts he predicted in 1860 that there would be a group of severe winters round the winter of 1870-71.

In furtherance of the British Guiana Local Exhibition to be held in that Colony during the present year, the Committee of Correspondence has issued an address to the "Farmers and other Proprietors of the Soil," calling upon them energetically not only to aid the Exhibition by simply sending specimens of the products of the Colony to be seen, passed by, and forgotten, but to bear in mind how many useful products are lost to commerce through lack of continued exertions in the cultivation of the plants and the supply of the home market, and pointing out how that Exhibitions "can never be regarded as entirely satisfactory until additions are permanently made to the stock of commodities which are in daily use at home, as well as to the list of such as make the resources and the importance of the Colony known and esteemed in the markets of the world." British Guiana is a colony rich in natural productions, and it is to be hoped that the pamphlet will be read and digested by those to whom it is addressed.

Another tigress is recorded as dead in India, at the hands of Major Daires, of the Madras Presidency, and none too soon. She had, in seven years, killed above 140 persons in a few villages, so that many families had left, and a great part of the land had gone out of cultivation.

On the 28th October two shocks of earthquake were felt at Shikarhera, in Upper Scinde. On the 1st November a severer shock was felt in the Tinnevelley district than ever had been known in the memory of man. The shocks were undulatory.

THE LONDON CONJOINT EXAMINING BOARDS

WE reprint from the *British Medical Journal* the draft scheme of the committees of the Royal Colleges of Physicians and Surgeons, and the Society of Apothecaries, in the form in which it was presented to the College of Physicians at their Comitia and approved. In this form the scheme differs essentially from that of last year. This is no longer, as it now appears, a scheme for a minimum qualification for English practitioners, complete, unified, and preliminary in all respects to higher diplomas.

The following is the draft scheme :—

1. That one Board of Examiners, in the division of the United Kingdom, be appointed by the Royal College of Physicians of London, the Royal College of Surgeons of England, and the Society of Apothecaries of London, for the examination of candidates who desire to practise Medicine, Surgery, and Midwifery.

2. That candidates who shall have passed the several examinations of the Board be entitled, subject to the bye-laws of each institution, to the Licence of the Royal College of Physicians of London, the Diploma of Member of the Royal College of Surgeons of England, and the Certificate of the Society of Apothecaries of London.

3. That Examiners be appointed as follows; viz. :

In Medicine, by the Royal College of Physicians and the Society of Apothecaries.

In Surgery, by the Royal College of Surgeons.

In Anatomy and Physiology, by the Royal College of Physicians and the Royal College of Surgeons.

In Midwifery, by the Royal College of Physicians, the Royal College of Surgeons, and the Society of Apothecaries.

In Materia Medica, Medical Botany and Pharmacy, Chemistry, and Forsenic Medicine, by the Royal College of Physicians and the Society of Apothecaries.

4. That the number of Examiners assigned to each subject be as follows, viz.: Anatomy and Physiology, not less than 8; Chemistry, Materia Medica, Medical Botany and Pharmacy, not less than 8; Medicine, not less than 10; Surgery, not less than 10; Forensic Medicine, not less than 4; Midwifery, not less than 6.

5. That the appointment of the Examiners in each subject be made by each of the three Corporations in the following proportion; viz., Anatomy and Physiology—Royal College of Physicians, 3; the Royal College of Surgeons, 5. Chemistry, Materia Medica, Medical Botany and Pharmacy—the Royal College of Physicians, 4; the Society of Apothecaries, 5. Surgery—the Royal College of Surgeons, 10. Forensic Medicine—the Royal College of Physicians, 2; the Society of Apothecaries, 2. Midwifery—the Royal College of Physicians, 2; the Royal College of Surgeons, 2; the Society of Apothecaries, 2.

6. That there be two or more examinations on professional subjects, and that the fee payable for the examinations be thirty guineas, to be paid in two or more payments.

7. That one-half of the fees received for the examinations be appropriated to the payment of the examiners and the expenses of the examinations.

8. That the other half of the fees be divided amongst the three Corporations, upon the principle of giving to each Corporation a sum proportionate to that which each has respectively obtained from the grant of licences on the average of the last five years.

9. That the mode of division of the second half of the fees be subject to revision at the end of every three years.

ERNST HAECKEL ON THE MECHANICAL THEORY OF LIFE AND ON SPONTANEOUS GENERATION

IN his recently published Biological Studies* Professor Haeckel, of Jena, has briefly stated his views on the question of Abiogenesis, which is now so largely occupying attention in England. He, having done more than any other observer to establish the Protoplasm theory by his discovery of organisms of the simplest conceivable structure—not even cellular (*i.e.*, not even possessing a differentiated central nucleus)—his remarks on the present condition of the Spontaneous Generation question must possess great weight. The philosophy of Monism, of which he is the exponent, as opposed to Dualism, rests on what he calls the Carbon theory and the Plastic theory. These theories are thus set forth in weighty, but clear sentences :—

1. The forms of organisms and of their organs result entirely from their life, and simply from the interaction of two physiological functions, Heredity and Adaptation.

2. Heredity is a part of the reproduction,—Adaptation, on the other hand, a part of the maintenance of the organism. These two physiological functions depend, as do all forms of vital activity, on the character of the physiological organ through which they come into play.

3. The physiological organs of the organism are either simple Plastids (Cytods or Cells), or they are parts of Plastids (*e.g.*, Nuclei of Cells, cilia of Protoplasm), or they are built up of numerous Plastids (the majority of organs).

In all these cases the forms and actions of the organs are to be traced back to the forms and actions of the individual Plastids.

4. Plastids are either simple Cytods (structureless bits of Protoplasm without nuclei) or Cells; but since these last have originally arisen from Cytods by a differentiation of the inner "Nucleus" and the outer "Protoplasm," the forms and vital properties of all Plastids can be traced back to the simplest Cytods as their starting point.

5. The simplest Cytods, from which all other Plastids (Cytods and Cells) originally have arisen by Heredity and Adaptation, consist essentially and absolutely of nothing more than a bit of structureless Protoplasm—an albuminoid, nitrogenous Carbon-compound; all other components of Plastids have been originally formed secondarily from Protoplasm (plasma-products).

6. The simplest independent organisms which we know, and which moreover can be conceived, the Monera, consist in fact while living of nothing else but the simplest Cytod, a structureless bit of Protoplasm; and since they exhibit all forms of vital activity (nutrition, reproduction, irritability, movement), these vital activities are here clearly bound on to structureless Protoplasm.

7. Protoplasm, or Germinal Matter (*Bildungsstoff*), also called Cell-substance or Primitive Slime (*Urschleim*), is therefore the single material basis (*materielle Grundlage*), to which without exception and absolutely all so-called "vital phenomena" are radically bound; if the latter are regarded as the result of a peculiar Vital Force independent of the Protoplasm, then necessarily also must the physical and chemical properties of every inorganic natural body be regarded as the result of a peculiar force not bound up with its substance.

8. The Protoplasm of all Plastids is, like all other albuminoid or Protein-bodies, composed of four inseparable elements, Carbon, Oxygen, Hydrogen, and Nitrogen, to which often, though not always, a fifth element, namely, Sulphur, is added.

9. The forms and vital properties of Protoplasm are conditioned by the peculiar manner in which Carbon has

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* A series of papers published originally in the *Jenaische Zeitschrift* during the year 1870, the author's Monograph of the Monera—for a translation of which see the Quart. Journal of Micr. Sci., 1869; also, the January No., 1871, where M. Haeckel's views on Coccoliths and Bathybius are noticed and illustrated in a plate.

combined itself so as to form a highly developed compound with the three or four other elements named. Compounds devoid of Carbon never exhibit those peculiar chemical and physical properties which exclusively belong to only a part of the compounds of Carbon (the so-called "organic compounds"); on this account modern chemistry has replaced the term "organic compounds" by the more significant term "Carbon-compounds."

10. Carbon, then, is that element, that indivisible fundamental substance, which, in virtue of its peculiar physical and chemical properties, stamps the various Carbon compounds with their peculiar organic character, and in chief fashions this Protoplasm, the "matter of life" (*Lebenstoff*), so that it becomes the material basis of all vital phenomena.

11. The peculiar properties which Protoplasm and the other component tissues and substances of the organism derived secondarily from it, exhibit, especially their viscid condition of aggregation, their continual change of matter (on the one hand their facile decomposition, on the other their facile power of assimilation) and their other "vital properties," are therefore simply and entirely brought about by the peculiar and complex manner in which Carbon under certain conditions can combine with the other elements.

12. The entire properties of the organism are, therefore, ultimately conditioned with equal necessity by the physical and chemical properties of Carbon, as are the entire properties of every salt and every inorganic compound conditioned by the physical and chemical properties of its component elements.

II.

We now pass on to the chapter in Haeckel's work headed "The Monera and Spontaneous Generation." Although, remarks Haeckel, Darwin himself states in his work that he has nothing to do with the origin of life, every thinking reader of the "Origin of Species" must ask himself whence came the simplest original living form? and no question has been more actively discussed, in consequence of Darwin's reform of the descent theory than that of spontaneous generation (*Urzeugung*). Abiogenesis (*Urzeugung*, which may best be translated as Archigenesis) is, in fact, a necessary and integral part of the universal evolution theory. It is the natural bridge which places in continuity Kant's and Laplace's theory of the mechanical origin of the universe and the earth, with Lamarck's and Darwin's theory of the mechanical origin of animal and vegetal forms. When we perceive that all inorganic nature, as well as the development of organic nature from an original parent organism, is explained by the continual working of one great law of evolution, we cannot admit in explanation of the one dark point in this great causal network a supernatural act of creation. We are logically bound to seek a natural link, and this link is Archigenesis (*Urzeugung*), *i.e.*, the origin of the simplest organisms from so-called lifeless inorganic materal. Till recently the question of Archigenesis has been treated by most naturalists in a most unphilosophical and superficial manner. Instead of examining the bearings of the question in all directions, and discussing duly its very complex nature, they have rushed into experiment, and obtained an answer without fairly putting the question. Because in highly artificial apparatus and under artificial conditions no organisms have been developed in certain fluids prepared for examination, the whole doctrine has been denied, and the totally unwarranted conclusion arrived at, "There is no Archigenesis." Such experiments as those of Pasteur and other very marvellous ones have really no value in this question, since they merely prove that in the particular case, under certain artificial and complex conditions, no organism is formed by Archigenesis. Positive contradiction of the hypothesis of Archigenesis is impossible. Positive proof there is not yet, since no one has yet seen any organism take origin, except by Parentage. But, thanks to our progress in biology during the last ten years, the question no longer presents the theoretical difficulties which it did. Before the discovery of those simplest of conceivable organisms, the Monera—it was necessary that from inorganic materials a Cell should be formed by Archigenesis—an organism presenting two chemically, physically, and morphological distinct portions, the inner Nucleus and the external Protoplasm. The formation of such a nucleated Plastid by Archigenesis is difficult to conceive. But now by the discovery of the Monera the matter assumes quite a different aspect. Such forms as *Protogenes* and *Protamœba* present no definite shapes, have no individual development, but grow and multiply by division. Their growth and nutrition is purely a physico-chemical process, just as the growth of a crystal, with this difference, that the viscid cohesion of Protoplasm entails intussusception of nutriment, whilst the crystal grows at its surface only. The same viscid state of cohesion explains the reproduction of such Monera—which we do not observe in crystals—the cohesive power of the Protoplasm under certain conditions of nutrition is no longer sufficient to hold the body together, and fission occurs. Thus the chief vital phenomena of Monera are traced to physico-chemical causes.

Since in our chemical laboratories, with our exceedingly limited and rough methods, we have succeeded in forming many Carbon-compounds, and have good reason to suppose that we may one day synthetically produce albuminoid bodies, is it not reasonable to suppose that in the great laboratory of Nature, similar but more complex chemical synthesis may go on, such as the formation from inorganic materials of albuminoids and of living protoplasm? If we are to call in a special creative act—superior to mere chemical synthesis—to account for the existence of Protoplasm because we have not succeeded in forming it artificially, so also must we postulate a peculiar creative act for a great variety of minerals, such as felspar, fluor spar, heavy spar, augite, &c., since we are equally unable to build up these inorganic bodies. In this way we should divide the whole world into a Natural and a Supernatural group. The former would contain such salts, gases, &c., as we can build up in the laboratory, also alcohol, acetic acid, &c. All these bodies have arisen by Archigenesis, *i.e.*, by natural, mechanical means, solely by the interaction of the inherent physical and chemical forces of their matter. The latter group would contain all minerals not yet formed in the laboratory, also all the complex Carbon-compounds. These bodies would be considered as arising by "Creation," that is, by supernatural means, through a mysterious creative force existing externally to the bodies.

To every philosophic naturalist such a view must appear as untenable as is every assumption of a "Creation." On the other hand the assumption of an Archigenesis for the first living beings from which all others have developed, is a logical postulate of the human intelligence.

It is not at all remarkable that as yet we have not observed the Archigenesis of Monera. Supposing it were taking place every day and every hour, it would be very difficult to observe. Very minute particles of Protoplasm are found in quantity, both in sea and fresh water, when carefully sought for. They are generally regarded as fragments of decomposing organisms. But what proof is there of this? and how could it be clearly proved that these particles have *not* arisen by Archigenesis? The first commencement of a Protoplasm-granule in a fluid would be as difficult to observe as the first commencement of a crystal in its mother-liquor. And not less difficult would be the observation of the gradual growth of such an excessively minute Protoplasm-granule into the larger protoplasmic masses of *Protamœba*, &c.

Bathybius seems to be of the greatest significance for the theory of Archigenesis. For if not through Archigenesis, whence shall we derive this protoplasmic covering of the deepest sea-bottom?

Either the Monera were once for all, at the beginning of organic life on the earth, produced by Archigenesis—and hence—since Monera exist still to-day—they must have reproduced in a direct line unchanged for many million years; or, in the course of the earth's history, they have been produced by recurring acts of Archigenesis, and in this case there is no reason why this process should not occur at the present time. The latter view presents the fewest difficulties and exceptions to Prof. Haeckel. In any case the Monera still living at the present day point out to us the way to a correct understanding of the Origin of Life, and clear away the greatest difficulties which the hypothesis of Archigenesis previously presented. E. R. L.

SCIENTIFIC SERIALS

THE report of the meeting of Swiss Naturalists (*Schweizerische Naturforschende Gesellschaft*) in Solothurn on the 23-25 August, 1869, opens with an admirable presidential address on the progress of scientific investigation in Switzerland, by Prof. Lang. The reports of the sections contain numerous short notices of memoirs read, but among these are some of greater length and importance, such as a report by Dr. Hengi on his experiments in the culture of *Saturnia Yama-maya* and *S. mylitta*, a notice by M. F. Hermann on the use of Saussure's hair-hygrometer for scientific observations, a paper on creosote and other products by Prof. Volkel, a communication on fluorescence by Prof. Hagenbach, and a paper (printed in full) by Dr. von Fellenberg, containing analyses of some nephrites and jadeites, including a stone implement of the latter mineral from Möhringen-Steinberg, which he regarded as having been imported from the east. From his analyses of nephrites from different localities, it appears that they contain silica and magnesia in equal equivalents, but that the proportion of each of these to the lime varies as 7, 7½, 8, 9 : 3. Hence he regards them as forming not a definite mineral species, but a group of silicates of magnesia and lime formed by the metamorphosis of rocks of similar composition. He does not consider the nephrites allied to the amphiboles and augites. The memoirs include a report by Prof. Pictet de la Rive on the present state of the question as to the limits of the Jurassic and Cretaceous periods, a notice of which has already appeared in NATURE; an account of the Miocene Flora of Spitzbergen, by Prof. Oswald Heer; and a report on the investigation and preservation of the erratic blocks in Switzerland, by MM. A. Favre and L. Soret. An amusing account of the festal doings of the Society concludes the volume.

FROM the Natural History Society of Stralsund (Neu-Vorpommern) and Rugen, we have received the second annual part of their *Mittheilungen* containing the proceedings of the society for the year 1869. It contains a life and list of the works of Prof. A. E. Legnitz, by Prof. von Feilitsch; a description of the Island of Gottland, with a notice of the birds inhabiting it, by M. L. Holtz, unfortunately disfigured by some very absurd misprints; a notice on the proper heat of plants, by Dr. J. Romer, containing details of experiments made with *Philodendron pinnatifidum* Schott, from which it appears that the proper heat of this plant is much higher than stated by C. H. Schulz; and a paper (illustrated) on the itch-mites of fowls by Prof. Finsterberg. In the last-mentioned paper the author describes a species of mite inhabiting the feet of fowls, for which he proposes the formation of a new genus, *Knemidokoptes* (recte *Cnemidocoptes*). He names the species *K. viviparus*, as it produces living young.

THE last part of the *Archives Néerlandaises des Sciences Exactes et Naturelles à Harlem* for 1870 contains the following papers:—J. A. Groshaus on the Specific Heats of Solids and Liquids, which is a continuation of his former papers published in the same journal, in which he confirms Kopp's results in obtaining a constant when the specific heat of every substance is multiplied with its atomic weight, the constant being 6·3 to 6·5.—C. K. Hoffmann and H. Weijenbergh, jr., on the position of Chiromys (The Aye-Aye of Madagascar) in the natural classification. This is an elaborate memoir which was crowned by the society in 1869, and treats of all the characters in detail, their final decision being to make the following classification: Mammalia, Order ii. Quadrumana, Sub-order ii. Prosimiæ, Families: 1, Lemurini; 2, Nycticebini; 3, Macrotarsi; 4, Microtarsi. The fourth family is distinguished by the tarsal bones more or less elongated (*allongés*), and by difference in the dentary systems, especially in the character of the incisor teeth. In common with Macrotarsi they have the tail, long large eyes directed in front, and large ears, and they feed chiefly on insects. Of this family two genera are given; Microcebus Geoff. and Chiromys Cuv., two species of the first one found in Madagascar, and one of the second, the common Aye-Aye.—M. G. F. W. Baehr gives a note on the Results of the Mathematical Study of the Movements of the Eye.—M. H. H. von Zouteveen on the Petrified Forest of Cairo, and by the same author on the Synthesis of Sulphocyanate of Ammonium.—M. Van der Willigen on Holtz's Electrical Machine.—M. A. C. Oudemans, jr., on the Volumetric Estimation of Iron by Hyposulphite of Sodium.—M. H. Weijenbergh, jr., on Parthenogenesis among the Lepidoptera.—M. C. Ritsema on the Origin and Development of *Periphyllus testudo* v.d.H.; and lastly a report on the Purification of the Air of Hospitals by the Combustion of the Organic Germs, by MM. J. van Genns and L. H. von Baumhauer. This is the report of an investigation undertaken at the instance of the Dutch Government to determine if the apparatus devised by M. Woestyn, of Paris, completely destroy all vital properties in the germs. They report that the apparatus contains nothing new, and that it does not effect its purpose any better than the ordinary methods in use.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 19.—"Modification of Wheatstone's Bridge to find the Resistance of a Galvanometer Coil from a single deflection of its own needle," by Prof. Sir William Thomson, F.R.S. In any useful arrangement in which a galvanometer or electrometer and a galvanic element or battery are connected, through whatever trains or network of conductors, let the galvanometer and battery be interchanged. Another arrangement is obtained which will probably be useful for a very different, although reciprocally related object. Hence, as soon as I learned from Mr. Mance his admirable method of measuring the internal resistance of a galvanic element (that described in the first of his two preceding papers), it occurred to me that the reciprocal arrangement would afford a means of finding the resistance of a galvanometer-coil, from a single deflection of its own needle, by a galvanic element of unknown resistance. The resulting method proves to be of such extreme simplicity that it would be incredible that it had not occurred to any one before, were it not that I fail to find any trace of it published in books or papers; and that personal inquiries of the best informed electricians of this country have shown that, in this country at least, it is a novelty. It consists simply in making the galvanometer-coil one of the four conductors of a Wheatstone's bridge, and adjusting, as usual, to get the zero of current when the bridge contact is made, with only this difference, that the test of the zero is not by a galvanometer in the bridge showing no deflection, but by the galvanometer itself, the resistance of whose coil is to be measured, showing an unchanged deflection. Neither diagram nor further explanation is necessary to make this understood to any one who knows Wheatstone's bridge.

Zoological Society, February 21.—Mr. Osbert Salvin, F.Z.S., in the chair. The Secretary announced the birth of a young Hippopotamus in the Society's Gardens, which had taken place that day, being the first occurrence of this event in England, although this animal had previously bred in some of the Continental Gardens.—Mr. Sclater exhibited and made remarks upon the tusk of an Indian elephant, which appeared to have been attacked by parasites.—The Secretary exhibited, on behalf of Mr. E. Ward, F.Z.S., a remarkably fine series of heads, and horns of sheep and other wild animals, which had been collected in Ladahk by Mr. George Landseer.—A communication was read from Dr. W. Peters, F.M.Z.S., containing a note on the *Tænia* from the Rhinoceros, in reference to a previous communication from Dr. Murie, to the society, upon the same subject.—A communication was read from Mr. J. H. Gurney, F.Z.S., containing remarks on certain species of Abyssinian birds.—A communication was read from Dr. J. Anderson, C.M.Z.S., containing notes on certain Indian reptiles, belonging to the collection of the Indian Museum, Calcutta.—A second communication from Dr. Anderson contained the descriptions of eight new species of birds, recently collected by him during the Yunan expedition.—A paper was read by Mr. W. C. Atkinson, containing descriptions of some new species of Diurnal Lepidoptera, discovered in Yunan by Dr. Anderson during the same expedition.—The Secretary communicated a paper by Mr. Edward Bartlett, containing observations on the habits and distribution of the monkeys of Eastern Peru, as observed during a recent four years' sojourn on the Upper Amazons.

Geological Society, February 8.—Mr. Joseph Prestwich, F.R.S., president, in the chair. 1. "On the Punfield Formation," by Mr. John W. Judd. Those formations, which have been deposited under *fluvio-marine* conditions, and which yield at the same time marine, fresh-water, and terrestrial fossils, are of especial interest to the geologist, as they furnish him with a means of correlating the great fresh-water systems of strata with those of marine origin. At the bottom of the Wealden we have one such fluvio-marine series, the well-known Purbeck formation; at its summit is another, less known, but not less important, for which the name of "Punfield Formation" is now suggested. Some of the fossils of the latter were first brought under the notice of geologists by Mr. Godwin-Austen in 1850, and their peculiarities have since been the subject of remark by Prof. E. Forbes, Sir C. Lyell and others. The typical section of the beds is at Punfield Cove, in the Isle of Purbeck, where they are about 160 feet thick, and include several bands with marine shells. The lowest and most remarkable of these yields about forty well-defined species, many of which, as well as one of the genera, are quite new to this country. A section somewhat similar to that of Punfield is seen at Worborrow Bay. In the Isle of Wight, at Compton, Brixton, and Sanddown Bays, similar fluvio-marine beds are found at the top of the Wealden, and attain to a thickness of 230 feet. The marine bands here, however, yield but a very scanty fauna. Indications of the existence of beds of the same character and in a similar position are found in the district of the Weald. While the Purbeck formation exhibits the gradual passage of the marine Portlandian into the freshwater Wealden, the Punfield formation shows the transition of the latter into the marine Upper Neocomian (Lower Greensand). Thus we are led to conclude that the epoch of the English Wealden commenced before the close of the Jurassic period, lasted through the whole of the Tithonian and of the Lower and Middle Neocomian, and only came to a close at the commencement of the Upper Neocomian. In tracing the Cretaceous strata proper from east to west, they are found to undergo great modification, while the Neocomian and Wealden, which they overlap through unconformity, besides being greatly changed in character, thin out very rapidly. On stratigraphical and palæontological evidence, the Punfield formation is clearly referable to the upper part of the Middle Neocomian. Its fauna has remarkably close analogies with that of the great coal-bearing formation of eastern Spain, which is of vast thickness and great economic value. The claim of the Punfield beds, equally with the similarly situated Purbeck series, to rank as a distinct formation, is founded on the distinctness of their mineralogical characters, their great thickness, the fact of their yielding a considerable and very well characterised fauna, and of their being the equivalent of a highly important foreign series. The president remarked that the limited amount of fresh-water formations in this country was an obstacle to their correlation, and stated that Constant Prevost had endeavoured to correlate the secondary fresh-water and marine formations. Mr. Godwin-Austen remarked upon the thinning out of the Lower Greensand, especially in France; upon the imperfection of our knowledge of the great Cretaceous formation, and upon the probability of the intercalation of fresh-water conditions in the Lower Greensand. The formation at Punfield seemed to present an intercalation of marine between purely fresh-water conditions. He indicated how a slight change of level might have intercalated marine conditions in the Wealden. The deposition of the White Chalk and Oolite occupied enormous periods (in both cases purely marine), during which the northern hemisphere was a great northern ocean; and as the distribution of land and water was due to the operation of great cosmical laws, the duration of terrestrial and of the intermediate freshwater conditions was probably of equal length. Mr. Judd, in reply, said that he did not propose the term Punfield formation as a definitive term, but only as a matter of convenience. He believed that strata could be positively identified by he organic remains contained in them, although the method may have been grossly abused. Physical investigations alone led to nothing but confusion, as might be seen by the stratigraphical attempts of the predecessors of William Smith. The name *Vicarya* for the shell which had been referred to was only provisionally adopted, on the authority of De Verneuil and other writers. 2. "Some remarks on the Denudation of the Oolites of the Bath district, with a theory on the Denudation of Oolites generally." By Mr. W. S. Mitchell. The author briefly referred to the theory according to which oolitic deposits were supposed to have been originally spread out in continuous sheets over the country which they occupy, and to owe their division into separate hills to the action of denudation after their original deposition and consolidation. He suggested, as an equally probable hypothesis, that whilst the marls and clays of oolitic areas were probably originally deposited in continuous beds, the limestones in many cases may never have extended beyond the areas now occupied by them. He described the beds of limestone in the oolitic hills as thinning out towards the valleys on all sides, maintained that the limestones owed their origin to coral reefs, and cited several descriptions of coral islands by Prof. Jukes, to show the agreement in their structure with that which he ascribed to the oolitic hills. He assumed that in the event of a coral-area becoming one of sedimentary deposition, the sedimentary deposit would preserve intact the contour of the coral islands, and inferred that this has been the case in the Bath district, so that the Great Oolite cappings of the hills of that area may represent the original contours of coral islands, exposed by the denudation of the Bradford clay. The amount of denudation undergone by the Great Oolite limestone he considered to be very small. The Inferior Oolite, on the contrary, he believed to have suffered denudation, and he considered that the course of the valleys formed by this agent was dependent on the form of the limestones capping the hills. Prof. Morris did not consider that the author's views as to the oolitic masses round Bath being originally isolated coral banks with clay beds, although suggestive, were quite satisfactory. He pointed out that the strata on each side the valleys were similar in structure, mineral character, and fossil contents, and were once continuous; and the present intervening deep valleys were rather due to the movements which the area had undergone in producing lines of weak resistance, subsequently assisted by the erosive action of percolating and running water, both in excavating and undermining the harder rocks, so as to cause them to bend towards the hill-sides, or fall in larger or smaller masses on their slopes. Mr. Mitchell, in reply, stated that he had seen both sides of what he regarded as coral reefs. He remarked that his hypothesis was arrived at by deduction, by inferring from observations on existing coral-reefs that those of the Oolites must have been covered up as islands. He remarked that if the oolitic beds had slipped, as described, upon the underlying clays, they could hardly range on opposite sides of the valleys. He noticed that the action of water in covering the blocks of Oolite with crystallised carbonate of lime would be protective, and remarked that the surface of the reefs was virtually a sea-bottom on which mollusca lived, so that their occurrence at corresponding levels in different hills was not to be wondered at.

February 17.—Mr. Joseph Prestwich, F.R.S., president, in the chair.—The Secretary read the reports of the Council, of the Library and Museum Committee, and of the auditors. The general position of the Society, as shown by the state of its finances and the continued increase in the number of Fellows, was said to be very satisfactory. In presenting the Wollaston Gold Medal to Prof. Ramsay, F.R.S., F.G.S., the President spoke as follows—"Prof. Ramsay,—I have great pleasure in presenting you with the Wollaston Medal, which has this year been awarded to you by the Council of the Society, in recognition of your many researches in practical and in theoretical geology. Distinguished as your services have been in connection with the Geological Survey since you entered upon it as the Assistant Geologist of Sir Henry De la Beche in 1841, and more particularly since your appointment as Local Director in 1845, during which period you have superintended and carried out the admirably minute style of mapping now general on the survey, and done so much in training its members in the field, you have not less distinguished yourself by your investigations of the higher problems involved in the study of geology. Your first work was on the Isle of Arran; and although then only a beginner, you, instead of taking the rocks to be what they looked, worked out what they were, and gave a new and independent reading of them, which has since in great part proved to be the right one. In 1846 your well-known memoir, 'On the Denudation of South Wales and the adjacent Counties of England,' showed the enormous amount of denudation that the Palæozoic rocks had undergone before the deposition of the New Red Sandstone. At subsequent periods you dwelt on the power that produced 'Plains of Marine Denudation,' a term introduced, I believe, by yourself, and showed in all cases, by a series of true and beautiful sections, how this has operated in planing across the older strata, and how valleys had been scooped out by subsequent aqueous causes in the great plains so formed. Whilst unravelling the complicated interior phenomena of the Welsh rocks, you were not unmindful of the very different order of phenomena exhibited on their exterior surfaces. Here you showed the vast extent and power of ice-action, and what a glacier land Wales once was. Reasoning from the present to the past, you also boldly pushed your ice-batteries far back into geological time, and were the first to bring them to bear on rocks of Permian age. That advanced post you long had to hold alone; but other geologists have since followed your lead, and we have even lately had evidence in the same direction from Southern Africa, where it is asserted that boulders and glaciated surfaces have been found at the base of the Karoo formation of supposed Jurassic age. You have also held a prominent place among those who, by their public teaching, have done so much during the last twenty years to advance the cause of our science. To myself, personally, whose geological career has run nearly parallel in time with your own, it is a source of much pleasure that it has fallen to my lot to hand you this, the highest testimonial the Society has to bestow."—Prof. Ramsay made the following reply:—"Mr. President,—I cannot say whether I am more pleased or surprised by the unexpected award to me of the Wollaston Medal by the Council of this Society. Pleased I well may be, not because I ever worked for this or any other honour, but because I feel a sense of satisfaction that the work on which I have been engaged for the last thirty years has been esteemed by my friends and fellows of the Council of the Society so highly, that they have deemed me a fit recipient of this honour. It is also a special satisfaction to me that this award has been bestowed by the hand of one of my oldest geological friends, who is so universally esteemed and beloved, and is himself so distinguished a contributor to physical and other branches of our science. My first endeavour in geology (the construction of a geological map and model of Arran) necessarily drew my attention to the physical part of our science; and when, consequent upon that work, I was, through the intervention of my old and constant friend, Sir Roderick Murchison, appointed by Sir Henry De la Beche to the Geological Survey of Great Britain, my whole subsequent life was thereafter necessarily involved in questions of physical geology, for no man can work on or conduct the field-work of such a survey who does not, aided by palæontology, necessarily make that his first aim. If some of my theories, induced by that work, were long in being recognised, the recognition has been all the more welcome when it came. Probably I never should have been able to do what I have done but for the wise example of my old master, Sir Henry himself, in his time the best thinker in England on the physical branch of our science, and to whose remarkable work, 'Researches in Theoretical Geology,' all geologists are to this day indebted. The papers which I have written are mere offshoots from my heavier work on the Geological Survey. Perhaps they are enough for the readers; but I wish they had been more numerous, for I certainly have had many more in my mind. Two of these, on old physical geographies of the world, I have lately given to the Society; and if they should be printed, I shall be well pleased should they soon or late be found worthy. The present physical geography of the world is but the sequel of older physical geographies; and to make out the history of these is one of the ultimate aims of geology. These are the subjects I have striven to master in part. I consider your award as a sign that I have had some success; and if, before I cease to work, I have a little more, I may be well content."—The President then presented the Balance of the Proceeds of the Wollaston Donation Fund to Mr. Robert Etheridge, F.G.S., in aid of the publication of his great stratigraphical "Catalogue of British Fossils," and addressed him as follows:—"Mr Etheridge,—The Council of the Society has awarded to you the Proceeds of the Wollaston Fund, to aid in prosecuting your valuable work on the fossils of the British Islands, stratigraphically arranged. In this work, on which you have been engaged during the last eight years, and which occupies nine volumes of MS., representing as many geological groups, you give the natural history lists of each group, and trace the history of each species both in time and space. Of the magnitude of the work few can have any idea, nor would many have an idea of the marvellous extent of past life in our small portion of the globe without a comparison of our recent fauna with those (necessarily incomplete, because only partly accessible) which you have enumerated in your most useful lists. This comparison shows:—

	Polyzoa Zoophytes Echinoderms.	Crustacea.	Mollusca.	Fishes.	Reptilia.	Birds.	Mammalia.	Plants.	Total.
Number of Species in the existing fauna and flora of Gt. Britain . . .	616	278	567	263	15	354	76	1820	3,989
Number of Species found fossil in Gt. Britain . . .	2574	746	7091	815	224	12	172	819	12,453

I trust that this work will not be allowed to remain in MS., and that, presuming you will begin with the oldest, we may soon look for an instalment in the fauna of the Palæozoic rocks. I have much pleasure in presenting you with this token of the importance which the Geological Society attaches to your labours."—Mr. Etheridge made the following reply:—"I have great satisfaction in receiving from you, Sir, and the Council of the Geological Society, the award of the Wollaston Fund. It is given for work known to be nearly done, and faith in its completion. The time and labour devoted to my book upon the 'Stratigraphical Arrangement of the British Fossils' has extended over nearly nine years of incessant work, and has been an arduous, yet pleasant undertaking, now made lighter by the recognition of those who know and value the researches made for so extensive a catalogue of the British organic remains, now numbering nearly 13,000 species. It is this estimation of my labour by the Council and Society that tends to increase the desire to make my work as perfect as possible, well knowing how difficult, if not impossible, it is to do so. This acknowledgment, Sir, from your hands will stimulate me to finish my researches into the literature of the British species, and their history through space and time throughout Europe."—The President then proceeded to read his anniversary address, in which he discussed in considerable details the bearing of the recent deep-sea dredging operations upon geological reasoning. The address was prefaced by biographical notices of deceased fellows, including Sir Proby Cautley, Sir Frederick Pollock, Mr. Robert Hutton, and Prof. Gustav Bischoff.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President: Joseph Prestwich, F.R.S.. Vice-presidents: Sir P. de M. G. Egerton, M.P., F.R.S., Prof. T. H. Huxley, F.R.S., Sir Charles Lyell, Bart., F.R.S., Prof. John Morris. Secretaries: John Evans, F.R.S., David Forbes, F.R.S. Foreign Secretary: Prof. D. T. Ansted, F.R.S. Treasurer: J. Gwyn Jeffreys, F.R.S. Council: Prof. D. T. Ansted, F.R.S., Dr. W. B. Carpenter, F.R.S., William Carruthers, W. Boyd Dawkins, F.R.S., Prof. P. Martin Duncan, F.R.S., Sir P. de M. G. Egerton, Bart., F.R.S., John Evans, F.R.S., David Forbes, F.R.S., J. Wickham Flower, Capt. Douglas Galton, C.B., F.R.S., R. A. C. Godwin-Austen, F.R.S., J. Whitaker Hulke, F.R.S., Prof. T. H. Huxley, F.R.S., J. Gwyn Jeffreys, F.R.S., Sir Charles Lyell, Bart., F.R.S., C. J. A. Meyer, Prof. John Morris, Joseph Prestwich, F.R.S., Prof. A. C. Ramsay, F.R.S., R. H. Scott, F.R.S., Prof. J. Tennant, Rev. Thomas Wiltshire, Henry Woodward.

London Mathematical Society, Thursday, Feb. 9.—Mr. W. Spottiswoode, President, in the chair. Mr. C. R. Hodgson, B.A., was proposed for election, and the Rev. J. Wolstenholme and Mr. R. B. Hayward, of Harrow, were elected members. Prof. Cayley made a communication "On an Analytical Theorem from a New Point of View," and also a second communication "On a Problem in the Calculus of Variations." The problem is, $z = \frac{1}{3}(3x - y^2)y$, to find v a function of x such that $\int z\,dx =$ maximum or minimum, subject to a given condition $\int y dx = C$ (the limits of each integral being $x_1\ x_0$ where these quantities are each positive, and $x_1 > x_0$). The ordinary method of solution gives

$$y^2 = x + \lambda, \text{ for}$$

$$\text{where } (x_1 + \lambda)^{\frac{3}{2}} - (x_0 + \lambda)^{\frac{2}{3}} = \tfrac{3}{2} C.$$

So long as c is not less than $(x_1 - x_0)^{\frac{3}{2}}$, there is a real value of λ, but for a smaller value of c there is no real value. The difficulty arising in this last case is somewhat illustrated by replacing the original problem by a like problem of ordinary maxima and minima; viz., $x_1, x_2, \ldots x^n$ being given positive values of x, in the order of increasing magnitude—and if in general

$$z_1 = \tfrac{1}{3}(3x_1 - y_1^2)\, y_1,$$

then the problem is to find y_i a function of x_i, such that Σz_i = max. or min., subject to the condition $\Sigma y_i = c$. We have here $y_i^2 = x_i + \lambda$, where λ is then to be determined by the condition $\Sigma y_i = c$; the remainder of the investigation turns on the question of the sign

$$y_i = +\sqrt{x_i + \lambda} \text{ or } y_i = -\sqrt{x_i + \lambda}$$

to be taken for the several values of i respectively.—Prof. Henrici exhibited a plaster model of a tubular surface of the 6th order, which may be generated in either of the two following modes. Either a sphere of constant radius moves with its centre on a parabola, or it rolls along the same parabola always touching both its branches. The two envelopes thus produced differ in position only. The second mode of generation shows that the surface has a nodal curve, which is a parabola congruent to that on which the centre of the sphere moves; but in a plane perpendicular to it. Through a part of it only do real sheets of the surface pass. There is also a cuspidal curve of the 6th order, which has two cusps. The nodal curve passes through them, and has at these cusps the same tangents. The equation to the surface is

$$(27\, py^2 + 9\, x\, K - x^3)^2 = (x^2 + 3K)^3$$

where

$$K = (x + 2p)^2 + y^2 + z^2 - r^2$$

r = radius of the sphere and $4p$ is the parameter of the parabola. The equations to the parabola, on which the centre of the sphere moves, are

$$y^2 = 4p\,(x + 2p),\ z = 0\,;$$

those of the nodal curve,

$$y = 0,\ z^2 = -4px + r^2 - 4p^2\,;$$

the equations to the cuspidal curve are

$$27py^2 - 4x^3 = 0,\ x^2 + 3k = 0\,;$$

the first is a cylinder, which cuts the plane $z = 0$ in the evolute of the parabola, the second represents an ellipsoid of revolution. The model was constructed to the scale

$$p = \tfrac{1}{20},\ r = 2 \text{ inches.}$$

It was agreed, on the suggestion of Dr. Hirst, that Prof. Henrici should order a second model to be cast for the use of the society. Mr. Merrifield, F.R.S. laid the following statement before the society. "If the equation of a surface be

$$Z = F(x\,y) \qquad (1)$$

it is very well known that the condition that it should be a ruled surface is that

$$\left(\lambda \frac{d}{dx} + \mu \frac{d}{dy}\right)^2 z \qquad (2)$$

and

$$\left(\lambda \frac{d}{dx} + \mu \frac{d}{dy}\right)^3 \qquad (3)$$

should have a common factor of the form $A\lambda + B\mu$; and also that the condition of its being developable is that (2) should have two equal factors of that form. I have found upon actual trial that for a conical surface (3) will have two equal factors, and for a cylindrical surface, three equal factors; that is to say, if we write, $\alpha = \dfrac{d^3z}{dx^3}$, $\beta = \dfrac{d^3z}{dx^2dy}$ &c., we have for a conical surface

$$(\alpha\,\delta - \beta\,\gamma)^2 = (\alpha\gamma - \beta^2)\,(\beta\,\delta - \gamma^2)$$

and for a cylindrical surface we have separately

$$(\alpha\,\delta - \beta\,\gamma) = 0,\ (\alpha\,\gamma - \beta^2) = 0,\ \beta\,\delta - \gamma^2 = 0$$

If, following Monge, we regard the surface as traced out by a right line moving on three director curves, the condition of two or three equal roots is evidently the same as that, out of the three characteristics passing through a point, two or three should become coincident. I have not yet had time to look into the question whether the converse of the proposition is true, viz., whether the introduction of the condition of developability ($rt = s^2$) necessarily reduces the surface, in which two or three of the characteristics coincide, to a cone or cylinder." The president and members present expressed their wish that Mr. Merrifield would be able to find time for the consideration of this converse proposition. Dr. Hirst then made some remarks on the connection between the correlation of two planes, as described in his last communication to the Society, and Sturm's solution of the problem of projectivity, as given by him in his memoir on the subject, published in the *Mathematische Annalen*, Vol. i., p. 533.

Linnean Society, February 16.—Mr. G. Busk, Vice-president, in the chair. Dr. J. D. Hooker presented to the Society on behalf of a committee appointed for the purpose, a half-length portrait of the President, Mr. G Bentham, the expense of which had been defrayed by a subscription raised among the fellows of the Society. The following papers were then read, the interest of which was purely technical:—On Tremellineous Fungi and their Analogues, by L. and C. Tulasne; Bryological Remarks by Dr. S. O. Lindberg.

Entomological Society, February 20.—Mr. A. R. Wallace, president, in the chair. Mr. Bond exhibited a hybrid between *Bombyx Pernyi* and *B. yama-mai*, two of the larger silk-worm moths; this individual was of the colour of the one parent with the form of the other. He also exhibited an example of *Bombyx mori*, bred by Dr. Wallace, still retaining the larval head. Mr. McLachlan called attention to the first-recorded instance of a similar arrest of development, being a paper by O. F. Muller in "Der Naturforscher" for 1871. Mr. Smith mentioned that a common Egyptian wasp, *Rhynchium brunneum*, obliterated, by its nest, the inscriptions on the ancient monuments in that country; and he exhibited an example of the same wasp which had been found in the folds of the covering of a mummy, showing that the same species had inhabited Egypt for many ages. Mr. Smith further alluded to a passage in Pepys's Diary, dated May 1665, in which the writer narrated how he had seen a glass-hive where the bees could be seen at work, proving that observatory hives were not a modern invention. Mr. Müller read a paper "On the Dispersion of Non-migratory insects by Atmospheric Agencies," in which he had collected together a number of records of showers of insects after violent storms, and at sea at long distances from land; and he was of opinion that these agencies played a considerable part in the geographical distribution of insect life, though, no doubt, in many cases, the species thus involuntarily dispersed died out from inability to cope with the pre-existent denizens of the localities to which they were driven. Mr. H. Jenner-Fust communicated a supplement to his treatise on the geographical distribution in these islands of the indigenous Lepidoptera.

DUBLIN

Royal Irish Academy, Feb. 13—Rev. J. H. Jellett, B.D., president, in the chair.—Dr. Ferguson read a paper "On the Difficulties attendant on the Transcription of Ogham Legends, and the Means of Avoiding them." Leave was given to Mr. Charles E. Burton to read notes "On the Results obtained by the Agosta Sicily Expedition to Observe the recent Solar Eclipse."—A paper was read by Profs. W. King and T. H. Rowney, "On the Geological and Microscopical Structure of the Serpentine Marble or Ophite of Skye."—Papers "On Eozoon Canadense," by Principal Dawson, and on Messrs. King and Rowney's paper "On Eozöon Canadense," by Dr. T. S. Hunt, were deferred to the meeting of the 27th inst. when the discussion of all the papers on this subject will be taken.—Rev. President Henry, D.D., Belfast, H. Dix Hutton, LL.B., and T. W. Ellison Macartney were elected members of the Academy, and Prof. Traquair was admitted a member.—Sir W. Wilde presented on behalf of the Earl of Mayo, a collection of ancient Indian Coins, for which the marked thanks of the meeting were voted.

HOBART TOWN

Royal Society of Tasmania, October 11, 1870.—His Excellency, C. Du Cane, Esq., President, in the chair. The Secretary read some "Notes on an experiment with the fumes of sulphur, and of other methods for the destruction of rabbits in their burrows," by W. Archer, Esq., F.L.S. The fumes were forced into a burrow by means of bellows, attached to a receptacle in which the sulphur was burned; and that this was effectually done was proved by the escape of sulphurous vapour from the bolt-holes. When the burrow was afterwards opened, however, no trace of the fumes was left, nor were the animals destroyed. The experiment was recorded as a "guide or warning to others who may be induced to try further experiments with the fumes of sulphur, or with any other vapour." (Carbonic acid gas would not become condensed, and it would be fatal to animal life, but its use would probably be much too expensive). Mr. Abbott read a paper "On the Sun and its Office in the Universe." Some discussion of a conversational character having taken place, Mr. M. Allport, begged to call the attention of the meeting to the fish presented by Mr. Wise (presentation No. 8), on account of its high scientific importance, as furnishing a complete answer to the theory raised by Dr. Günther in reference to the salmon first sent to England. The Doctor then assumed that the fish sent was hatched from one of the eggs imported to England in 1866. This assumption was met by the statement that the fry unnaturally detained in fresh water had reached a higher state of development than the smolt sent to England, and as the fish now presented was but just assuming the smolt stage, all the arguments used in reference to the smolts first caught apply with tenfold force to this specimen. It was, moreover, fortunate that they had in the Museum one of the fry hatched from the English eggs received per *Lincolnshire* in 1866, and which died in the spring of 1867. Upon comparing this with the fish now caught, it would be found that they accorded with one another so closely, as to leave little doubt of their identity in species. No report had yet been received from England as to the smolt last sent, though they had heard of its safe arrival. Mr. Allport further observed that Mr. Youl, in writing to Sir Robert Officer, had expressed a wish that the Salmon Commissioners should make it publicly known that after careful examination he entirely concurred with Dr. Günther in the opinion that the specimen first sent to England was a Salmon trout (*Salmo trutta.*)

Berlin

Royal Prussian Academy of Sciences, June 2, 1870.—Prof. G. Rose communicated a long and elaborate memoir on the connection between the hemihedric crystalline form and thermo-electrical properties in iron-pyrites and cobalt-glance, with some remarks on the theory of hemihedric forms in general; and Prof. Dove read a paper on the reference of the annual curve of temperature to the conditions upon which it depends.

June 16.—M. Kummer read a paper on the simplest representation of the complex numbers formed from unitary roots, which can be effected by multiplication with unities. Prof. W. Peters read a description of *Propithecus Deckenii*, a new species of Lemuroidea from Madagascar; it is the species which had previously been identified by him with *P. diadema* Bennett.

June 23.—A paper was read on the Morphology of *Chondriopsis cœrulescens*, Crouan, and the optical phenomena presented by that Alga, by Dr. Leopold Kny. The author described in some detail the peculiar cell development and mode of ramification of the plant, and noticed more briefly the structure of the reproductive organs. The peculiar colour presented by the plant is produced by the contents of the outermost cortical layer of cells, and is due to the presence in them of certain corpuscles which have the faculty of reflecting blue light. Prof. du Bois Reymond read a supplement to his memoir on the aperiodic movement of muffled magnets.

June 27.—Prof. C. Rammelsberg read some contributions to the knowledge of meteorites. He first communicated some remarks on the analysis of meteorites, relating to a more recent process for the separation of nickel from iron, to the separation and determination of meteoric iron in stony meteorites, and to the analysis of the silicates, and then furnished analyses of meteoric irons, of the pallasite of Brahin, and of the chondrites of Pultusk, Richmond, and Iowa. His analyses of these chondrites and of that of Klein Wenden, lead him to the conclusion that they all contain only two silicates, olivine and broncite, a result which he finds to be confirmed by other analyses, and he affirms, that mesosiderite and chondrite do not differ petrographically but only in structure.

BOOKS RECEIVED

English.—The Descent of Man, 2 vols.: C. Darwin (Murray).—The General Structure of the Animal Kingdom, 4th edition (Van Voorst).—A Treatise on Smoky Chimneys: F. Edwards (Longmans)—Mathematical Papers of the late George Green: N. M. Ferrers (Macmillan).—A Synopsis of the Family Unionidæ: Isaac Lee (H. C. Lea, Philadelphia)—Thesaurus Syriacus, fasc. ii.

DIARY

THURSDAY, March 2.

Royal Society, at 8 30—Further Experiments on the Effect of Diet and Exercise on the Elimination of Nitrogen: Dr. Parkes, F.R.S.—Magnetic Observations made during a Voyage from St. Petersburg to the Coasts of the Arctic Sea, in the Summer of 1870: Capt. Belavenetz, I.R.N.

Society of Antiquaries, at 8.30.—On Roman Antiquities at Lydney Park: Rev. W. H. Bathurst.

Chemical Society, at 8.

Linnean Society, at 8.—On the Tamil names of Plants: Rev. S. Mateer.—Contributions towards a knowledge of the *Curculionidæ*: H. P. Pascoe.

Royal Institution, at 3—Davy's Discoveries: Dr. Odling.

London Institution, 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers

FRIDAY, March 3.

Royal Institution, at 9.—Pressure of Fired Gunpowder: Capt. Noble.

Geologists' Association, at 8.—On the Range in Time of the Foraminifera: Prof T. Rupert Jones, F.G S.—On the English Crags, considered in reference to the Stratigraphical Divisions indicated by their Invertebrate Fauna: Alfred Bell.

Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, March 4.

Royal Institution, at 3—Socrates: Prof. Jowett.

SUNDAY, March 5.

Sunday Lecture Society, at 3.30.—Iceland: its Physical Features, Volcanoes, Hot Springs, &c.: Jon A. Hjaltalin.

MONDAY, March 6.

Royal Institution, at 2.—General Monthly Meeting.

Entomological Society, at 8.

Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

London Institution, at 4.—On Astronomy: R. A. Proctor, F.R A.S.

Anthropological Institute, at 8.—On the Racial Aspects of the Franco-Prussian War: J. W. Jackson.

TUESDAY, March 7.

Royal Institution, at 3—Nutrition of Animals: Dr. Foster.

Zoological Society, at 9.—Notes on rare or little-known Animals now or lately living in the Society's Gardens: P. L. Sclater.—List of the Lizards belonging to the family *Sepidæ*, with Notes on some of the species: Dr. A. Gunther.—On new Insects collected by Dr. John Anderson during the Expedition to Yunan: F. Moore.—Observations on the Record of Accessions to the Gardens of the Zoological Society: Dr. J. E. Gray.

WEDNESDAY, March 8.

Society of Arts, at 8.—The Cultivation and Uses of Sugar-beet in England: Dr. A. Voelcker.

Geological Society, at 8.

Royal College of Surgeons, at 4—On the Teeth of Mammalia: Prof. Flower.

Royal Microscopical Society, at 8.

Press Literary Fund, at 3.—Anniversary Meeting.

THURSDAY, March 9.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

London Mathematical Society, at 8.—Remarks on the Mathematical Classification of Physical Quantities: Dr. Clark Maxwell, F.R.S.—On Skew Cubics: Prof. H. J. S. Smith, F.R.S.

London Institution, at 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers, M.A.

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Erratum.—Page 322, second column, line 20, for "western or right hand" read "eastern or left hand."

THURSDAY, MARCH 9, 1871

THE TEMPLE MEMORIAL AT RUGBY

WE hear with peculiar satisfaction that one of the memorials of the Bishop of Exeter at Rugby is be an Observatory. It is a very appropriate memorial a man who, above most others, recognised the true rela- ons of science and literature, and did so much to give ience its rightful place. And it is a valuable addition the resources of a school; at present perhaps it will ot find its full use; but when it does it will be well ssociated with the name of one who has foreseen the ture position of our great public schools.

Our great public schools fulfil two functions, they repare for the universities, but they are themselves the niversities for the great majority of the boys who go to hem; those who go into business, into the army, and to any other occupations, do not in general go to Oxford r Cambridge. Their school education ends at eighteen. Iow this large class is scarcely sufficiently contemplated t schools. For them it is necessary that a school should ffer, not the first part of an education which requires any years to complete it, but the best education that n be given where this limit of age is imposed. For such ys as these it is highly desirable that their school course ould have more elevation about it, and more of prac- al application. It would take us far from our present bject if we were fully to develop our meaning; but it ems a step, and an important one, in the right direction, establish an astronomical and meteorological ob- vatory at one of the public schools; for this is to assert t such knowledge of these subjects as is attainable ght to be within reach of boys who will not have the portunity of studying them after complete mathematical ning at the universities. It is one step further towards ablishing the ideal education, the co-existence of gious influence, literature, science, art, and handicraft he same institution.

ome amount of astronomical teaching is necessary in hool; it is very much neglected at present, because it s not pay in examinations. Yet we know by expe- ce that few subjects are so interesting as astronomy. in schools, as at Rugby, where geometry and hanics are taught, the noblest illustrations may be n from the mechanism of the heavens. But astronomy ot be taught by a book only. The most useful, indis- able instrument to an astronomical teacher is an y. No descriptions, no diagrams, give boys a con- on of the solar system so clearly as a few minutes an orrery. The next most useful instrument is a cope. And certainly Rugby seems to be fortunate in nstrument that is to be given to it.

e Temple Telescope is a noble one. The object-glass, in., was made with especial care and pains by Alvan e for Mr. Dawes. It is 8¼ inches aperture, and focal length. It is mounted equatorially, has an ent driving clock, of an unique kind; and the eye- s range from 92 to 1,000.

instrument like this is, of course, a luxury, but its y is, in itself, a great inducement to its use. There w things so wondrously beautiful as the moon, or Jupiter, or a star cluster, seen with a low power in such a telescope. And any one who has tried to show the moon to several people with an ordinary telescope will appreciate the advantage of having it equatorially mounted, and of its being provided with a driving clock.

Other instruments for astronomy, surveying, and meteorology, will be added to the observatory; some of the masters having provided a fund adequate to give so fine an instrument all the surroundings that are required to make it practically available. And ere long we hope to see a really useful observatory established.

It is to be under the joint management of Mr. Wilson, who munificently gives the telescope, and Mr. Seabroke, an old Rugbeian, who is already favourably known as a worker. He still has his spurs to win, however, and will soon be very favourably situated for winning them.

The telescope is now the property of the Rev. H. E. Lowe, of Atherstone, but will become the property of Mr. Wilson in March. We greatly regret to hear that circumstances which have lately happened at Rugby affecting the tenure of masterships will prevent for the present the gift of the observatory to the school, and can only hope that it will not be long before the present difficulties are overcome. Meantime it will be established on private ground, and will be accessible to the school. Verily there be head-masters and head-masters, and masters and masters!

L.

THE EXPERIMENTAL AND NATURAL SCIENCES IN TRINITY COLLEGE, DUBLIN

THE condition of the Experimental and Natural Sciences in the various Universities is at the present time a subject of such general interest that we give the following sketch of what is now done for them in Trinity College, Dublin.

Until a student passes the Michaelmas term examination of his second (Senior Freshman) year he is supposed to confine his attention to classics, mathematics, and logic; but in his third and fourth years he must devote himself, to a certain extent, to the study of Experimental Physics, including heat, electricity, magnetism, and chemistry, and pass examinations on these subjects even at the ordinary term examinations. In the fourth year of his studies the student can go in for honours in Natural and Experimental Sciences, the course for which includes Jamin's "Cours de Physique," Lloyd's "Wave Theory of Light," Naquet's "Principles of Chemistry," Cotta's "Classification of Rocks," and Haughton's "Manual of Geology."

At the conclusion of his collegiate studies the student can graduate in either Experimental or in Natural Science, and the Court of Examiners can recommend the most distinguished of the candidates to the Board for gold and silver medals; candidates thus recommended receiving their B.A. degrees as Senior and Junior Moderators respectively.

The subjects for examination for the Moderatorships in Experimental Science are (1) Experimental Physics viz., Heat, Light, Sound, Electricity, and Magnetism; (2) Chemistry, Inorganic and Organic; (3) Mineralogy, including Crystallography; and the hundred marks allotted to the examination are as follows:—Light and

Sound, 20; Heat, Electricity, and Magnetism, 30; Inorganic and Organic Chemistry, 15 each; Mineralogy and Crystallography, 10 each. It need only be added that lectures on all these subjects are delivered every term by the respective Professors.

The subjects of examination for the Moderatorship in Natural Science are the following, each of which has equal weight:—(1) Physiological and Comparative Anatomy; (2) Zoology and Botany; (3) Geology (including Physical Geography and Palæontology).

It may be objected that the distinction between Zoology and Comparative Anatomy will not hold good, and the books ordered to be read under both sections seem to demonstrate this. But there can be little doubt that the establishment of this Moderatorship is a step in the right direction, and the course is such that every medical student ought to take it up and do his utmost to attain a place among the golds.

It is of course not to be denied that there are no scholarships, no studentships nor fellowships to be attained by a knowledge of these sciences, but perhaps even these may in time come. It is not so very long ago when a student could aspire to but few distinctions if he were not a first-rate mathematician; now this is completely altered, and as the world rolls on changes come with it.

The chemical and physical laboratories of the College leave nothing to be desired. The distinguished Medical Registrar takes care that there shall be every facility given to students to work out the Comparative Anatomy of the Vertebrates, and places at the disposal of the College the animals that from time to time die in the Zoological Gardens. The Professor of Zoology demonstrates the Anatomy of the Invertebrates to his class during two out of the three terms. The Botanical Gardens and the Herbarium are as extensive as any University can require, and there are two courses, one of forty and another of twenty lectures, delivered each year in Botany, besides garden demonstrations.

In conclusion we venture to suggest that if the M.D. degree should only be taken by the reading and publishing of a Thesis, as in some of the German Universities, it would help materially to assist the cause of the Sciences in Trinity College, Dublin; for, though some of the candidates might select practical subjects, others, doubtless, would turn their attention to the wide fields of Zoology and Botany.

W.

SIR JOHN LUBBOCK ON THE ORIGIN OF CIVILISATION

The Origin of Civilisation and the Primitive Condition of Man: Mental and Social Condition of Savages. By Sir John Lubbock, Bart., M.P., F.R.S., &c. (Longmans, 1870.)

NOW that Sir John Lubbock's work on the "Origin of Civilisation" has reached a second edition, it is perhaps only natural that those who make it their business to warn the public against the encroachments of Science should raise an alarm against the first. In a recent number of the *Christian Advocate and Review* appears, accordingly, an article specially devoted to the demolition of Sir John's theories, and the vindication of human degeneracy. With the felicitous instinct of clerical antagonism, the Advocate and Reviewer makes his fiercest onslaught precisely where his opponent happens to be least vulnerable, and lays about him with all the fine, fervid imbecility distinctive of his particular clique. Such an attack, however, were ignorance its only characteristic, would hardly call for remark. We notice it, not for its absurdity, but because, in combining with its absurdity a certain unctuous disingenuousness, it is really a typical example of a kind of criticism unhappily influential, if obscure, and widely accepted, if not popular. It would perhaps be too much to expect that reviewers of this class should read through the books they review, but at least they have no right to misquote what they do read. On p. 256 (first edition), Sir John Lubbock, speaking of errors into which, in the absence of education, not even Christianity prevented mankind from falling, writes thus: "We know that a belief in witchcraft was all but universal until recently even in our own country. This dark superstition has indeed flourished for centuries in Christian countries, and has only been expelled at length by the light of science." He then proceeds to observe: "The immense service which science has *thus* rendered to the cause of religion and humanity, has not hitherto received the recognition which it deserved." His reviewer, omitting any reference to witchcraft, quotes Sir John as asserting that "the immense service which Science has rendered to the caus of religion and humanity, has not hitherto received th recognition it deserves"—a proposition which may or ma not be accurate, but is certainly not the one laid down b Sir John Lubbock. But he is not content with merel misrepresenting the book under review. Sir John Lub bock, he correctly remarks, at Liverpool, "frankly avowe 'there was no opposition between science and religio an admission," he adds, "of no slight importance by s great an authority in the scientific world, *as it is such quiet rebuff to the boast of Bishop Colenso, that t differences between these two are such as to render hopeless to attempt their reconciliation.*" If, however, w are bewildered at the brisk audacity which could ventu on such a statement without even a hint at its who fabulous character, what are we to say to a critic w gravely asserts that "the Drift age had not been invent at the time" when Sir Charles Lyell wrote his "G logical Evidence of the Antiquity of Man"? and w assigns to the "prehistoric period in Sweden a minim antiquity of 20,000, or it may be of 20,000,000 years."

But enough of the *Christian Advocate.* Turn we n to other and nobler opponents. The conclusions ma tained by Sir J. Lubbock in this work are, in his words—

"That existing savages are not the descendants civilised ancestors.

"That the primitive condition of man was one of u barbarism.

"That from this condition, several races have inde dently raised themselves."

On the other hand, we have the opinion of the Archbishop Whately, that "We have no reason to pose that any community ever did or ever can em unassisted by external helps, from a state of utter barism into anything that can be called civilisation;" that of the Duke of Argyll, who holds that the prim condition of man was one of civilisation; that "th no necessary connection between a state of mere childhood in respect to knowledge and a state of utter barbarism," and that man "even in his most civilised condition, is capable of degradation; that his knowledge may decay, and that his religion may be lost."

That the general propositions laid down by Archbishop Whately and the Duke of Argyll contain a certain limited amount of substantial truth, will probably be admitted by the staunchest adherents of the opposite theory. That "external helps" of some kind or other have played a most important part in the case of all civilisations the history of which is accessible, is as little open to question as the fact that under certain conditions civilisation among certain races may be arrested or may even retrograde. At the very threshold, however, of any discussion in terms less general, we are met by the question "What is civilisation?" The baffling complexity, indeed, of the idea conveyed in the word "civilisation" is the fountain-head of most of the confusion which exists among writers on the subject. That development is the vital principle, so to speak, of civilisation is universally admitted, but there would probably be a very general disagreement of opinion as to the particular kinds and directions of development which constitute the essential elements of civilisation. As generally understood, civilisation appears to involve a development more or less advanced of commerce and the means of communication, of natural advantages, products, and wealth, of navigation and warfare, of the arts, mechanical and ornamental; of science, theoretical and practical; of legislation and the administration of the law; of customs and language; of morals and religion; of all the faculties of the individual and the race. It includes also a consideration of the diffusion of personal liberty, and of the proportion of those who participate in the general welfare and possess the necessary appliances both for physical comfort and intellectual culture.

This, of course, is an inadequate definition of civilisation; and it is further manifest not only that development in many of the directions indicated is not absolutely necessary to civilisation, but that no civilisation on record has been equally developed in every direction. What is still wanting is some standard by which to measure civilisation in any particular case. Mr. Wallace, following Montaigne, appears to consider civilisation compatible with a very low development in nearly every direction. Archbishop Whately would consider as civilised the Germans described by Tacitus. The Duke of Argyll goes further still, for he seems to consider that Adam and Eve when expelled from Paradise were, nevertheless, distinctly civilised beings. The diversity of opinion is, indeed, owing to the absence of a recognised standard, almost universal. Civilisation is nearly always measured by the recorded achievements of men of genius. Yet, if this were the true test, no nation of modern Europe is so highly civilised as was Greece in the age of Pericles, and English civilisation has been retrograding from the days of Elizabeth, nay, from those which gave us the Canterbury Tales and Lincoln Minster, if not from those of Anselm and the Norman Bastard. Another fruitful cause of error is the natural but illogical assumption of the superiority of our modern Western European civilisation over all other civilisation. That in certain respects, principally material, it is actually superior, we do not of course deny, but when we contemplate the condition of our criminal, our pauper, our agricultural population, the troglodytes of the city, and the nomads of the country, it is difficult to avoid the conclusion that other civilisations, less advanced in certain respects, were more advanced in others, perhaps in some cases to an extent which turns the balance in their favour. On the whole, therefore, we apprehend that the relative civilisation of any country must be estimated rather by the sum of its general development than by its development, however advanced, in any particular direction. And this leads us naturally to a standard which, wherever it can be applied, is an infallible indication of the general civilisation of any race. General civilisation involves the multiplication of ideas, and the multiplication of ideas involves the multiplication of the symbols which express them. The language, therefore, or more strictly speaking, the vocabulary of any race, becomes a crucial test of its development. The total disappearance of numberless languages and our necessarily limited acquaintance with those which survive, obviously diminish, not only the number of cases in which this test can be applied, but the certainty of its application in particular instances. In spite of these drawbacks, however, language still supplies material to the student of comparative civilisation, not less invaluable than the material supplied by geology to the student of comparative anatomy. The imperfection of the record in both cases is extreme, but in both cases, so far as it extends, it is authentic and decisive.

We now return to the controversy between Sir J. Lubbock and his opponents. Independent of the cogent arguments adduced by Sir J. Lubbock against the conclusions arrived at by Archbishop Whately, there is one which seems to have been altogether overlooked. The Archbishop's theory traces the history of mankind up to a single primeval pair, and assumes the impossibility of their survival after their expulsion from the Garden of Eden unassisted by some supernatural revelation. Some supernatural revelation of the same kind he also holds necessary in order to raise any race to that stage of culture at which it is enabled to make progress of itself. The perfectly gratuitous character of this hypothesis seems to us its sufficient refutation. Surely it is sufficient to believe that causes analogous to those which, in later ages, gave to the world the exceptional intellects of an Aristotle or a Newton, possessed potency enough at an earlier epoch to account for the appearance of men endowed with genius to make the successive inventions recorded, without resorting to a superfluous hypothesis of supernatural intervention.

Even the Duke of Argyll virtually abandons Whately's position, although, perhaps, his own is even less logically tenable. When he tells us that there is no necessary connection between a state of mere childhood in respect to knowledge and a state of utter barbarism, we are forcibly reminded of Mr. Phœbus's eulogy on the aristocracy of this country, whose strongest points he declares to be that they live in the open air and speak only one language. It is manifest that the Duke of Argyll when he penned this passage had in his mind's eye the ideal "noble savage," who has figured so picturesquely in works of historic fiction from the days of Anacharsis downwards—a being who, although represented as in "a state of mere childhood in respect to knowledge," meets the greatest of civilised monarchs as his inferior, and convicts of folly the acutest of civilised philosophers. Unhappily this charming ideal person eludes the search of authentic travellers. They tell us of savages whose presence and bearing stamp them as gentlemen, they record many keen and pregnant sayings of barbaric wisdom; but the possession of the capacity for civilisation thus manifested is a very different thing from the possession of civilisation. Manly courtesy, strong commons ense, many of the moral virtues, are as compatible with a state of barbarism, as the absence of all these qualities is compatible with a state of highly-advanced civilisation. On the other hand, civilisation necessarily implies a familiarity with certain ideas to which "a state of mere childhood in respect to knowledge" is equally of necessity an utter stranger. In fact, both Archbishop Whately and the Duke of Argyll seem to have been the victims of a wholly imaginary necessity. They appear to have forgotten the syllogism implied in the old rhyme,

When Adam delved and Eve span,
Who was then the gentleman?

and have felt themselves under an obligation of crediting our first parents with a degree of civilisation utterly at variance with any accepted record of their condition. As Sir J. Lubbock observes, "Adam is represented to us in Genesis not only as naked and subsequently as clothed with leaves, but as unable to resist the most trivial temptation, and as entertaining very gross and anthropomorphic conceptions of the Deity. In fact, in all three characteristics—in his mode of life, in his moral condition, and in his intellectual conceptions—Adam was a typical savage" (p. 409, note). It may be added, too, that Adam's naming the beasts and birds is by no means incompatible with his otherwise barbaric condition. "It is remarkable," says Sir J. Lubbock, "that, supporting such a view, the Duke should regard himself as a champion of orthodoxy."

With regard to the question of degradation, however, the Duke has a slightly stronger case, though he has hardly made the most of it. That decline as well as progress in civilisation does really go on in the world is a historic fact beyond dispute. Egypt and Assyria, Greece and Rome, Mexico and Peru, groan with the monuments of ruined civilisation, and all history bears witness to periods of stagnation and decadence following on periods of progress and development. Nor is evidence wanting of an analogous sequence of events among the lower races. Degeneration is known in some instances to have taken place as the result of crossing ill-matched breeds; in others as the result of conquest, when the conquering tribe is in any respect less civilised than the conquered; in others, owing to the oppression of other tribes; in others, by the expulsion of a tribe into less favoured territories; in others, by a change in the external conditions of life,—in short, the whole of our present knowledge and experience tends to show that in every stage of civilisation from the lowest to the highest, development may be and frequently is succeeded by decline of greater or less duration and degree. So far we quite agree with the Duke of Argyll; and although Sir J. Lubbock admits the fact of occasional degradation, he hardly seems to us to recognise its real frequency and extent. In fact, however, it is only less universal than progress. For just as the present population of the world represents the difference between all preceding births and deaths, so the existing civilisations of the world represent the difference between all foregoing developments and declensions in civilisation. In other words, the present civilisation of the world bears witness to a vast mortality among previous civilisations, much in the same way as the present population bears witness to a vast mortality among previous populations. This consideration, however, so far from being favourable to the gloomy views of the Duke of Argyll, tells, on the whole, manifestly in favour of Sir J. Lubbock's more hopeful theory, for the tendency of civilisation like that of population, is always to increase and multiply. In both cases, certain conditions may and do counteract the tendency in certain times and places, but the tendency remains the same, and sooner or later always predominates on the whole. It thus happens that all the great civilisations of the world have been in some material respect in advance of any which preceded them, and at the same time have manifested development in a greater number of directions. Thus the civilisation of Greece is more complex and more advanced than that of Egypt, that of Rome than that of Greece, that of Elizabethan England than that of Rome, that of modern England than that of Elizabethan England. It is to be observed, however, that every civilisation has some special and distinctive glory of its own unsurpassed by any of the subsequent ones. Grecian art, for instance, of certain kinds has never since been equalled, but the student of the Roman rule and law will certainly not be disposed to rank Grecian civilisation as a whole so high as the Latin. In fact, loss of some kind accompanies every gain of civilisation. One savage possesses the eye of the vulture, a second the scent of the deer-hound, a third the fleetness of the stag—civilise these men, and you destroy their special characteristics of excellence. Nor is this true only of physical qualities. Civilised man knows nothing of that barbaric power of perception and memory which enables the savage to detect at once the loss of one sheep out of three hundred, though he cannot even calculate the number of his fingers. Such losses, however, are in the long run more than compensated by gains in other directions, just as the losses incurred by the decay of one civilisation are eventually more than compensated by the benefits conferred by another. We fully agree, therefore, with Sir J. Lubbock in his remark at the conclusion of his answer to the Duke, "that the past history of man, has, on the whole, been one of progress, and that in looking forward to the future, we are justified in doing so with confidence and with hope."

But we have hitherto said nothing about Sir J. Lubbock's book itself. When we remember that it is one of the first attempts to treat the Origin of Civilisation on a rational and philosophic basis, we are not disposed to quarrel greatly with its somewhat lax arrangement. Its necessarily miscellaneous character lends it no small part of its value, and renders it exceedingly readable, but a more rigorous method and proportion are required to render it easy of complete digestion. In his laudable anxiety, too, to collect and co-ordinate facts as the only trustworthy foundation of his hypothesis, Sir J. Lubbock himself has a provoking way of latitating for a whole chapter together behind a heap of quotations, just when we want him to tell us their precise significance in that particular connection. This, indeed, and a very general absence of dates, deprives many of his facts of some portion at least of their intrinsic value. Occasionally, too, we come across a statement which we want verified, as, for instance, at p. 283, where he tells us "it is said that among the Ancient Britons money was habitually lent on what may strictly be called 'post-obits,' promises, to pay in another world." We own that we should like to see the authority for the prevalence of so singular a commercial transaction among our grandfathers. So far as we know, the Bonzes of Japan, and not the early Britons are the real culprits. A letter from a Jesuit father in Japan, dated March 1565, printed in Maffei's collection (B. iv. 2), tells how certain Bonzes were in the habit of borrowing money to be repaid with interest in another life, and giving their creditors I. O. U.'s (syngraphas) for *post mortem* presentation.

After all, however, Sir J. Lubbock's work is the completest summary of barbaric life that we possess. It does not profess to be exhaustive. It is designed rather as a breaking of the ground for further research in a direction precisely opposite to the "high *priori* road," on which theorists about the origin of civilisation have walked so long towards nowhere, discoursing prettily about the family being the first of human institutions, language being the perfect instrument of primeval thought, and so forth. In this respect, as well as in being a handbook of facts nowhere else collected together, it is undoubtedly a most useful contribution to contemporary literature. Its main value, however, consists in what we have only lately found a word for,—its "suggestiveness." Very few, even of the "cultured classes," at all realise the profound and abject barbarism of primeval antiquity. We can only guess darkly at the life of those wild ancestors of our race who fashioned and wielded the flint tools of the drift while yet the Thames was tributary to the Rhine,—how they skurried to their caves or burrows from the wolf, the bear, and the tiger, kept watch on the rhinoceros snorting in the shallows, or trembled as the mammoth herds crashed through the jungle. What were the relations among them of man to man, of man to woman? Was the one generally either the slayer or the victim,—the other either the temporary slave of an animal lust or the material of a fireless feast? Had they language beyond the scream of terror or pain, the shriek of triumph, the chattering of menace, the muttered mumbling of gratified gluttony? Who can tell? We know only that between the lowest savages of to-day and their earliest ancestors lies an interval of years far beyond the limit of historic chronology; and carefully weighing the facts of the case, we find it on the whole one degree less inconceivable and incredible that they should have risen to their present level of utter barbarism from one still lower, than that they should be the degraded progeny of any known or unknown civilisation.

SEBASTIAN EVANS

OUR BOOK SHELF

A Sketch of the Life and Writings of Robert Knox, the Anatomist. By his pupil and colleague, Henry Lonsdale. (London: Macmillan and Co., 1870).

DR. KNOX was in many ways a remarkable man, and if his life had been written with greater clearness of statement and less redundance of language, it might have been made both interesting and instructive. But those who have read Dr. Lonsdale's life of Professor Goodsir will not be disappointed by the present volume.

Robert Knox was born in 1791, and his misfortunes began by an attack of small-pox which destroyed his left eye. He was educated at the High School of Edinburgh, and became a student of medicine in the University when nineteen years old. Five years later he was appointed assistant-surgeon in the army, and spent more than three years on duty in the Cape Colony, where he seems to have made his first studies in ethnology and natural history. He next studied at Paris, and after his return to Edinburgh in 1822, became curator of the Museum of the College of Surgeons. In 1825 he joined Dr. Barclay in his extra-academical lectures on anatomy, and at once took the highest position in Edinburgh as a lecturer. He had many distinguished pupils—Goodsir, Reid, Edward Forbes, Owen, and Falconer were among them—and he appears to have been as popular with the students as he was disliked by most of his colleagues. In the winter of 1828-9 the terrible discovery was made of the system of murder carried on by Burke and Hare; and one of their victims was traced to Knox's dissecting room. This exposed him to much opprobrium, and even to personal danger from the Edinburgh mob; but his reputation was fully cleared from any suspicion of complicity in these crimes by the report of a committee of inquiry on the subject, which Dr. Lonsdale prints at length, together with a moderate and manly letter written by Knox himself to the *Caledonian Mercury*. That this affair was not the real cause of his leaving Edinburgh is admitted by his biographer, and amid the chronological confusion of the whole book the reader is left to guess the causes which reduced a class of five hundred in 1832 to nothing in 1842. Knox had been a candidate for the University Chair of Pathology in 1837, and for that of Physiology four years later. He failed each time, and the letters in which he submitted his claims give abundant reason for the enmities with which he surrounded himself. The latter of these productions, full of personal abuse of eminent men, some of whom are still living, was discreditable to Knox at the time, and its publication in this volume is still more so. We are told that "regardless of both legal and moral obligations, he commenced lecturing on anatomy in Edinburgh in 1842, but got no class." He tried lecturing at Glasgow, failed again, and after various wanderings settled in London. Here he maintained himself as a popular lecturer and a literary hack. Among his other occupations during the last melancholy twenty years of his life, he was pathologist to the Cancer Hospital, he contributed to the Proceedings of the Anthropological Society, and he practised midwifery. Hard working to the last, he died in London at the age of seventy-one.

The moral of this life seems to be a very trite one; Knox was his own enemy. In spite of a biographer's adulation, we are told that "on matters of business involving a *bonâ fide* principle, Knox was prone to be evasive, whilst on matters of fact he was not always considered trustworthy."[*] Yet he must have had redeeming qualities, and made warm friends as well as bitter enemies. As an anatomist, he belonged to the older British school and possessed many of its merits. He did good work on the *Cetacea* and *Sirenia*, and appears to have been the first to maintain the true nature of the ciliary muscle, as well as to describe the *fovea centralis* in the retina of reptiles, and the tracheal pouch of the emeu. He was also a leader in the study of ethnology as a branch of anatomy. But his great distinction was as a lecturer, and here his success was for a time unequalled. Like other teachers of the time, he had the advantage of introducing a great deal of comparative anatomy, physiology, and histology (so far as it was then known) into his lectures on human anatomy. He must have been gifted with great natural eloquence, and disdained none of the tricks of oratory. His biographer gives a wonderful account of the clothes he wore when lecturing, in what seems to have passed for "full dress" in Edinburgh about 1830, but also informs us that "the richest costume would hardly have availed had not the silvery tongue been brought into play." Knox never drew diagrams, and equally rejected the help of ready-made drawings for his class, nor did he ever use notes. Above all, he thoroughly enjoyed his work. He left little written that is likely to preserve his fame. His translation of Milne-Edwards' "Zoology," by which he is perhaps most widely known at present, is a bad specimen of his inferior work. The history of his life, besides its interest as the record of an able man's remarkable success and as remarkable failure, is of importance from the light it throws on the study of anatomy in this country, on Scottish university politics, and on methods of scientific teaching; so that the intention of the present work is more praiseworthy than its execution. P. S.

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[*] This statement is introduced by the remark that "a portrait of the anatomist, without its shadows and sinuous lines, would be no portrait at all." No doubt there were many such portraits of Knox on the blank walls of Edinburgh, when he was abused as the accomplice of Burke and Hare, but Dr. Lonsdale has done wisely in giving his readers the characteristic sketch of the anatomist lecturing, which was taken by his brilliant pupil, Edward Forbes. This sketch is the best thing in the book.

Strange Dwellings: being a Description of the Habitations of Animals. By the Rev. J. G. Wood, M.A., F.L.S. (Longmans & Co., 1871.)

MR. WOOD'S works are well and favourably known as presenting the study of Natural History in its most attractive form to the young. The present work is not new, being abridged from his "Homes without Hands," which obtained, some years ago, a deservedly large amount of popular favour; and even in its original form it consisted of course mainly of selections from accounts given by travellers and naturalists. It is, however, extremely well suited to place in the hands of any boy or girl, who is already fond of reading about strange animals, or whom it is desired to interest in the study of nature. We find in it accounts of the curious trap-door spider of Jamaica, the bower-bird of Australia, with its remarkable edifice of twigs and shells, the mud wasps of Guiana, the repulsive-looking aard vark of South Africa, the weaver bird and tailor bird, and many other animals of singular habits, and illustrated with woodcuts, which combine with the lively style of the text to make the volume a very attractive one.

The Duke of Edinburgh in Ceylon: a book of Elephant and Elk Sport. By John Capper, *Times* correspondent. Illustrated with chromo-lithographs. (London: Provost & Co., 1871.)

THIS book is sufficiently described by its title, being a record of the visit of the Duke of Edinburgh to Ceylon last year, and of his success in the colonial sports of elephant hunting and elk hunting. It appeals to two sections of the public, those who eagerly seize upon every incident connected with the mode of life of any member of our Royal family, and those who are equally eager after any description from life of sport in those countries where wild beasts worthy of a hunter's rifle abound. We may quote the following as an instance of the perils encountered by our Prince in navigating the Cingalese rivers. "The stream was teeming with life. Fish of all varieties and sizes sprang into the boats as they paddled along, one of them finding its way into the Prince's coat pocket" (loyal fish!); "on all sides could be heard the snapping of alligators' jaws as tiny fish were caught in the monsters' mouths. The party had proceeded about a mile down the stream, when one of them, leaning down and resting his head on the gunwale of the boat, was startled from his quiet rest by the apparition of an alligator's gaping jaws, which made a direct snap at his head, fortunately missing it, but seizing, in place of it, the barrel of the rifle held in the hands of the Prince's English attendant, who was seated next to him, and which the monster nearly wrenched out of his hand, splashing the water about, and drenching every one in the canoe." Is the *Times* correspondent quite certain that alligators are found in Ceylon?

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Lunar Halo seen from Two Stations

I HAVE presented to the French Institute at its last sitting a copy of NATURE, January 26, with the sketches of a halo seen from Liverpool by two different observers from two stations at a little distance. It is the first time, as far as my knowledge goes, that we have had two different sketches of the same phenomenon. The difference is very considerable indeed, as a *paraselene* was seen by one of the observers, and not by the other. It would be very important to ascertain what was the exact situation of each of them, and I should be glad if you can take the trouble to ascertain it by an inquiry through your paper. When I return to England I will ascertain the circumstances of the observation, which may lead to some definite conclusions on the form and altitude of floating snow during the night of the 4th January.

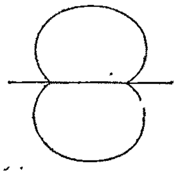
In my note to the French Institute, I say that, unquestionably, reflexion or refraction took place on several faces of crystalline snow, and each observer saw the refraction or reflexion on a particular face of crystal, placed conveniently for observation. So that double observation is very valuable, as affording a direct proof of the correctness of the explanations given by Mariotte, and others after him, of these magnificent appearances.

According to Mariotte's theory, the presence of a paraselene in one of the observations with the halo of 22° shows the presence of prismatic crystals, the faces of which must be hexagonal. The second appearance should, according to my opinion, show that these crystals were terminated by small pyramids, and the other observer perceived the halo reflected on the oblique face of these crystals. As he saw two or three halos almost concentrical, it must be supposed that one of them was due to the upper pyramid and the other to the lower.

Very probably the air was not disturbed by any wind, and elongated crystals were falling very slowly, or rather floating, owing to the smallness of their dimensions, which possibly may have been a small fraction of $\frac{1}{100}$ inch.

I send you these suggestions only to direct further inquiries, and I make no pretension to exhaust the subject, which is very interesting.

I myself observed on Thursday, February 12, what is not quite unworthy to be noticed, as showing how inexhaustible is the field opened by Nature to her inquirers. The sun was setting when I arrived at St. Pierre le Calais with my friend Alexandre Lille. I noticed the disc was distorted, the two partial suns being almost alike, and of a red colour. This appearance was certainly owing to the air not being of a uniform density. Two different streams were separated by a horizontal surface. The truth of this supposition was very easily ascertained, as two different kinds of clouds were flying in two different directions. Neither of them was heavy, and the distinction was very admirably made by the sun itself, which soon disappeared, leaving a rosy tint behind him. The lower clouds first presented a rosy colour, but soon became dark, and the upper clouds in their turn took the beautiful colour which the others below were just losing. The horizontability of the surface separating the two streams of air was a proof of great quietness in the atmosphere, and the night was magnificent, as well as the following day.



W. DE FONVIELLE

Quinary Music

MR. JEVONS, in his interesting paper on the Limits of Numerical Discrimination (NATURE, Feb. 9), asserts in support of his views (Rees Cyc. "Rhythm") that "no musicians have yet been found capable of performing" quinary music.

I have never found the slightest difficulty, nor can I conceive any, either in performing or inventing music subdivided into five isochronous measures, *i.e.*, with an accent recurring on every fifth unit of measure; nor do I see anything to justify his doubt that the ear can grasp divisions of 6, 8, 9, without regrouping them into smaller periods; as amongst skilled musicians there exists no doubt whatever about the continuous reading of those larger groups, without even a suggestion of such subdivision; and it would be easy to multiply quotations from the best writers of passages whose only correct performance *and reception by the listeners* would be entirely destroyed by such a sub-grouping as Mr. Jevons seems to think necessary.

Apropos:—I greatly doubt whether the question of musical time is at all pertinent to the subject of numerical discrimination, as an instantaneous conception; the latter being a synchronous mental act, while the former is altogether consecutive in its operation, in which every group, however small, is only a sequence of units. JOSEPH MULLEN

38, Synge Street, Dublin, Feb. 14

The Power of Numerical Discrimination

IN an article with this title in a recent number of NATURE, Mr. W. S. Jevons offered the results of some ingenious experiments he had been making to determine how many objects the human mind could count by an instantaneous and apparently single act of attention. He comes to the conclusion that the power of his mind was limited to something less than five.

If it were Prof. Jevons' purpose to ascertain the number of objects he could count within an interval too short for more than a single conscious act of attention, his experiments were, doubtless, conclusive, at least to him; but if he sought through them to prove that he or any other person could fix his attention upon more than one object at a time, I fancy he commits the mistake attributed to the Royal Society in puzzling over the question put them by Charles I. about the effect of throwing a shrimp into a pail full of water. He is trying to account for a phenomenon that never occurred and which can never occur.

Prof. Dugald Stewart, in his work on the "Philosophy of the Human Mind"[*] has proved very clearly, it seems to me, that the attention is never fixed upon two points or objects at the same time, but that it passes from one to another in certain cases, as in playing upon musical instruments, in feats of jugglery, &c., so rapidly as to seem to be instantaneous.

In addition to what Prof. Stewart has said upon this subject, permit me to ask how it is any more possible for the attention to be fixed on two beans at one time than for two beans to be in the same place at the same time? The argument that could demonstrate the absurdity of the last of these propositions would demonstrate the absurdity of the first.

If Mr. Jevons will try to look at both eyes at once of the first person he talks with, he will find that one of the eyes seems more distinct to him than the other, and every effort he may make to equilibriate his attention will only result in changing it from one to the other.

If he is talking with great earnestness, or in a way to make his interlocutor very anxious to divine his meaning and penetrate or anticipate the expression of his inner thoughts, he will notice that his interlocutor's eyes seem to be running from one of his own eyes to the other, as if in hopes of getting from one some disclosures not made by the other. This would not be done if both eyes could be seen simultaneously.

Mr. Jevons seemed to see five beans, because he was able to run over and count five in the minimum of conscious time.

I think there is no authority for saying that there is any period of conscious time necessary for any purely mental operation. To the mind itself, or the spirit of a man, there is neither space nor time. There are incidents of our material organisation which limit our capacity to notice and remember mental operations, but not the operations themselves. Therefore the rapidity with which the attention is transferred from one word to another in reading, or from one key to another of a piano when played by a master, authorises no presumption whatever that his attention is ever fixed upon more than one key at a time, while all the presumptions are against the possibility of any person's attention ever being in two places at the same moment. J. B.

Berlin, March 4

Eozoön Canadense

ON a careful consideration of Dr. Dawson's reply to the objections urged by me against the supposed organic nature of Eozoon Canadense, I confess my inability to see that one single fact is brought forward calculated to shake the position of those who regard it as a purely mineral production.

In opposition to all previously received opinion, Dr. Dawson would now confine Eozoön to the Laurentian period. I am glad to accept this as evidence that its Canadian discoverers begin to feel the force of the "difficulties" I have stated, and instead of ignoring them, make a genuine and *direct* attempt to meet them.

Though Dr. Hunt now doubts the accuracy of the observations which refer the Skye ophite to the Lias, he has elsewhere as good as admitted that it is not confined to the Laurentian period.[*] Gumbel has also determined it for Cambrian metamorphic rocks in the Fichtelgebirge, Bavaria, and Rupert Jones and Sandford for rocks of the Lower Silurian period in Connemara, as pointed out by Mr. Kinahan in his letter in NATURE (No. 66). The Tudor specimen, it is also considered, may belong to the Cambrian or Potsdam group. This, to say the least, is somewhat contradictory.

The determination of the age of the Skye ophite I am willing to leave Dr. Hunt to settle with Professors King and Rowney, merely remarking that both McCulloch and Geikie, as independent observers not looking for evidence in support of a theory, declare the rock to be of the Liassic age.

The lengthy disputes as to what is to be considered Eozoön and what is not, are most amusing. When each disputant takes up a different position and shifts it as occasion requires, how is he to be met? And is this not of itself sufficient *primâ facie* evidence of weakness such as to warrant a suspension of judgment on the part of those—and their number is great—who have accepted the "fossil" only on the strength of eminent names and reiterated assurances?

As far as I can make out, the whole positive evidence is now narrowed down to the determination of what is and what is not the true "nummuline layer." A reference to the published figures and descriptions gives no information by which we may detect any difference between the "nummuline layer" of the Skye ophite and that of the typical Canadian specimens. Thus, then, until Dr. Dawson points out the difference, this objection cannot be said to be "wholly irrelevant." To aver, without proof, that when the characteristic structure occurs in an unlooked-for position, that it is an *imitative form*, or, on the contrary, to assume it to be a *fossil* when discovered elsewhere, is easy, but does not tend to carry conviction to the unbiassed mind. To do this, we require distinct and ample evidence. The Eozoön before referred to as discovered by Mr. Sandford in the Connemara ophite, and "verified" by Rupert Jones,[†] belongs, according to Murchison, to the Lower Silurian age.[‡] The discovery, it appears, had at the time induced Sir Roderick to class this rock as Laurentian, but shortly afterwards, purely from stratigraphical considerations, he pronounced it to be Lower Silurian.[§] Here, again, we have a discordance with the views of Dr. Dawson; are we, then, to throw away such independent testimony, and say that the unfortunate Eozoön "Hibernicum" is an *imitative* form, or, are we to consider the veteran geologist wrong, and the Connemara marble Laurentian?

It is now seen that all the theories which attempt to meet the objections I have stated are in conflict—which then is right?

As regards the Tudor specimen, which, it is thought, I have too summarily "disposed" of, I would observe that it was brought forward with great *éclat* as a conclusive answer to all objections founded on the comparison of the structures of Eozoön with the forms of fibrous, dendritic, or concretionary minerals. The reasons why I dissent from this view are: 1st. The "chambers" are admittedly "more continuous and wider in proportion to the septa" than specimens found elsewhere. They are, in fact, little more than an aggregation of concentric plates or perhaps only bands, and according to the figure do not show the true segments. 2nd. The microscope reveals "for the most part merely traces of structure consisting of small parts of canals filled with the dark colouring matter of the limestone," and in only "a few rare instances" are detected "with a higher power in the margin of some of the septa traces of the fine tubulation characteristic of the chamber wall of Eozoön." 3rd. It is almost an isolated example, and the measure of the metamorphism of its matrix together with its character—organic or otherwise—(from its generally doubtful nature as shown by Dr. Dawson's own description) could only be ascertained and settled by independent inquiry.

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[*] Works of Dugald Stewart. Edited by Sir William Hamilton. Art. "Attention."

[*] Silliman's *American Journal*, July 1870

[†] Geological Magazine, vol. ii. p. 87.

[‡] Ibid. p 147. [§] Ibid. p. 97.

I may also here observe that other cases of "chambers and canals stated to be injected with calcite appear to me to be of a no more reliable character. If, indeed, we accept the Tudor specimen and Madoe fragments as Eozoön, why refuse this term to the Skye specimens which apparently possess the true features (chamber casts and nummuline layer) in a much more marked manner? As regards imitative forms, Dr. Dawson and myself are in agreement, excepting that I must contend, from all the facts we are acquainted with, that *all* Eozoönal forms are imitative, and not merely those that the exigencies of the discussion demand looked at from the organic stand-point.

Respect for your space prevents me going into further details, but I may be permitted to suggest that the truth of the matter in no way hinges upon the possibility of comprehending the constructive pseudomorphic theories advanced by King and Rowney. In what way though, excepting by pseudomorphism, I would ask, does Dr. Dawson account for the presence of the *imitative forms* which he thinks have confused other observers? And if we believe pseudomorphism to have originated them, why is it so trying to our faith to consider Eozoön Canadense to have been formed in the same manner? The fact is, chemical geology and the replacement and alteration of minerals—occurring as they do in the forms of other minerals—have been little studied by palæontologists, or probably Eozoön might have remained "unconstructed" to this day. It is well known that not only do minerals assume by replacement the crystalline forms proper to the mineral replaced, but also amorphous shapes filled with one mineral may be replaced by another without in any way destroying the original form.

There can be no doubt that a little knowledge of this kind would have infused the necessary caution, and have prevented anyone accepting as a fossil that which required the invention of a method of chemical deposition (excepting in these pages) hitherto unknown in nature. Serpentinous marble, as we may ascertain from all sources, is the typical Eozoönal rock, and, though the minerals filling the so-called chambers and canals may be Loganite or pyroxene, in addition to the serpentine, they are intimately related in a pseudomorphic point of view. Loganite and serpentine, as I have before stated, are both products of alteration. The organic hypothesis demands that we should consider the infilling to be, as contended for by Dr. T. S. Hunt, the *same mineral originally supposed to be deposited therein.* If this be so, then what becomes of the meaning of metamorphism? This to me is a trial of faith greater even than the acceptance of King and Rowney's "constructive criticisms."

I await with interest the publication of the papers sent to the Royal Irish Academy by Dr. Dawson and his colleague, Dr. Hunt, and trust they may contain new matter for consideration, as my only object has been to elicit further evidence, if there be any, in favour of the organic hypothesis.

Feb. 17 T. MELLARD READE

Ocean Currents

IN reference to Mr. Laughton's letter in NATURE of the 23rd of February, I must admit that the question of the movement of barometric depressions was not introduced into my first letter, for the reason that I did not anticipate the objection of a state of equilibrium which he raised, since the average differences of pressure only were dealt with; but I do not see that it necessitates a change of ground to show how this difficulty is met by the variations which occur in the region of lower average pressure, and how these changes themselves, taking place in a certain line of movement, might affect the surface currents of the ocean; and I am not disposed to accept the sweeping rejection of the whole power of differences in the atmospheric pressure, permanent or moving, which is contained in the last paragraph of the letter referred to, until more extended observations shall show what directions the great movements of changing pressure take in passing over the parts of the ocean which lie outside of the trade wind regions.

The action of a barometric depression in moving over the sea differs entirely from that of the winds in this, that by the former the level of a large area of the ocean surface may be raised and carried along with the depression round which the winds blow, whilst by the force of the latter the waters can only be drifted at the same level before the wind.

I have been particularly careful to suggest difference of atmospheric pressure only as a supplementary power in the production of ocean streams, not as a chief one, and it was indeed the partiality of the various theories of the causes of currents which led me to open the subject at all.

That the trade winds have a very large share in originating the Gulf Stream is undoubted, but that they can account for the whole of the phenomena of ocean currents, as Mr. Laughton maintains, appears more than doubtful.

The existence of the under outflowing current of the Mediterranean, corresponding to the inflowing surface stream, has been abundantly proved by the recent Government expedition under Dr. Carpenter. If this current be due to the action of the winds alone we should expect to find the direction of the upper and under streams reversed with a change of wind to opposite points east or west in the Strait. But the observations on the direction of the winds for six years at Gibraltar show that westerly winds (from N.W., W. and S.W.) prevail there for 198 days in the year, and easterly winds (N.E., E., and S.E.) for 144 days; further it happens that in the months of July, August, and September, in one of which the observations on the outflowing under-current were made, east winds prevail in the ratio of two days to one day of west wind. Experiments similar to that used to determine the presence of an under-current in the Strait of Gibraltar, have been made in the open ocean, and Maury (at p. 206 of the 9th edition of his work) quotes an instance of an apparatus constructed of a block of wood, sunk by weights to 500 fathoms, and attached by a line to a small floating barrel, having moved off "against wind and sea, and surface current." The members of the late German Arctic expeditions have observed that where the warmer blue waters moving from south-westward meet the impure waters of the Spitzbergen and east Greenland current, there is a definite line of demarcation which would indicate that the Atlantic water here dips down beneath the specifically lighter water of the ice-bearing current, a conclusion which is supported by the increase of temperature with the depth beyond this point.

Such under-currents can in no way be primarily caused by the action of the winds, and if difference of temperature and density must be called in to account for them, it must be admitted that these causes have to do with the upper streams also.

I would take this opportunity to correct a statement made in my second letter, of a probable movement of a barometric depression across the British Isles at the rate of ninety miles an hour. The depression there referred to appears to have had an oblong form, the longer axis moving nearly parallel to the length of the British Isles from N. to S., so that the record of its passage took place at Valentia and at Aberdeen within a short interval of time, thus giving an apparently great rate of travelling. But I have the authority of the Secretary of the Meteorological Office for the facts given beneath, which prove that a rate of depression movement of upwards of seventy English miles an hour may take place. On the 16th of December, 1870, a minimum reading of the barometer was registered at Valentia at $2^h\ 45^m$ P.M.; at Kew, at $9^h\ 30^m$ P.M.; at Yarmouth, at 10^h P.M., where the mercury remained at the same level for four hours. The interval of time between the registrations at Valentia and Yarmouth is $7^h\ 15^m$; the distance between these places is 520 miles. It seems probable also that the centre of the depression moved directly from Cork to Yarmouth, for the wind records prove that it passed north of Falmouth, and south of Holyhead and of Valentia.

KEITH JOHNSTON, JUN.

Perpetual Motion

PROBABLY your sense of justice will induce you to insert some very brief remarks on your review of my article in the *Quarterly Journal of Science*. The tone of the review is a penalty which all who venture to impugn commonly accepted theories must be prepared to submit to. Heresy in science meets with as little mercy as heresy in theology. I confess that in one sense of the word I am consciously a perpetual-motionist, but not in the sense of believing that any merely mechanical contrivance can produce perpetual motion. That there are forces in nature which can and do produce it, is a matter of daily, yearly, and secular experience. If I am a perpetual-motionist in this sense, I am in good company. You will find that Sir W. Thomson, in the *Philosophical Magazine* for February 1854, described a machine by which a steam-engine or water-wheel could produce thirty-five times the heat commonly considered as equivalent to the force used; or the corresponding amount of cold. At that time, then, two years after his paper read to the British Association (to which you refer me), he certainly did not hold such an opinion with regard to the mechanical equivalent of heat as to exclude the possibility of such an engine.

The final judgment of the question I confidently leave to time and facts. When any of the "grand founders of a rapidly progressive science" can spare time from their investigations to refute my fallacies, I shall gladly retract them. H. HIGHTON

The Spectrum of the Aurora

IN the sketch appended to my letter on this subject in last week's NATURE, I notice that the engraver has made the line at 4·1 much too sharp and definite in both spectra. It really shades off rapidly to the more refrangible side, at least in the spectrum of the vacuum tube, and possibly also in that of the aurora. Though much the brightest line in the auroral spectrum, it is not the most conspicuous in that of the tube, but the relative brightness of lines frequently varies much at different temperatures. The band at 8 in the auroral spectrum is also represented too narrow. Those who have practical experience with the spectroscope will appreciate the great difficulty of representing faint spectra correctly in a woodcut.

HENRY R. PROCTER

Science Teaching for the People

THE subject of Science Teaching in our elementary schools having been ably brought forward by Mr. Henry Ullyett in a recent number of your Journal, and the scientific instruction under the Science and Art Department, South Kensington, having been at various times the subject of consideration in it, I venture to ask for a short space in your columns in order to submit the following proposition for the consideration of your readers, many of whom, have, probably, special opportunities of coming to a correct judgment on the point. The question I wish solved is this: Is the spread of scientific education, under the auspices of the Science and Art Department, likely to be best promoted by the whole of the Department's assistance to any one town being dispensed by a single committee, by whom a central school shall be provided, of which all other schools established, or that may be established, in various districts of the town, shall be considered only as branches, and be subject to the control of the central committee, on whose books the names of all students would be borne, and through the one secretary of which all the returns and other communications to and from the Department would have to pass?

Is it not better that the schools established in various districts of a town, say in connection with each elementary school, should each have their separate organisation of committee and secretary, at least in so far as the teachers connected with each are different? In this town, in common with a great many others, the latter plan has been the rule, but a suggestion has now been made that all these committees should be amalgamated, together with those of the art-classes also, the plea being that it might lead to the erection of a central building for the purposes of an art and science school.

Now, however necessary for art it is that there should be a central building for the provision of higher instruction than can be given in the night classes, I cannot see that there is anything in the study of science that demands greater facilities than can easily be provided by any district school, and the possession of which is indeed required by the Science Directory before any science school receives the approval of the Department.

Why, then, the science committees should be asked to unite in this town alone, by which a most dangerous precedent would be established, I cannot understand, for I do not believe that such a course would be beneficial to the town at large, while it would be very prejudicial to the interests of the existing district schools, and of any persons wishing to commence teaching in future.

Perhaps some of your readers will favour us with their views on the question I have stated, which I venture to think involves a principle of very general interest to the science schools of the kingdom. A MEMBER OF A SCIENCE COMMITTEE

A Rare Moth

IT may be interesting to know that the rare and beautiful moth, *Deilephila Galii*, appeared somewhat plentifully in the neighbourhood of Derby during the past summer. I have in my possession a fine male and female which were captured at Long Eaton, some few miles from Nottingham, whilst flying in company over a bed of geraniums. Is not this the first recorded instance of their appearance in this locality? *Zeuzera Æsculi* also appeared in unusual abundance in the same garden, upwards of fifty specimens being taken by one person. W. H. G.

Measurement of Mass

WILL you allow me a few words in explanation of a sentence in my last letter, which has strangely been misunderstood by Prof. Everett. In defending the system which makes the standard pound a unit of force on the ground that although not the most philosophical, it simplifies the conception of mass which is always difficult for beginners, I said, "The assumption of a hypothetical force of gravity not dependent on latitude, seems to stand on the same footing as the employment of a mean solar day," meaning, of course, that just as we assume (for convenience) that a solar day is the same length at whatever period of the year we take it, so we may assume a mean force of gravity (the actual force of gravity in latitude 45°) which is the same all over the earth. Such an assumption will enable us to explain the unfamiliar notion of mass by the familiar one of weight, and when it does become necessary to take into account the variation in the force of gravity at different points of the earth's surface, the correction is easily made.

Prof. Everett seems to think that I suppose that the average length of the apparent solar day is not the same at all places on the earth. Will you allow me to quote the following passage from the original in support of my first assertion, that Prof. Everett's tacit assumption that everybody knows what mass is, is less likely to lead to clear ideas than the explanation given by Deschanel. He says—"Un corps a une masse plus grande qu'un autre lorsque la même force lui imprime une vitesse plus petite, et reciproquement, ... si nous considérons en particulier le poids d'un corps, on aura, entre ce poids, la masse et l'accélération de la pesanteur, la relation fondamentale,

$$P = Mg$$

Cette formule nous montre que dans le même lieu le poids est proportionnel à la masse, parceque g a la même valeur pour tous les corps. Il n'en est pas de même quand on passe d'un lieu à un autre; mais comme après tout les variations sont extrêmement petites, en réalité la masse et le poids sont deux quantités toujours sensiblement dans le même rapport. Toutefois il faut se rappeler que ces deux expressions correspondent à des notions distinctes, et, abstraction faite de toute évaluation numérique, la masse d'un corps est quelque chose qui lui est propre et qui est indépendant du poids. La pesanteur n'existerait pas qu'il n'en serait pas moins vrai qu'une sphère de plomb a une masse plus grande qu'une sphère de liége de même diamètre. Nous reconnaissons ce fait ordinairement à ce que la poids de la première sphère est plus grande que celui de la seconde; mais à défaut de la pesanteur, l'emploi de toute autre force pourrait nous conduire au même resultat." W. M. W.

PHOTOGRAPHS OF THE ECLIPSE

PERMIT me to call your attention to the position of the woodcut illustrating my remarks on the Eclipse Photographs. The south point is where the north should be. As what I have now to say refers to the picture I shall feel obliged if you will permit its reinsertion in its true position.* With reference to the power of the light of the Corona, I used the word *actinic*, not *active* as printed.

The readers of NATURE may perhaps be glad of the opportunity to compare for themselves tracings of the American and of my own photographs, which I now give in outline in illustration of remarks in your second article on the Eclipse Expedition. In Mr. Lockyer's article it is stated:—"Now at Syracuse Mr. Brothers also photographed rifts, three rifts, but the sketches did not record a single one;" forgetting, evidently, that at Syracuse no attempt was made to sketch the Corona either by our own party or the Americans. At Agosta Mr. Brett was stationed, but as the Eclipse was only visible there for about *five seconds*, of course in that time no artist could pretend to make a drawing. It happens, however, that Prof. Watson was at Carlentini, and being favoured with a clear sky he succeeded in making a very careful drawing, which I had the good fortune to see and compare with my photograph No. 5 a few days after the Eclipse. An outline of this drawing I now give, so that it may be compared with the photographs made in Spain and at Syracuse.

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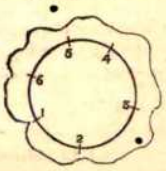
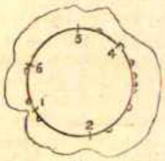
* This vexing mistake was due to a blunder of the printer in reversing the block after it had been placed on the machine. Its re-insertion this week will rectify the mistake.—ED.



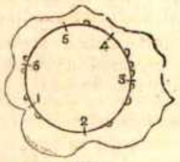
I

From Prof. Watson's Drawing.



II

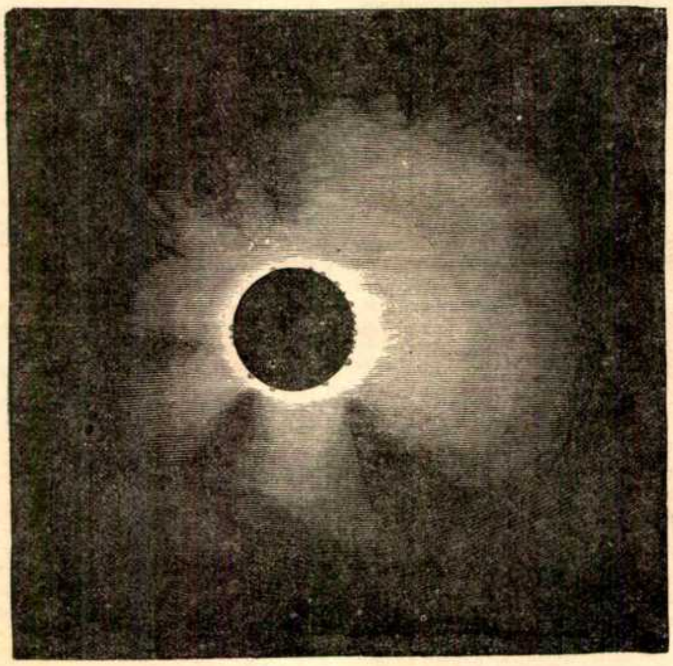
From American Photo. taken at Cadiz.



III

From Photograph taken at Syracuse.

There are two or three points which must be considered in comparing drawings and photographs. The photographs will differ according as they are made with a camera or telescope, and the drawings will differ according as they are made with the aid of a telescope or without. With the telescope the field of view is limited, and the eye is naturally attracted chiefly by the intense light of the red prominences and the corona near the moon's limit. Naked-eye drawings ought to be as valuable as photographs, but I doubt if any two artists will ever be found to make sketches agreeing in every particular. On photography must we depend for settling doubtful points of this nature, and it seems to me in this case to be absolutely settled that three rifts are identical. The outline sketches speak for themselves. A pair of compasses applied to the points formed by lines drawn from the moon's centre to the centres of the depressions (or rifts) in the corona, will show whether or not the places of the three gaps are the same.



THE LATE ECLIPSE, AS PHOTOGRAPHED AT SYRACUSE

It may be said that Lord Lindsay's photographs taken five miles from the station occupied by the American observers in Spain, do not show the rifts. This, I think, must be accounted for by the presence of cloud. The cloud may have been so thin as to be quite invisible in the feeble light of the Eclipse, but yet sufficient to prevent the photographic delineation of the rifts. Three of my photographs were taken through cloud, and they show us traces of rifts. The fifth plate shows three distinctly, and less plainly five or six others.

Professor Watson's drawing shows two gaps corresponding with 1 and 6 in both photographs, and depression in the corona agreeing very closely indeed with my picture.

This evidence seems to me to be absolutely irresistible as to the identity of the great rifts in the corona.

In explanation of the way the outline drawings have been made, I may say that the points marked from 1 to 6 have been pricked through the photographs, Professor Watson's drawing having been reduced to the same scale as the photographs, and pricked off in the same manner.

A. BROTHERS

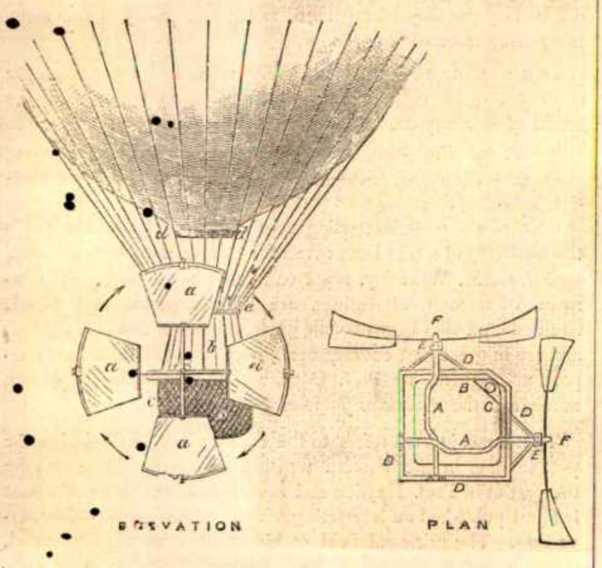
EXPEDITION OF THE "DUQUESNE"

M. RICHARD, master in the Royal Navy, directed the Expedition, and is now attached to the Lille aëronautic station for the Department of the North. I have interrogated him and elicited from him the following details, which can without inconvenience be placed before the eyes of the general public. The French Republican Government having in view the promotion of general knowledge, as well as the defence of the national integrity, did not object to any communication which is not directly connected with warfare.

The aërostat, "Le Duquesne," was despatched from Paris on January 9, at three o'clock in the morning, before a large attendance, among them some members of the French Institute. The reigning current was a strong S.W. wind, which was unfavourable for escaping the Prussian lines, as the intended directing power was only a motion of three feet per second. The experiment should have been postponed for a fair trial. Another drawback to the Expedition was the despatch of the balloon in the night-time, although the moon, being almost full at the time, afforded some light to the aërial travellers, being very low on the horizon when "Le Duquesne" left the Orleans railway station. There were in the car M. Richard and three sailors of the national navy, so that two could be kept pulling without interruption; three sacks of despatches, four pigeons, eight sacks of ballast, thirty kilogrammes each, were also in the car. The provisions were bread, wine, and chocolate. The weight of the machinery was 300 pounds. As will be seen by the accompanying diagram, the two screws were worked by a very simple contrivance, rotating only at a rate of twenty-five rotations per minute, but the diameter four yards, so that the motion in feet per second of the screws was about sixteen, five or six times more than the rapidity intended to be given to the balloon, which was three miles an hour.

Before leaving ground the car had been placed with its diagonal due north. But that precaution proved useless, as the balloon rotated when ascending before the screws could be put into operation.

We will give the explanation under M. Richard's own authority, with some remarks. We are certain of his perfect truthfulness, but it is very hard to say if he saw everything correctly for the whole length of the eventful journey.



THE DUQUESNE DIRECTING BALLOON, DESPATCHED FROM PARIS JANUARY 9, 1871

a a a a segments of the screw revolving in the direction of the arrow or the opposite direction, with variable rapidity; *b* valve line; *c* car; *d* appendage *e* hoop.

A A handle for moving screw, two men pulling at once; C place of the captain; B place of one man; D D board in iron tubes fixed to the car; E E extremities of the axes; F F fixing screw, which can be removed so that wheels may be thrown overboard when landing.

The observers on the ground had placed themselves in a right position to ascertain the effect of the screws, and the *compte-rendu* published in Paris gave the summary of their impressions. They suppose M. Richard succeeded in giving to his balloon a deviation of 15° from the due course of the wind during the earlier part of his journey. If experiments had been made during daylight, matters should have been more easy to ascertain. If I can procure authorisation, I will get an experiment tried anew at the Crystal Palace before an English audience. The fact is that two of the three sailors pulled with all their strength during a few minutes, after having exhibited some hesitation in the first instance. The scenery was so magnificent that it was necessary to call them twice before they began to pull.

M. Richard soon perceived that he was unable to ascertain the effect of the propeller. He resolved upon letting the balloon follow its way undisturbed, and he noted carefully the barometric altitude and the direction. When passing over Prussian lines at an altitude of $0^{mm}\cdot69$, some shots were directed at the balloon without any result.

At $3^h\ 10^m$ altitude 0·68° was reached; temperature 20° F. The Valley of the Marne was below.

M. Richard turned the screws and tried to pull towards the north, in order to go N.N.E; but the rotation was difficult to stop. It was necessary to work only one screw during a long time in order to rotate the aërostat in the right direction. It was only when some real torsion was established between aërostat and car that the required revolution took place. But when movement was given to the balloon it was difficult to prevent it, and the aërostat executed one entire revolution against the will of its captain. Then the two screws were worked together; the same effects were produced, but it was only with great difficulty that the car was placed in the right direction. When the two screws were worked together, the balloon was rotating sometimes in one way and sometimes in another. These observations, moreover, says M. Richard, were made at night by a man who had never before ascended in a balloon. As already said they cannot be considered as wholly reliable, as rotation depends on the changing of the fuel as well as on the friction in the forward half or on inequality in the pulling of the screws. But it is very easy to understand that the fact of the screws being able to rotate the balloon in a given direction is unquestionable, although men and captain were equally unable to move it in the right direction for escaping Prussian lines.

The difficulties experienced were so great that M. Richard stopped the experiment, and tried it again only after day-break, but with no other success than previously.

At 7 o'clock in the morning, altitude 65 centimetres, temperature 11° Fahr., cirrus was visible at a great height, cumulus down below, and stratus covering the earth.

At 11 o'clock only one sack of ballast was left. The balloon stopped its descent; voices of peasantry were heard from above the stratus. They cry "Versy; commune of Chigny." The balloon emerging in stratus, fell rapidly by condensation. All the ballast was thrown away, and the balloon ran horizontally for two miles. The guide rope only had been thrown; then the grapnel was thrown out. The wind was so strong that the grapnel rope was broken. The screws and wheels had been thrown overboard, but the car incliners and the screw axes project outside. One of these comes into contact with the ground, the car is upset, and the crew are dragged under it for a length of time, 600 yards. The balloon is stopped by the loss of gas and the peasantry. The three sailors are slightly injured, the captain is left for dead. The sailors, helped by the peasantry, make their escape, and conceal balloon, car, and despatches in the woods. The captain is brought like a corpse into a neighbouring town, but is found to be alive. He is cured, and sent secretly into Lille, where the balloon arrived two days later.

W. DE FONVIELLE

NOTES

AT a recent meeting of the American Philosophical Society it was resolved "That a committee, consisting of the President of the Society and five members, be appointed, whose duties it shall be to consider and report whether it is desirable, and if desirable, whether it be practicable, to establish in the City of Philadelphia, under the auspices of the Society, an Observatory, astronomical and physical, either or both; and if so at what cost, on what site, and what instruments are requisite for such purposes, and at what cost such instruments can be procured." We suppose such a proposal for England would be looked upon as a joke, and yet already we cannot compare with America in our observing power.

WE are informed that the Royal Commission on Scientific Instruction and the Advancement of Science have their First Report nearly ready.

DR. J. CLERK MAXWELL, F.R.S., was elected yesterday to the Professorship of Experimental Physics in the University of Cambridge.

It will have been observed that Sir F. Goldsmid carried his motion in the House of Commons last week: "That, in the opinion of this House, young men qualified by character and attainments for admission into the service of the Government of India as civil engineers, ought not to be excluded from such service by reason of their not having been educated at a Government College." If the facilities afforded by existing institutions are not found sufficient for the training of practical engineers, it is quite right that the Government should step in and supply the deficiency; our recent article on the subject shows whether the Institute of Civil Engineers is alive to the wants of the time in this respect. And if the Government establishes such a College, it is quite right that it should examine all candidates who present themselves, and give diplomas to those who pass them with credit; otherwise, it shows but little faith in its own system of education. Mr. Lowe has well pointed out that the Government will place itself altogether in a false position if it abandons its intention with reference to the proposed College, and yet takes no notice of the want of adequate instruction in the existing institutions. A leading article in the *Engineer* of last week on the subject points out that the idea of a competition between the Royal Engineers and the proposed College is a bugbear.

The number of candidates for admission to the Royal Society this year is fifty.

The members of the French Institute have learned with deep regret the death of one of its most celebrated members. M. Lartet died in the department of the Gers during the investment of Paris. It is in that very department that he discovered an immense quantity of fossils at Saint Salut. M. Lartet was professor of Palæontology at the Museum of Natural History, filling the place of the lamented d'Archiac. He was ill from the time of his nomination, and was unable to deliver a single lecture except his inaugural address. M. Lartet is known to the whole scientific world by an immense quantity of academic and scientific memoirs. His only work of consequence was published in connection with Mr. Henry Christy, an English merchant well known for his love of geology. That publication, which marks a starting point in palæontological inquiries, is called *Reliquiæ Aquitanicæ*, and cost the late Mr. Christy an immense sum of money.

We are very glad to observe that a fund is being raised in this country for the relief of French horticulturists and gardeners who have suffered by the recent war. Many of them have been reduced to a deplorable plight, their gardens, greenhouses, and orchards having been completely destroyed. Their condition presents a strong claim on their more fortunate *confrères* in this country. Contributions may be made in money, plants, cuttings, grafts, stocks, seeds, tools, mats, &c., and it is urgently requested that subscriptions in money be forwarded at once to the treasurer, Mr. G. F. Wilson, F.R.S., Heatherbank, Weybridge Heath, or to the honorary secretary, Rev. H. H. Dombrain, Westwell Vicarage, Ashford, Kent.

The impulse given to the study of Natural Science at Rugby is already bearing fruit. Mr. R. J. Williamson, who was elected to a Natural Science Studentship at Christ Church, Oxford, on the 4th, and Mr. C. J. Taylor, who obtained Miss Burdett Coutts's Geological Scholarship on the third of this month, were both Rugbeians. Including these, Rugby has had six Natural Science Honours during the last twelve months, viz., two Oxford "Firsts," one Cambridge "First," two Christ Church Studentships, and the Geological Scholarship. All this is encouraging for the future.

The *Bookseller* records the death at Dartford, at the age of eighty-two, of Mr. Augustus Applegarth, the eminent mechanist and inventor, to whom the world at large and the printing trade in particular are largely indebted for his improvements in the printing-press. Until substituted by Hoe's American machine, the *Times* was printed from an Applegarth, which printed from 8,000 to 10,000 copies per hour. Mr. Applegarth took out no fewer than eighteen patents for various purposes, including three for the printing of bank-notes, and for printing silk; but, like many inventors, although he enriched others, he failed to secure a fortune for himself.

M. Sorrel, one of the youngest members of the Observatory, has been a victim of the privations endured during the investment of Paris. He was engaged in the artillery of the National Guard. He had made one ascent on board the "Géant" and another on board the "Pole nord."

The cattle plague is raging amongst the herds which were congregated for the revictualling of Paris, and the carcases of dead animals are every day brought out in open carts, and conducted to Monfauçon to be buried. It appears that the same plague followed the invasion of 1814. M. Bouley, a member of the French Institute, contended in its last sitting that the meat of those animals could be eaten with impunity, and he said in support of his argument that cases of plague were discovered during the investment of Paris, and that the animals were not thrown away as useless. But it must not be forgotten that the mortality during the siege was very high, and that a part of the deaths may possibly be attributed to the bad quality of the meat taken from infected animals.

The weather at Paris is now very mild, and some fears are expressed of an outbreak of epidemic, in consequence of the burial of so many dead bodies of men and horses round Paris. It is supposed that 200,000 men and more than 100,000 horses were killed, besides 60,000 persons who died from different affections.

One of the most interesting scientific novelties of the day is the discovery of a true bone cave near Phœnixville, Pennsylvania, by Mr. C. M. Wheatley, reported in *Harper's Weekly*. The remains of animals, all extinct, are quite abundant and varied, and include bones of mastodon, horse, mylodon, and other forms, and are in great part entirely new to science. A preliminary report upon them, by Prof. Cope, was to appear in the March number of the *American Journal of Science*.

The same journal refers to the explorations made during the past summer in the western territories of the United States by the parties of Prof. Hayden and Prof. Marsh, the latter of whom has just published an interesting *résumé* of his geological observations. The principal field of his labours, as already stated, was the neighbourhood of Fort Bridger, among certain fresh-water deposits from an ancient lake, the strata of which formed a thickness in places of at least 1,500 feet. Vertebrate remains of great variety were found entombed in these deposits, differing in marked features from those belonging to the Miocene basin east of the Rocky Mountains. In the latter bones of ruminating animals were especially abundant, while fishes and reptiles, with the exception of a single species of tortoise, were entirely wanting. In the Fort Bridger basin, on the other hand, reptilian life was in great development, and was represented by crocodiles, tortoises, lizards, and serpents, together with numerous fish, while many mammals, allied to the tapirs, as well as other small quadrupeds, occupied its borders. A detailed report of this exploration will be found in the March number of the *American Journal of Science*.

Lieutenant Payer, well known for his geological investigations in the Alps, has lately communicated some facts in regard to discoveries in Greenland by the late German expedition, of which he was a member; and in this he calls attention especially to the probability of the hypothesis that Greenland is essentially a congeries of islands similar to that west of it, and not a huge continental mass, as has been supposed by most authors. One strong evidence of this he considers to be furnished by the deep inlet discovered by the expedition, previously unrecorded on any chart, and which received the name of Emperor Francis Joseph's Fiord. This was found to extend deep into the interior of the land, continually opening into new arms, and widening in places until it was traced out for over one-third of the estimated breadth of Greenland, and without any indication of coming to an end. Indeed, in a south-westerly direction it opened out into what looked like a great basin into which the fiord itself emptied. The circumstance also that the saltness of the fiords is generally greatly diminished by the fresh-water streams pouring into them when they are simply *cul-de-sacs*, and the fact that the great Greenland fiord, notwithstanding the enormous addition of fresh water, retained all its saltness, pointed to a maritime communication with the opposite side of the country. Time was not allowed to the party to prosecute the exploration of this supposed strait; but it is believed, as stated, that it finds its opposite opening in Baffin's Bay. Another still more potent argument in favour of the assumption that Greenland is a congeries of islands, and not a continent, was found in the apparent absence of great longitudinal valleys, such as usually characterise continents, these being entirely wanting in the north-eastern part of Greenland.

WE understand that the collection of books belonging to the late A. H. Haliday, A.M., has been left by his will, dated in 1847, to the Royal Irish Academy. This collection is very rich in rare entomological tracts, and it is expected that a catalogue of it will be formed under the directions of the librarian of the Academy. Any duplicates of works at present in the library of the Academy are to go to the library of the Natural History Society of Belfast. The entomological collection of Mr. Haliday was bequeathed to the Museum of Trinity College, Dublin. This collection chiefly consists of British Diptera and Hymenoptera, and contains the type specimens of the many classical monographs and papers of Mr. Haliday. It also contains the results of some years' collecting in Italy, and especially the insects, many of them of great interest, taken during a tour in Sicily in 1868 by Mr. Haliday and Professor E. P. Wright. Unfortunately the collection is still in store or collecting boxes, and is for the most part unnamed, the localities of the European insects, which, however, form far the smallest portion of the collection, being indicated only by dates. We hope soon to be able to report what steps will be taken to have this collection named and arranged, knowing well that the authorities at Trinity College, Dublin, will not be slothful in this matter.

A COMMITTEE has been raised for the purpose of collecting subscriptions to present Mr. John Banting Rogers with a testimonial, as a mark of the high appreciation in which the shipping community of this country hold his untiring labours in the introduction of his valuable invention for life-saving purpose.

A CORRESPONDENT calls our attention to a state of things in the village of Woking which appears to require the notice of the Government inspector. He states that the drainage from the convict prison at Woking and from the gas-works is allowed to flow into an open meadow, which has become perfectly saturated with it. Fever has been prevalent in the neighbourhood for the last two years, and small-pox is now added. At the neighbouring village of Horsell a school of 200 children has been closed in consequence. As usual, it seems impossible to fix on anyone the responsibility for abating the nuisance.

CAPTAIN C. F. HALL is busily engaged in fitting out his vessel for his cruise next summer, and will be amply provided with everything necessary for his comfort and the success of his enterprise. It is understood that Dr. David Walker, the well-known companion of Sir Leopold M'Clintock during his expedition in the *Fox* in search of Sir John Franklin, will accompany Captain Hall in charge of the scientific department.

DR. A. W. EICHLER, the Editor of Martius's "Flora Brasiliensis," has been appointed Professor of Botany and Director of the Botanic Garden at Graz in Austria. No change will be necessary in the editing of the magnificent work under Prof. Eichler's charge.

THE region bordering upon the Black Sea has long been known to be full of antiquarian treasures of the highest interest, as evinced by the superb reports published from time to time at the expense of the Russian Government. A late exploration of the peninsula of Toman, situated between the Black Sea and the Sea of Azov, in continuation of previous researches, has brought to light many striking objects, particularly of those belonging to a past period of Greek art, and consisting of gold ornaments, sarcophagi, terra cotta statuettes, &c.

IN the Intercolonial Exhibition, held last year in Sydney, New South Wales, much interest and curiosity were felt in the specimens of wine exhibited. The report of the jury on the Victorian produce was highly satisfactory, more especially with regard to those wines retailed in Melbourne at 2*d.* and 4*d.* per tumbler. These wines are said to compare more than favourably with the cheap wines in general consumption in the wine countries of Europe, and promise ere long to supplant the use of beer and spirits in the classes most accustomed to use and abuse those beverages. For experience has long proved that men will drink wine in hot and dry climates in preference to all other beverages if they can get it abundantly, cheap, and good in its kind. The jury say that the cheap and wholesome wines sold in Melbourne at the above prices all over the city, and even a better class of wine retailed at 1*s.* per quart bottle, are the outward evidences of the hold which pure wine is taking on the masses in Victoria.

IT would appear from a communication in *Le Journal de Quebec* that the sea is steadily swallowing up the land at St. Thomas, and possibly other points on the lower St. Lawrence, a correspondent of the *Engineer* asserting that at low tide, some thirty years ago, he saw the ruins of the third church built in that locality within the space of two hundred years, and that since the battues, or banks covered by the tide, have advanced from one to two leagues into the interior.

MEAT-PRESERVING appears to be getting a profitable branch of trade in all countries where stock abounds or pasture exists. A new company with a capital of 50,000*l.*, under the title of the "Victoria Two-fold Bay, and London Meat-preserving Company" has been started in Melbourne. The meat is to be preserved by a process of cooking *in vacuo* by which air and moisture is drawn from the inside of the tins during the cooking.

THE Monthly Notices of Papers and Proceedings of the Royal Society of Tasmania for March, April, and June, 1870, contain several valuable articles, including Contributions to the Phytography of Tasmania, by Dr. Ferd. von Müller; and some additional Observations on more recent changes which have taken place in the star η Argus and its surrounding nebula (with diagrams), by Mr. F. Abbott.

THE great booksellers like Hachette and Co. have been surprised to learn that their branch offices in occupied districts had transacted an immense amount of business. The reason is singular enough, and worthy to be noticed. The Prussian soldiers are learning the French language, and are purchasing an immense number of books. There is a new market opened for French authors, an unforeseen consequence of bloody defeats in the field.

AT one of the sittings of the French Academy, Dr. Felix Rochard proposed to establish on the Seine floating ambulances for the wounded as being probably more free than common hospitals from miasmatic influence. The proposition was warmly supported by Baron Larey, the great practising surgeon.

PAPERS ON IRON AND STEEL

II.

THE BESSEMER PROCESS (CONTINUED)

IN the first part of this paper* I described the facts of the Bessemer process, and now proceed to a theoretical examination of these. In order to do this at all satisfactorily, it is necessary to have, at the outset, a clear idea of the composition of the raw materials,—the pig-iron and the spiegeleisen. I insist the more urgently upon this, because the descriptions or definitions of cast-iron or pig-iron usually given in our chemical text-books are by no means satisfactory, and are frequently erroneous.

The following are the results of my own analyses of fourteen brands of pig-iron and five brands of spiegeleisen, all of which are rather extensively used in the manufacture of Bessemer steel. In addition to the substances there determined, most of the pigs contain a small quantity of calcium, but this and the small traces of the metals of the other alkaline earths, and of the alkalis, were not determined, as the analyses were made for commercial purposes, and I have not been able to detect any practical modifications in quality of the finished iron or steel, which is due to the presence of these metals in the pig-iron. For this reason the statement of "iron by difference" is but an approximation, and somewhat in excess.

Composition of Bessemer Pig-Irons

No.	Combined Carbon.	Graphite.	Silicon.	Phosphorus.	Sulphur.	Manganese.	Iron by Difference.
1	0·60	4·12	1·92	0·10	0·06	0·32	92·88
2	trace	3·52	3·10	0·07	0·06	0·30	92·95
3	0·70	2·68	3·60	0·09	0·12	1·16	91·65
4	0·55	1·85	3·60	0·13	0·18	1·22	92·47
5	0·55	1·92	3·65	0·09	0·13	none	93·66
6	0·75	2·00	3·36	0·10	0·23	trace	93·56
7	0·54	2·20	3·15	0·10	0·26	2·00	91·75
8	0·72	2·10	2·90	0·06	0·16	1·00	93·06
9	0·62	4·00	1·40	0·02	0·06	0·38	93·52
10	0·50	3·00	2·00	0·03	0·10	trace	94·37
11	0·20	2·80	2·33	0·02	0·10	1·92	92·63
12	trace	3·20	1·58	0·11	0·12	2·22	92·77
13	0·37	1·96	4·08	0·15	0·23	1·15	92·06
14	1·25	1·65	2·15	0·24	0·21	1·10	93·40

For commercial reasons, which will be readily understood, I abstain from publishing the names of the above brands. No. 12 is a Swedish pig-iron; all the rest are English hæmatite pig-irons, made expressly for Bessemer purposes. Nos. 9, 10, and 11 produced exceptionally good steel; Nos. 1, 2, and 3, good average qualities; No. 4, inferior; No. 13, *very* inferior; No. 14 produced such bad steel that the whole parcel was returned, though it came from a well-known firm, it was of the same brand as No. 4.

Composition of Spiegeleisens

No.	Combined Carbon.	Graphite.	Silicon.	Phosphorus.	Sulphur.	Manganese.	Iron by Difference.
1	4·10	0·45	1·23	0·12	0·16	4·60	89·34
2	4·10	0·40	0·96	0·03	0·26	5·86	88·39
3	4·50	0·40	0·88	0·04	0·10	9·61	84·47
4	4·10	0·42	0·65	0·05	0·15	8·64	85·99
5	3·00	0·70	0·14	0·04	0·07	6·44	89·61

No. 3 is the best of these; No. 4, the next in quality, rather better than average; No. 2, rather below average; No. 1, inferior quality; and No. 5 so poor that it was rejected.

Excluding the rejected samples, the average of the above is as follows:—

Average Composition of 13 Brands of Bessemer Pigs

Combined Carbon	0·47
Graphite	2·72
Silicon	2·84
Phosphorus	0·08
Sulphur	0·14
Manganese	0·90
Iron by difference, about	92·85
	100·00

Average Composition of four Brands of Spiegeleisen

Combined Carbon	4·20
Graphite	0·42
Silicon	0·93
Phosphorus	0·06
Sulphur	0·17
Manganese	6·63
Iron by difference, about	87·59
	100·00

The sulphur of this average of the spiegeleisens is excessive, being raised unduly by the very unusual quantity contained in No. 2: 0·12 per cent. would state the general average more correctly. In like manner the phosphorus average is raised by the excessive quantity in No. 1. Excluding this the average is reduced to 0·04.

I will pass over the small amount of chemical change which results from the mere melting of the pig and speigeleisen in the cupola, and regard the above as the composition of the material which enters the converter. When a mixture such as these Bessemer pig-irons is fused and exposed to the action of atmospheric air, the silicon is the most readily oxidised, silicates of iron and manganese are formed, which separate and float on the surface, forming the "cinder." The carbon oxidises simultaneously with the silicon, but in a much smaller degree, until the silicon is nearly all burnt out. When the silicon is reduced below one per cent., the combustion of the carbon takes the lead, and the small remainder of silicon is but slowly oxidised, the last traces resisting oxidisation with considerable stubbornness.

I have made some special investigations of this subject, and shall show in the course of another paper that manganese is remarkably efficient in removing these last traces of silicon. I should also mention that the above-stated generalisations respecting the prior combustion of silicon and the suppression of carbon combustion by the presence of unburnt silicon, are based chiefly on examinations I have made of the actions which take place in the "refinery;" the difference between the Bessemer converter and the refinery being, that in the one, air is blown upon or a little below the surface of the melted pig-iron, while in the other, it is blown through it from below, and thereby acts with far greater efficiency and rapidity; the *kind* of action is, however, the same in both cases, the difference is only in *degree*.

In order to test the accuracy of the above conclusions, I have requested my late assistant at the Atlas Works, Mr. G. C. Barker, to make analyses of the Bessemer material during different stages of the same blow. This he has kindly done, and the following are his results. The carbon, silicon, sulphur, phosphorus, and manganese only are determined.

The first column shows the percentage of these constituents in the pig-iron after being melted just before being poured into the converter.

Second, the same after six minutes blowing.

Third, the same after twelve minutes blowing.

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* NATURE, Vol. iii. No. 63, p. 211

Fourth, the same when the blowing was finished, but before the spiegeleisen was added.

Fifth, the finished steel when poured into the ingot moulds.

	1	2	3	4	5
Combined carbon	1·000	3·040	1·640	0·190	0·370
Graphite . . .	2·570	trace	trace	trace	trace
Silicon	2·260	0·955	0·470	trace	trace
Sulphur	0·107	0·091	0·098	0·095	0·090
Phosphorus . .	0·073	0·070	0·070	0·070	0·059
Manganese . .	0·410	trace	trace	trace	0·540

We shall now be able to understand the changes I have described as occurring in the flame. Before the full combustion of the carbon can commence, there is about 2½ per cent. of silicon to be converted into silicic acid. In a charge of 6 tons this amounts to 3 cwt. For the complete combustion of this, nearly 3½ cwt. of oxygen, or about 14 cwt. of atmospheric air, is necessary. My explanation of the smaller and less brilliant flame that at first roars from the mouth of the converter is that it is mainly a silicon flame, mingled, however, with a small proportion of carbon flame; that the amount of silicon combustion goes on diminishing, and in a proportionate degree the carbon combustion increases, as the demand of the silicon upon the oxygen of the blast diminishes in consequence of its less abundant diffusion among the melted iron.

I shall not be surprised if this explanation is controverted, as in offering it I fly in the face of the spectroscope, which has made such glorious conquests that modern philosophers are disposed to trust it as implicitly as successful soldiers rely upon the general who has led them continually to victory; but without failing in due deference to those who are more skilful than I am in the use of this instrument, I am satisfied that in this and other cases where the question has been to determine the presence or absence of the *metalloids*, the *negative* replies of the spectroscope have been too hastily accepted. Prof. Roscoe, who devoted a considerable amount of time and labour to the spectroscopic examination of the Bessemer flame, says, "Those who are practically engaged in working this process would like spectrum analysis to do a great deal more: they would like to be told whether there is any sulphur, phosphorus, or silicon in their steel; questions which, unfortunately, at present spectrum analysis cannot answer, for this very good reason, that these substances do not appear at all as gases in the flame, but that they either remain unvolatilised in the molten metal, or swim on its surface in the slag of the ore; and, consequently, the lines of these bodies are not seen in the spectrum of the flame." Dr. Watts's observations and conclusions accord with those of Prof. Roscoe.

If by the above Prof. Roscoe is to be understood as asserting that no portion of the Bessemer flame at any period of its existence is due to the combustion of silicon, or that silicon is not present in the Bessemer flame, I must very decidedly affirm that such conclusion is erroneous. I do not for a moment question the accuracy of the observations of both Prof. Roscoe and Dr. Watts. I merely maintain that the absence of "the lines" of silicon in the spectrum of the flame does not prove its absence as a constituent in producing such flame, and for the following reasons:—

We know that that the silicon existed in the pig-iron in the proportion already stated, and that although a very small quantity of that which ordinary analysis detects may have existed as entangled silicate in the pig, and another small portion is of course oxidised in the cupola, the bulk of it enters the converter as unoxidised silicon, and that it is oxidised and converted into silicic acid during the blow. We also know that silicon when heated in air or oxygen burns brilliantly, and that the product of such combustion when heated with a blast such as that which supplies its oxygen in the Bessemer converter, is sufficiently volatile to form concretions in the throats of furnaces, which have been compared to natural chalcedony. Besides this a large quantity of solid matter is mechanically forced into the flame, and is seen above as a red smoke, which, without the slightest indication of unburnt carbon, is often sufficiently dense to hide the mid-day sun. The greater the quantity of silicon in the pig the more dense is this red smoke, which appears to consist of silicate and peroxide of iron. I maintain, therefore, that silicon is there, and that it must contribute to the luminosity of the flame, though it shows no characteristic "lines" in the spectrum.

Under such circumstances, we have no good *a priori* reasons for looking for the silicon *lines*; a *continuous spectrum* being that which we are theoretically justified in anticipating as the result of such combustion of silicon, and this is exactly what the spectroscope reveals. The spectrum of the Bessemer flame at the commencement and early stages of the blow is of a most uncommunicative continuous character: occasional flickerings and vanishing ghosts of lines and bands come and go with perplexing irregularity; and even the brilliant and ever obtrusive sodium lines do not appear at this stage, but commence with spasmodic flashings across the spectrum at about the period when the elongation and brightening of the flames which I have described is most decidedly taking place. When the flame has reached its maximum of extension and brilliancy, the sodium lines cease their intermittent flashings, and become a steady stream of light, the lithium band appears (though not in every blow), and the whole spectrum becomes striped, but the continuous spectrum still remains as the permanent background.

My general reason for questioning the negative conclusions of the spectroscope in reference to silicon and the other metalloids is, that these bodies usually give a continuous spectrum when, as elements, they combine freely with oxygen, as in direct unrestrained combustion under ordinary pressure in the open air. It appears to me that there is thus presented a broad distinction between the spectra of the metals and of the non-metallic elements, which is of great practical importance, and which has not been sufficiently considered, when conclusions have been based on negative spectroscopic results. I have already, in chapter 13 of "The Fuel of the Sun," referred to the worthlessness of the negative evidence of the spectroscope in reference to the non-existence of the metalloids in the sun, and maintain that "they may all be there though the spectroscope should not detect one of them." The mere fact that nothing but metals (I include hydrogen with these) should have been discovered in the sun is very suggestive.

W. MATTIEU WILLIAMS

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 23.—"On the Thermo-electric action of Metals and Liquids," by George Gore, F.R.S. It is well known that the degree of rapidity with which a metal immersed in an acid, alkaline, or saline liquid is corroded varies considerably with the temperature, and that the speed of corrosion usually increases with the heat; also a few experiments have been published (Gmelin's "Handbook of Chemistry," vol. i. p. 375) showing that changes of electrical state occur in metals under such circumstances; but a further examination of the relations of the temperature and chemical change to the electrical state has not, that I am aware, yet been made.

In an investigation on the development of electric currents by unequally heated metals in liquids (*Phil. Mag.* 1857, vol. xiii. p. 1), I found that hot platinum was electro-negative to cold platinum in liquids of acid reaction, and positive to it in alkaline ones, provided in all cases chemical action was completely or sufficiently excluded. In the present experiments I have endeavoured to ascertain what electrical changes are produced in cases where chemical action more freely occurs, and I have there-form employed not platinum plate, but plates composed of a metal (copper) which is more easily corroded.

The following are the general results arrived at.

The chief fact brought out conspicuously by the experiments with copper dishes is, that in many cases an increase of chemical action produced by heat instead of making the hot metal electro-positive makes it considerably negative.

The results show that hot copper was positive to cold copper in the following liquids:—hydrochloric, hydrocyanic, boracic, and tribasic ortho-phosphoric acids; chloride of copper (weak solution); chloride of cobalt; chloride of manganese; chromic acid; chloride of chromium; sulphate of zinc (weak solution); sulphate of magnesia; chloride of calcium; nitrate and chloride of strontium; chloride of barium; nitrate of sodium (strong solution); chloride, iodide, carbonate, and biborate of sodium; sulphate of sodium (strong solution); tribasic phosphate of sodium; nitrate, chloride, and chlorate of potassium; bromide of potassium (strong solution); iodide of potassium (strong solution); carbonate, acid carbonate, and bichromate of potassium; aqueous ammonia; chloride of ammonium; cyanide and ferrocyanide of potassium; acetate of zinc; and acetate of sodium. And negative in the following ones:—nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids; ferrous sulphate; chloride of copper (strong solution); sulphate of copper; sulphate of zinc (strong solution); nitrate and iodide of sodium (weak solutions); bromide and iodide of potassium (weak solutions); iodate of potassium; chrome alum; nitrate of ammonium; oxalic, acetic, tartaric, and citric acids. The number of liquids in which hot copper was positive was thirty-six, and those in which it was negative was twenty.

In several instances where the hot metal was negative with a weak solution, it became positive with a strong one; for instance, with sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide, and iodide of potassium; but with chloride of copper the reverse occurred. These results may be connected with the fact that in weak neutral solutions the chemical action is generally the most feeble, and therefore interferes the least with the direct influence of the heat in producing electric currents.

The influence of free hydrochloric, hydrocyanic, boracic, ortho-phosphoric, and chromic acids, was to make the hot copper positive; whilst that of nitric, chloric, hydrobromic, hydrofluosilicic, sulphuric, and some of the organic acids was to make it negative.

In consequence probably of the small amount of interference by chemical action in solutions of oxalic, acetic, tartaric, and citric acids, the direct influence of the heat made the copper negative, similar to its influence on platinum, in all acid liquids which do not attack that metal.

The nature of the acid in a salt appears to exert much more influence than that of the base on the direction of the current; for instance, in nearly all chlorides, including those of a considerable variety of bases, hot copper was positive, probably because copper is more readily attacked by acids than by bases.

In all decidedly alkaline liquids the hot copper was positive; this is similar to the behaviour of platinum in such solutions, and is probably due to the same course, viz. the direct influence of the heat, as well as to chemical action.

The results also show that the quantity of the current obtained with any given liquid generally increases with the number of molecules of the substance contained in the solution; in some cases, however, as with sulphuric acid, carbonate of potossium, chloride of ammonium, and acetate of zinc, there was a limit to this increase; and beyond that limit the quantity of the current decreased up to the point of saturation of the liquid.

In the great majority of cases the value of the deflection increased much more rapidly than the strength of the solution, particularly with solutions of sulphate of magnesia, and also of hydrochloric acid and of chloride of sodium, probably because two causes operated, viz. increased strength of solution and diminished resistance; in a very few cases, however, the opposite result took place, as with solutions of chloride and nitrate of strontium.

Inversions of the direction of the deflection by difference of strength of the liquid occurred with solutions of chloride of copper, sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide, and iodide of potassium.

Irregularities of the amount of deflection were very apt to take place with liquids which gave strong deflections, or which acted much upon the copper plates (for instance, nitric acid), especially if bubbles of air remained under the plates, or the dishes were wetted on their side above the liquid by the solution.

In certain acid liquids, viz., nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids, the hot copper was strongly negative (notwithstanding the chemical action upon it was distinct, and in some cases even strong); this is similar to the electrical behaviour of platinum in such liquids, and may be attributed either to the more direct influence of the heat alone (such as occurs with platinum plates), or to a different influence of the chemical action produced by the heat. Both these causes probably operate in such cases.

It is probable that in all cases where the hot copper was positive in liquids of strongly acid reaction, the positive condition was due to chemical action alone.

With some liquids, especially with solutions of hydrocyanic, boracic, acetic, tartaric, and citric acids, the deflections were very feeble, and the chemical action on the plates not perceptible; whilst with others, such as nitric and chloric acid, solutions of the chlorides of strontium, sodium, potassium, and ammonium, and of carbonate, acid carbonate, and cyanide of potassium, the deflections were considerable, and the chemical action distinct, and in some cases strong. In none of the liquids (except hydrobromic and chromic acids) did the hot plates appear to be *less* stained or corroded than the cold one; probably, in all cases, it was the most corroded, although in some cases the corrosion was not perceptible.

The amount of deflection was not always proportionate to the amount of chemical action; for instance, with solutions of chloride of copper and iodate of potassium there was considerable corrosion, but only feeble currents, probably because the plates became covered with a badly conducting film, whilst with hydrochloric acid, chloride of cobalt, chloride of manganese, and nitrate of potassium, the reverse occurred.

I consider the currents in all these experiments of difference of temperature to be due either (1) to the direct influence of heat, the effect of which is to make the hot copper negative in acid liquids and positive in alkaline ones (see *Phil. Mag.* 1857, vol. xiii. p. 1); (2) to chemical action, which sometimes overpowers the direct influence of heat and reverses the effect; or (3) to both these influences combined. The more ultimate cause, however, of the phenomena in these causes must be sought for in the *molecular movements* produced by heat in the metals and liquids.

The current obtained with copper plates were no doubt influenced in their amounts (if not also in their direction) by the oxidising action of the air upon the liquid and metal at their line of mutual contact, for we know that metals in contact with liquids oxidise much more quickly if oxygen has access to their wet surfaces; and the currents were also influenced by the action of unequal temperature upon the air-contact line, for we know that wet metals oxidise still more rapidly if heat is applied.

General Conclusion.—The electric currents produced by the direct influence of unequal temperature or friction of platinum or copper electrodes, in conducting liquids which do not act chemically upon those metals, have their origin in temporary changes of cohesion of the layers of metal and liquid which are in immediate and mutual contact, and may be considered as a very delicate test of the kind and amount of temporary molecular movements produced by those causes.

"Further Experiments on the effect of Diet and Exercise on the Elimination of Nitrogen," by E. A. Parkes, M.D., F.R.S.

"Magnetic Observations made during a Voyage to the North of Europe and the Coasts of the Arctic Sea in the Summer of 1870," by Capt. Ivad Belavenetz, I.R.N., Director of the Imperial Magnetic Observatory, Cronstadt.

"On the Mutual Relations of the Apex Cardiograph and the Radial Sphygmograph Trace," by A. H. Garrod, of St. John's College, Cambridge.

Geological Society, February 22.—Mr. Joseph Prestwich, F.R.S., president, in the chair. Mr. John Thornton Harrison, C.E., and Mr. M. Hawkins Johnson, were elected Fellows of the Society.—The following communications were read:—1. "On supposed Borings of Lithodomous Mollusca," by Sir W. C. Trevelyan, Bart., M.A., F.G.S. The author referred to Mr. Mackintosh's paper on this subject (*Q. J. G. S.* vol. xxv. p. 280), and stated his conviction, from examination of specimens, that the holes in question are the work of *Helices*, or other terrestrial Mollusca. He ascribed the same origin to the so-called "*Pholas*-borings" in the limestone at Orme's Head and elsewhere. He considered length of time to be a necessary element in the formation of these holes. The author also remarked that he had suggested a glacial origin for the terminal curvature of the laminæ of slate-rocks as early as 1849. Mr. Gwyn Jeffreys read extracts from a work published by the Rev. Mr. Hodgson in 1827, on the Natural History of Northumberland, in which these borings in limestone were referred to the action of snails. Mr. Jeffreys considered the foot to be the sole instrument employed by the boring Mollusca in excavating their burrows. He exhibited specimens of Lias from Lyme Regis perforated by *Pholas*, and of hard limestone from Malta perforated by *Lithodomus*, and remarked, in connection with the notion that asperities on the shell might be boring agents, that the shell of *Lithodomus* is perfectly smooth. Prof. Ramsay mentioned that he had seen *Helices* taken out of these holes at Tenby by Dr. Buckland, who believed that the snails effected the perforations by the agency of an acid. Mr. Charlesworth thought that if so much uncertainty could prevail upon such a subject, it threw great doubt upon some of the grandest generalisations of geology. He referred to the evidence connected with the glaciation of the Great Orme's Head, in which the origin of the perforations under discussion was of much importance, Mr. Darbyshire maintaining that they were the work of *Pholades*, while Mr. Bonney asserted that they were produced by snails. In the same way the origin of the celebrated borings in the Temple of Jupiter Serapis might be disputed, and the generalisation founded upon it rendered doubtful. Mr. Charlesworth noticed the necessarily small proportion of borers to the whole snail population of Britain, and remarked especially upon the absence of perforations in the chalk districts. He considered that repeated observations were necessary before this snail-engineering could be admitted, and suggested a systematic course of experiments. Mr. Boyd Dawkins suggested that the carbonic acid exhaled by snails in respiration might act upon limestones, and remarked that chalk weathers too rapidly to preserve the excavations.—2. "On the probable Cause, Date, and Duration of the Glacial Epoch of Geology," by Lieut.-Col. Drayson, R.A. In this paper the author started from the fact that the pole of the ecliptic could not be the centre of polar motion, as the pole varied in its distance from that centre. He indicated the curve which the pole did trace, and this curve was such as to give for the date 13,000 B.C. a climate very cold in winter, and very hot in summer for each hemisphere. The duration of the glacial epoch he fixed at about 16,000 years. The calculations resulting from this movement were stated to agree accurately with observation. Prof. Ramsay inquired whether the author's theory involved the recurrence of glacial epochs, and whether he considered the course of phenomena to be constant in early astronomical epochs. Rev. Osmond Fisher inquired whether the theory was founded on observed facts, or whether it was a purely physical theory. He also asked whether the line representing the change in the direction of the pole formed a re-entering curve, and whether the theory would account for the climate of Greenland in Miocene times. He suggested changes in the form of the earth which must have affected the direction of its axis. The President remarked upon the difficulty that arose from astronomical theories differing so much among themselves. He referred particularly to Adhémar's theory, and remarked that the difficulty connected with it is, that it invokes a recurrent cause, which must produce similar effects every 21,000 years, whilst there is very little evidence of glacial action during the whole long period of the Tertiary epoch. The author, in reply, stated that he could not go back beyond 30,000 years, but that he thought glacial conditions must recur. He had not astronomical data beyond 2,500 years, and these were very vague. The motion would be the same in kind, but uncertain in degree. His theory was based entirely upon observed facts. In laying down the curve, he considered it safe to go as far as the semicircle, as he had observations covering 40°; but he could not say whether the curve would be a re-entering one, although it showed a tendency that way, and would certainly be very nearly so. With regard to the change of climate of Greenland, as evidenced by its Miocene flora, he was not sufficiently versed in botany to pronounce an opinion. He remarked, in conclusion, that the distance of a planetary body from the sun did not seem to affect climate, and stated that Venus is at present suffering under a most severe glacial epoch.—3. "On Allophane and an allied Mineral found at Northampton," by Mr. W. D. Herman. In this paper the author gave analyses of an amorphous, translucent, reddish-yellow mineral, found incrusting sandstone in the ironstones of the Northampton sands, the comparison of which with Mr. Northcote's analysis of allophane from Charlton leads him to infer the identity of the two minerals. He also noticed a soft white substance found in certain joints in a section of the Northampton sand, and also referred to allophane by the late Dr. Berrell, who analysed it. This substance was said to occur not unfrequently in the inferior oolite of the Midland Counties. By analysis, it was shown to agree nearly with Samoite and Halloysite. Mr. David Forbes stated that he had found phosphoric acid in the first-mentioned mineral, which was perhaps the cause of its lustre. The mineral was probably not pure allophane. Prof. Morris suggested a chemical and microscopical examination of the strata above the places in which these minerals occur, which would probably reveal the conditions under which they have been formed. They were probably produced by the decomposition of silicates in the overlying rocks during the percolation of water. This applied also to the Charlton locality. Mr. Carruthers mentioned that allophane often fills the inflorescence of the Cycads of the Yorkshire Oolite, entirely destroying the vegetable structure, and that it also occurs in clay nodules from the coal-measures. Mr. Carruthers suggested that the decomposition of vegetable matter in clays might aid in the production of the mineral. —4. "Notes on the Peat and underlying Beds observed in the construction of the Albert Dock, Hull," by J. C. Hawkshaw, Esq., M.A., F.G.S. The Albert Dock is situated on the foreshore of the River Humber. The excavation for the dock extended over an area of about thirty acres, and they were carried down to a depth varying from 8 feet to 27 feet below low water of spring-tides. Beneath the more modern deposits of Humber silt a bed of peat, Hessle Clay, Hessle Sand, and purple clay, were successively met with. The peat was found at the west end of the Dock at the level of low water; at the east end the bed dipped so that the upper surface was found at eight feet below the level of low water. In the peat were found the remains of a fire, which the writer attributed to human agency. Oak-trees of large size were imbedded in the peat, some of which had grown where they were found, as was shown by the stools remaining with the roots penetrating the Boulder-clay beneath. In one oak-tree, five feet in diameter, a hole was found filled with acorns and nuts. Many of the nuts were broken open at the ends, and had evidently formed part of the store of a squirrel. Remains of Coleoptera were found, and one horn-core of a *Bos*. The excavation did not extend below the upper parts of the purple clay. Some of the borings, however, penetrated the chalk at a depth 85 feet below low water level, passing through a bed of sand 16 feet thick below the purple clay. Several thousand cubic yards of this sand were brought up into the foundations by springs of water which flowed up through old bore-holes. The abstraction of this sand from beneath the clay-bed caused it to subside many feet. The writer thinks that analogous subsidences may take place from natural causes; for instance, where large springs occur in tidal rivers. Two sections exhibited showed the beds above the chalk for a distance of rather more than a mile along the foreshore. The Hessle Sand was shown to thin out to the westward. It does not, in the writer's opinion, increase in thickness in that direction, as it was shown to do in a section already published in the Proceedings of the Society. The President remarked upon the singularity of the occurrence of a bed of ashes at such a depth in these deposits. Mr. Gwyn Jeffreys referred to the President's paper on the Kelsey Hill beds, and remarked on some of the mollusca obtained by Mr. Hawkshaw. Mr. Boyd Dawkins mentioned the occurrence of a submarine forest on the coast of Somersetshire, forming a layer of peat, beneath which was a land-surface, on which the forest had grown, and in which flint-flakes were found at Portlock and Watchet on digging through the peat. He remarked on the depression of the coast of Somersetshire within the human period, and suggested that the forest at Hull may have been contemporaneous with that of Somersetshire. Prof. Morris inquired whether any trees or roots were found as when growing. The shells obtained were estuarine. Prof. Morris remarked on a submerged forest near Whittlesey, with terrestrial plants and freshwater shells imbedded in the overlying clay. The author, in reply, stated that the trees had fallen where they grew. The general appearance of things led him to the belief that the fire which had destroyed part of the forest was of human production. The following specimens were submitted to the meeting:—Specimens of Allophane, from the Northampton Sands; exhibited by Mr. F. V. Runler, F.G.S. Specimens of Websterite, from the junction of the Tertiaries and the Chalk, near Bromley, Kent, exhibited by Mr. W. Whitaker, F.G.S. Specimens from the Peat-beds at the Hull Albert

Dock; exhibited by Mr. J. C. Hawkshaw, F.G.S., in illustration of his paper. Specimens of *Pholas*-borings from Lyme Regis, and of *Lithodomus*-borings from Malta; exhibited by Mr. J. Gwyn Jeffreys, F.R.S.

Chemical Society, March 2.—Prof. Williamson, F.R.S., president, in the chair. The following gentlemen were elected Fellows:—G. D. Harding, W. H. Hudleston, A. H. Mason, J. J. Nicolson. The following papers were read:—"On the distillation and boiling point of glycerin," by T. Bolas. It is known that when glycerin is heated under the ordinary atmospheric pressure so much as to cause ebullition it is more or less decomposed. This decomposition may be, however, prevented by a reduction of the pressure in the apparatus employed. The author has in that way found that pure glycerin boiled under a pressure of 12·5^{mm} at 179·5° C and under a pressure of 50^{m} at about 210° C.—"On the action of Heat on Silver Nitrite," by Dr. E. Divers. The products of this action consist principally of silver nitrate, reguline silver, and oxides of nitrogen. But the relative proportions of the quantities of these substances to each other, and consequently the composition also of the gaseous matter, vary considerably in different experiments. When the nitrite is heated in an open vessel over a lamp or in an oven at any temperature between 85° and 140° C. the result of the operation may be represented by the equation $3\,NO_2\,Ag = N_2\,O_3 + Ag_2 + NO_3\,Ag$. When, instead of an open crucible, a closely covered one is used, so that the gaseous and fixed products of decomposition may be kept for a time in contact, the ultimate change effected in this way approaches, though not closely, to what is expressed by the equation $2\,NO_2\,Ag = NO + Ag + NO_3\,Ag$. In a third series of experiments, where the nitrite was heated in a vessel only nearly closed, the facts observed show that there is a tendency to yield only metallic silver and nitrogen peroxide, thus: $NO_2\,Ag = Ag + NO_2$. From all his experiments Dr. Divers draws the conclusion that like other silver salts the nitrite splits up under the influence of heat into metallic silver and the acid radical or its components, and that silver nitrate, nitric oxide, and, perhaps, nitrous anhydride are formed only by secondary reactions. The fusion which occurs in the mass of heated nitrite so soon as it has undergone some oxidation causes the author to throw out the suggestion that the nitrate formed perhaps combines with the nitrite to a nitrite-nitrate or hyponitrate.—After the reading of the above papers Dr. Gladstone communicated some remarks on the "Relations of Chemical Reaction and Time." He had instituted most varied experiments bearing on this subject, and, in briefly mentioning some of them, he wished to call the attention of chemists to this wide field of inquiry. Hitherto experimentators seemed to have limited their observations to only the circumstances at, and the products with, which a chemical reaction begins and ends, all that happens between was left wholly unnoticed. How fruitful attention paid to the intermediate products of a reaction could be is seen in the beautiful results which Prof. Williamson had gained on his researches on Etherification. The President, Dr. Odling, Mr. Vernon Harcourt, and others concurred in Dr. Gladstone's view as to the importance of a closer study of this subject.

Anthropological Institute of Great Britain and Ireland, March 6th.—Dr. R. S. Charnock, Vice-president, in the chair.—The following new members were elected:—Messrs. C. P. L. Naidoo Garroo, Henry Cook, Joseph Sharpe, LL.D., Danby P. Fry, Charles Edward Moore, Jesse Tagg; and W. S. W. Vaux, F.R.S., an honorary member.—Colonel Lane Fox exhibited a flint implement from Honduras.—Mr. Edward Blyth exhibited some cloth from West Africa.—Mr. Josiah D. Harris read a letter from his son on some remains found in the Macabi Islands, Peru.—Mr. J. W. Jackson read a paper "On the Racial Aspects of the Franco-Prussian War." After some remarks on the Aryan and Semitic divisions of the so-called Caucasian race, the former being defined as the flower of a Turanian, and the latter of a Negroid root, the author said that in the present imperfect state of our knowledge, it was impossible to decide whether Europe or Asia should be regarded as the primal and appropriate habitat of the Aryan, although he inclined to the former hypothesis. Neither could we yet assign the date when, and the place where, the various sub-divisions of this great race originated, and so must be contented with the fact of finding Slavons, Iberians, Teutons, and Celts on their existing areas of occupation, when, like the flora and fauna that accompany them, they must be regarded as Telluric organs. From a rapid survey of the earlier periods of European history, it was shown that the Celtic area of Gaul and Britain must have been ethnically effected at the time of the Roman conquest, which civilised but did not physically regenerate the Provincials. This was effete at the Gothic conquest of the Empire, when the Gauls received a slight and imperfect, and the Britons an effectual, baptism of Teutonic bone and muscle. The result of this diversity of fortune is seen in the fact that France, which retained more of the refinement, and with this more of the corruptions, of classic culture than Britain, preceded the latter in the attainment of civilisation, and now, after some centuries of quasi-imperial leadership in literature, science, manners and taste, is once again sinking into national weakness as an inevitable result of racial exhaustion. Hence it is that she no longer produces masterminds in any department, not even in war. Where are the successors of Cuvier and La Place, of Corneille, Racine, and Voltaire? This ethnic collapse of France, however, does not necessarily imply a subsidence of the entire Celtic area of Western Europe, as Britain is still at her maximum of racial vigour, and, like Rome after the decadence of Greece, will probably inherit that portion of the mission of imperial leadership forfeited by her effete sister and former rival. The Germans cannot do this, having so recently attained to unification, and being consequently devoid of any great capital like London, which may serve as the future metropolis of cultivation. Their mental constitution is, moreover, not adequately synthetic for the mission of Imperial centrality, which must accordingly devolve on England, the geographical terminus of the great north-western march of empire from the Euprates to the Thames. Discussion having ensued, on the motion of Mr. Joseph Kaines, seconded by Capt. Pim, it was adjourned till the 20th instant.

Linnean Society, March 2.—Mr. G. Bentham, president, in the chair. The following papers were read:—On the Tamil Mames of Plants, by Rev. S. Mateer; Contributions towards a Knowledge of the *Curculionidæ*, by Mr. H. P. Pascoe.

Royal Institution of Great Britain, March 6.—Sir Henry Holland, Bart., M.D., F.R.S., president, in the chair. Messrs. W. Blenkin, J. Browning, E. Maynard Denny, F. A. Eck, Sir Frederick Elliot, K.C.M.G., Col. A. Lane Fox, Mr. R. Graham, Col. J. A. Grant, C.B., Mr. E. W. Grubbe, Dr. G. Harcourt, Capt. F. Helbert, Mr. G. W. Henderson, Mr. G. Middleton Keill, Dr. J. Kennedy, Mr. J. Macauley, Mr. K. R. Murchison, Mrs. Sheffield Neave, Mr. G. W. Royston Pigott, Mr. Eustratrios Ralli, Mr. F. S. Reilly, Mr. W. C. Roberts, Mr. W. Dehague Routh, Mrs. W. C. Smith, Mr. T. Sowerby, were elected members.

MANCHESTER

Literary and Philosophical Society, February 7.—Mr. E. W. Binney, F.R.S., president, in the chair. "On the Organisation of an Undescribed Verticillate Strobilus from the Lower Coal Measures of Lancashire," by Professor W. C. Williamson, F.R.S., &c.—"The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena, part ii.," by Prof. Osborne Reynolds, M.A.—"Further Experiments on the Effects of Cold upon Cast Iron," by Peter Spence, F.C.S., &c.

February 21.—Mr. E. W. Binney, F.R.S., president, in the chair. "The Overthrow of the Science of Electro-Dynamics," by John Hopkinson, D.Sc. In science no theory should be considered unquestionable and no man's work held sacred from attack, and our scientific periodicals should afford the freest scope to discussions no matter how hostile to established notions. Still, it is evident that the journals ought not to publish everything that may come to hand; they should at least take care that a hostile critic understands the meaning of what he criticises. Two papers appeared last month in the *Quarterly Journal of Science* and the *Chemical News* respectively, in which the author (the Rev. Mr. Highton) somewhat summarily disposes of the science of Thermodynamics, fancying he has disproved the equivalence of heat and work. I will only trouble you with one or two quotations with a view to support my opinion that the papers in question ought never to have been permitted to appear in any journal pretending to scientific position. In the *Chemical News*, p. 42, we find, speaking of Joule and Scoresby's experiments on electro-dynamic engines—"They say that 'the quantities of zinc consumed' (that is, respectively, when the engine is at rest and doing work) 'being as a to b, $(a-b)$ represents the quantity of heat converted by the engine into useful mechanical effect.' Therefore, since on the supposition of a mechanical equivalent of heat a grain of zinc consumed equals 158 foot pounds, if x = pounds raised a foot-high per consumption of a grain of zinc in the battery,—

$$x = \frac{(a - b)\ 158}{a}$$

Hence the authors draw the conclusion:—'Therefore when b vanishes, or becomes infinitely small, the economical duty is a maximum.' Certainly this is a most startling result; that the maximum of work should be done when no zinc at all is consumed." The last sentence is a mis-statement of the conclusions of Joule and Scoresby's paper, in which (*Philosophical Magazine*, vol. 28, p. 451) it is stated that "the economical duty will be a maximum when b vanishes or becomes infinitely small in comparison with a. In this case $x = 158$, while the power of the engine will become infinitely small with regard to work performed in a given time." Comparing the phrases "economical duty" and "maximum of work," as he uses them, he evidently confuses the duty of an engine with the whole work done by it. A little further on we have—"They calculate the maximum theoretical power of a grain of zinc to be 158 foot pounds, and yet using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electromagnets never approaching the permanent magnets nearer than ¼ of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at haphazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 foot pounds out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased tenfold." It is hardly credible, but the above looks very like a confusion between Force and Work! The author seems to assume that if the forces in operation in an engine are greater, that the engine will necessarily produce more work from the same quantity of fuel. In these experiments the quantity of zinc $(a - b)$ used to produce work W is observed; if the engine was made more powerful, if the permanent magnets were four times as strong, and the electromagnets passed ⅛ of an inch from them, doubtless W would be greater, but so also would $(a - b)$, and it does not follow that $\frac{W}{(a - b)}$ with which we are concerned would be at all changed. What becomes then of the dogmatic assertion that the duty of a grain of zinc could be increased tenfold? Now let us turn to the paper in the *Quarterly Journal*. Here we may find enough in one article for our present purpose, taking chap. ii. art. 2,—"Why are we forced to suppose . . . . . . that the same amount of fuel produces the same amount of energy, whether it is consumed in the steam-engine, the horse . . . . . . the gnat? At any rate, we may observe that the very phrase is certainly a misnomer, and a misnomer of such a kind as to have a fatal effect in producing a false conception of things. For mechanical energy just as often produces cold as heat; it may produce either heat or cold, or neither. In fact, as a general rule, though with notable exceptions, every pushing or compressing force produces heat, and every pulling or expanding force cold. Place a weight on a pillar, and the weight produces heat in the pillar; hang it on a wire and it cools the wire. In exactly the same way, in a fire-syringe use force to press down the piston, it produces heat—heat enough to kindle tinder; but use the same force to pull up the piston, and it produces cold." Surely this is enough to show that the author's notions of what he is attacking are, to say the least of it, shallow; for what he quotes as paradoxes are simple deductions from the two laws of Thermodynamics. That a wire is cooled by stretching follows from the fact that heat expands it. In the case of the fire-syringe the case is simpler. The working body is the air in the syringe; on pulling up the piston this air does work, and therefore uses up heat and is cooled. Mr. Highton seems to imagine that because the arm of the experimenter does work, it is done *on the air in the syringe*, whereas this column of air and the observer are really co-workers in raising the air *external* to the cylinder. To point out all the fallacies of these papers in detail would take too much of your time. My object was to show that if the *Quarterly Journal of Science* and the *Chemical News* are to represent scientific opinion with any degree of truth, they would do well to use a little discretion as to what they print.—"Remarks on Mr. Spence's Experiments on the Effects of Cold on the strength of Cast Iron," by Joseph Baxendell, F.R.A.S. This was in reply to an assertion made by Mr. Spence the previous week that "he had so much confidence in the experiments then detailed, that he had no hesitation in giving it as an ascertained law that a specimen of cast iron having at 70° F. a given power of resistance to transverse strain will, on its temperature being reduced to zero, have that power increased by 3 per cent." Taking all the experiments on the effect of cold on iron which have yet been brought before the Society, they can only be regarded as indicating that if any effect at all is produced, it is more apparent on iron of good quality than on inferior iron, but that its amount is so small as to be wholly inadequate to account for the railway and other accidents which have been attributed to it.—"Further Observations on the Strength of Garden Nails," by J. P. Joule, F.R.S., &c. Since communicating the paper on the Alleged Influence of Cold in giving brittleness to Iron, I have collated the results with cast-iron nails in order to show the range of strength in such specimens:

Height of Fall of Hammer.	Percentage of Fractures
2 inches	0
2½ "	0
3 "	6·25
3½ "	23·5
4 "	30
4½ "	36·4
5½ "	37·5
6½ "	48
7 "	65·5
7½ "	62·3
8½ "	75
10 "	92·8

I chose the garden nails for experiment after some thought, as presenting a marked variety of metal in contrast with the iron and steel wire, tempered and untempered. I did not expect them to possess great strength, but having found them to require a heavier blow than I expected to fracture them, I have had the curiosity to make some experiments on them which may be interesting to the Society. I took pairs of the nails, placed them head to point parallel to each other, so that pressure applied in the middle by pincers sufficiently forcibly would fracture one of them. Paper slips were pasted on the edges of the nails, and their distances asunder measured by a microscope with micrometer eyepiece divided by lines corresponding to $\frac{1}{300}$ of an inch. Weights were gradually added to the lever of one arm of the pincers until fracture took place, which was always accompanied with a sharp report. The observed deflection or bending of the nails was taken continuously as the weights were laid on, and the calculation of what it would have been at the moment of rupture taken from the immediately preceding observations. The amount of deflection was almost exactly proportional to the weight laid on in each experiment.

No of Experiment.	Length of Nail between Supports.	Breadth of Nail in Fracture.	Depth of Nail at Fracture.	Deflection.	Breaking Weight. lbs.
1	1·05	0·13	0·127	·0062	145·5
2	1·1	0·114	0·125	·0067	141
3	1·1	0·120	0·115	·0090	171
4	1·08	0·111	0·106	·0073	142·5
5	1·12	0·122	0·145	·0098	189
6	1·06	0·138	0·120	·0087	184·5
7	1·08	0·150	0·118	·0095	201
Average	1·084	0·1264	0·1223	·0082	167·8

If we compare the above with Mr. Brockbank's experiments, we shall find, approximately, on reducing them to the dimensions he adopted, viz., three feet between supports and one inch section:—

	Breaking weight.	Deflection.
Mr. Brockbank's, with large bars...	860·7	·740
My own, with nails	2673	1·106

The metal, in the form I used it, was therefore more *than* three times as strong as that of the large bars to resist a compressing and tensile force, while its extent of spring at the breaking weight was half as much again. Therefore, so far from being of inferior quality, it would sustain a very much heavier blow without fracture.—"On the Action of Sulphurous Acid on Phosphates," by Dr. B. W. Gerland.

MAIDSTONE AND KENT

Natural History and Philosophical Society, Feb. 20.—An aggregate meeting of the members of the Maidstone and Mid Kent Natural-History Society was held at the Charles Museum, when Dr. Monckton, one of the vice-presidents, delivered a lecture on "The Metaphysics of Zoology."

EDINBURGH

Royal Society, February 20.—Mr. W. F. Skene, V.P., in the chair. The following papers were communicated:—1. "On the Pentatonic character of Scottish Music," by the Hon. Lord Neaves. 2. "On the Motion of Solids in a Liquid," by Sir W. Thomson. 3. "Laboratory Notes on Thermo-electricity, and on Phyllotaxis," by Prof. Tait.

PARIS

Academy of Sciences, January 27.—M. Janssen sent to the French Institute a letter to explain how his expedition failed, owing to the persistency of the clouds. He acknowledges fully the kind proposition of the Organising Committee to take steps to procure a *laissez-passer* from M. Bismarck on his behalf. In five hours M. Janssen proceeded from Paris to the mouth of the Loire, where he landed on the 2nd December at 11 o'clock in the forenoon. The journey was magnificent. M. Janssen travelled at a height of 1,100 metres at the beginning, but after sunrise his balloon elevated itself through the warmth of the sun, and he reached 2,000 metres without throwing out any ballast. M. Janssen has invented an instrument for helping aëronauts in the determination of their way along the earth. This contrivance, which was highly praised by M. Dumas, will be fully described.—M. Bazin presented a projectile gun which explodes in the air, where it is sent by a larger gun remaining on the ground. This projectile gun, when exploding at a certain point of the *trajectoire*, sends an explosive bullet with a new impulse. It is impossible to claim precision for such a projectile, which is called very properly *a double effect*, but the distance to which it reaches is increased, and the final bullet was sent to 7,919 metres, it appears with some effect. The experiment was tried against the Prussians during the last days of the siege.

February 21.—The sitting was very thinly attended by members. The public was more numerous. No scientific paper had yet resumed its publication. M. Faye presided over the sitting. M. Elie Beaumont and M. Dumas sat on the platform, reading over alternately the correspondence. An article on the "Duquesne" Expedition was sent by M. de Fonvielle. M. Elie de Beaumont, who had predicted that the "Duquesne" would go to Switzerland on account of its directing power, opposed the reading.—M. Stanislaus Meunier read a paper on the nature of meteorites, which, he thought, are evidently of astral nature. He is opposed to the theory of Schiaparelli, who accounted for them by supposing they are in some respects allied to comets.—M Chevreuil read over a letter from M. Vaillant, who hopes to be soon enabled to resume his seat.—M. Delaunay read the translation of a letter received from Prof. Piazzi Smyth, the director of the Observatory at Edinburgh. He noticed the admirable working of the International French Telegraphy. That service was transferred to Tours and from Tours to Bordeaux, where it is now working under M. Marié Davy's superintendence.—M. Ch. Saint Claire Deville reported that Dr. Berigny of Versailles had not interrupted for a single day his admirable series of observations kept regularly for 24 years. M. Renan, who is living at Vendôme, did not interrupt his observations, although he was several times arrested as a spy, but he was saved by his thermometer! The maximum of cold has reached -16° at Montpellier, -17° at Bordeaux, and -3° at Perigueux. The winter was a sharp one. It was predicted by M. Renan, as belonging to a series of recurring sharp winters. This law of recurrence was published in *Comptes Rendus* eighteen months before war broke out.—News was circulated of other learned men amongst the members. M. Martillet remains at St. Germain as curator of the Prehistoric Museum. One new hall was opened during the Prussian occupation, the Prussian Emperor, Princes, and generals frequently visiting the galleries. M. de Verneuil has established himself at Duc de Broglie's castle in Normandy. He is a relative of the present French ambassador, who is very much interested in scientific matters. M. Janssen is at Bordeaux, and expected daily by his family.

DIARY

THURSDAY, MARCH 9.

ROYAL SOCIETY, at 8 30—Magnetic Observations made at Stonyhurst College Observatory, from 1863 to 1870: Rev S. J. Perry.—Preliminary Notice on the Production of the Olefines from Paraffin by Distillation under Pressure: T. E. Thorpe and J. Young.—On the Action of Hydrobromic Acid on Codeia: Dr. C. R. A. Wright.

SOCIETY OF ANTIQUARIES, at 8 30—On a probable allusion to the Chsritians in a passage of the Sixth Satire of Juvenal: Earl Stanhope, P.S.A.

LONDON MATHEMATICAL SOCIETY, at 8.—Remarks on the Mathematical Classification of Physical Quantities: Dr. Clerk Maxwell, F.R S—On Skew Cubics: Prof. H. J. S Smith, F.R.S.—Note on the History of Certain Formulæ in Spherical Trigonometry: I Todhunter, F.R.S

ROYAL INSTITUTION, at 3—Davy's Discoveries: Dr Odling.

LONDON INSTITUTION, at 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers, M.A.

FRIDAY, MARCH 10.

ROYAL ASTRONOMICAL SOCIETY, at 8

QUEKETT MICROSCOPICAL CLUB, at 8.

ROYAL INSTITUTION, at 9—The latest Scientific Researches in the Mediterranean and Straits of Gibraltar: Dr. W. B Carpenter, F R.S.

ROYAL COLLEGE OF SURGEONS, at 4—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, MARCH 11.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold, F.R.S. (Swiney Course.)

ROYAL INSTITUTION, at 3.—Spirit of the Age: Mr. O'Neil.

SUNDAY, MARCH 5.

SUNDAY LECTURE SOCIETY, at 3.30.—On Ferns: Dr. Cobbold, F.R.S.

MONDAY, MARCH 13.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 4.—On Astronomy: R. A. Proctor, F R A.S. (Educational Course.)

ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

TUESDAY, MARCH 14.

PHOTOGRAPHIC SOCIETY, at 8.

ROYAL INSTITUTION, at 3—Nutrition of Animals: Dr. Foster.

WEDNESDAY, MARCH 15.

SOCIETY OF ARTS, at 8.—On the Different Methods of Extracting Sugar from Beet-root and Cane: Ferdinand Kohn.

METEOROLOGICAL SOCIETY, at 7.—Evaporation, Rainfall, and Elastic Force of Vapour: J. R. Mann.

ROYAL SOCIETY OF LITERATURE, at 8.30.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

THURSDAY, MARCH 16.

ROYAL SOCIETY, at 8 30.

SOCIETY OF ANTIQUARIES, at 8.30

ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

LINNEAN SOCIETY, at 8.

CHEMICAL SOCIETY, at 8.

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THURSDAY, MARCH 16, 1871

NATURAL HISTORY MUSEUMS

THE notes upon Natural History Societies which have already appeared in these columns * would be very incomplete unless some reference were made in connection with them to the subject of local Museums. We would by no means have it inferred that a Museum should be looked upon as an essential adjunct to every local society; nor would we urge upon any such body the formation of a Museum, unless the society was in a position to warrant the supposition that, if once started, there would be every probability that it would be kept up; but, at the same time it must be admitted that a well-arranged collection of objects in connection with the more important of our Field-clubs, especially such as are far from London or some other centre, would be extremely useful.

But, to be of any service at all to working naturalists, a local Museum must be a very different sort of thing from the Museums with which those who have visited country towns are familiar. Of course, no reference is intended to such collections as those at Liverpool or Manchester; but to those smaller ones which might be made very useful, but are at present practically useless, of which that at Canterbury may be taken as a fair type. Here the jumble of curiosities and objects of natural history is (or was, when we visited it in 1867) sufficiently remarkable; and the want of arrangement yet more striking. Ancient pottery and various implements of savage warfare, pieces of the *Royal George*, and samples of the meaningless curiosities which are brought home by travellers to their friends and relations, and are often got rid of by the said friends and relations on the first opportunity—these are present in full force. There are also drawers of foreign insects, unnamed, and more or less damaged; some stuffed birds; a good many fossils, the labels of many of which are misplaced; a small collection of dried plants, rapidly succumbing to the ravages of insects; and various other objects. This is, as far as we can judge from experience, a fair example of a country Museum, certainly by no means the worst which we might have selected.

Such a state of things as this may be contrasted with the following description of what provincial Museums ought to be, which we extract from Dr. Hooker's Address to the British Association in 1868:—"Each should contain a connected series of specimens, illustrating the principal and some of the lesser divisions of the animal and vegetable kingdom, so disposed in well-lighted cases, that an inquiring observer might learn therefrom the principles upon which animals and plants are classified, the relations of their organs to one another, and to those of their allies, the functions of those organs, and other matters relating to their habits, uses, and place in the economy of Nature. Such an arrangement has not been carried out in any Museum known to me, though partially attained in that at Ipswich; it requires some space, many pictorial illustrations, magnified views of the smaller organs and their structure, and copious legible descriptive labels, and it should not contain a single specimen more than is wanted. The other requirements of a provincial museum are, complete collections of the plants and animals of the province, which should be kept entirely apart from the instructional series, and from everything else."

One of the most hopeful experiments in connection with the establishment of a local Museum, is that which has just been set on foot at Folkestone by the Natural History Society of that place. There has for several years been a Museum at Folkestone, although visitors to the town may not have been aware of it; the management, or mismanagement, of it was vested in a committee annually elected from the town council for that purpose; and until lately admission could only be obtained by an order from one of these functionaries. The Museum, when we visited it in 1867, contained a very valuable collection of fossils, chiefly local, including a fine set from the unique Junction Bed of Folkestone; a few birds and fishes, and one or two other objects, the whole thickly covered with dust, and in a disgraceful state of neglect. In 1868 the Committee of the Natural History Society offered to take charge of it, on condition that they might have the use of the room for meetings, &c., and this offer was accepted by the Town Council, subject to the agreement of the Committee to provide glass cases for the objects at their own expense. This they very properly refused to do, and matters remained thus until the election of a fresh Council, when the Society again applied. The Museum was ultimately placed in the charge of the Society, the Town Council providing fittings to the amount of fifty pounds, and an annual payment of ten pounds towards expenses. The objects were at once arranged and named, and the Museum was formally reopened on the 4th of last October. It is now opened to the public twice a week, the secretary, Mr. Ullyett, to whose exertions the improved state of affairs is mainly owing, being in attendance to give information if required. The meetings of the Society are held in the Museum. The aim of the Society is to make a complete local collection, as well as a type collection, on the plan suggested by Dr. Hooker as above quoted; and to this contributions are solicited. A library for reference is also in course of formation.

We have entered thus into detail because the history of the Folkestone Museum is by no means uninstructive, showing as it does how readily such local collections are allowed to become utterly useless; and how, in restoring such Museums to the use of the public, a local society is doing a good work which only such a body could perform the remonstrances of mere individuals being powerless in such matters. We should be glad if other Field-clubs having a Museum within the radius of their operations, would exert themselves in the same way; the East Kent Natural History Society, for example, might well reorganise the Canterbury Museum; and examples might easily be multiplied.

A Museum may be looked upon as especially useful in a school society; and we have already referred* to that existing at Marlborough as presenting very satisfactory features. Mr. Preston, who has worked so hard on behalf of this society, is anxious to establish a typical collection in addition to the British one, which has already attained considerable proportions. The series of botanical diagrams lately prepared for the Science and Art Department at South Kensington, under the superintendence of Professor Oliver, will be valuable for such collections, so far as botany is concerned. The Museum at Clifton College, built by Mr. Perceval at a cost of 900*l.*, will, we understand, be confined to British objects, and will be a combination of Museum and Library.

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* See NATURE, vol ii p 469, vol. iii. p. 161.

* NATURE, vol. ii. p. 249.

In conclusion, it cannot be too strongly insisted that objects of *vertu* or of curiosity should be rigidly excluded from any Museum, be it large or small, which is ostensibly set apart for the illustration of Natural History. It may require a certain amount of firmness to draw the line, and so run the risk of offending good-natured persons by the rejection of their proffered help; but it is best to take at once a definite position, and, unless the space at command be much larger than is usual in local Museums, to refuse even objects of Natural Science which do not illustrate some typical peculiarity, or at the least tend to the completion of a provincial collection. It is better to have a few objects, well arranged, and each teaching some definite truth, than hundreds of disconnected specimens, which, however interesting in themselves, are valueless as aids to instruction.

SCIENCE IN VIENNA

IT is well to turn from time to time to what is doing in the cultivation of Science in other lands. We are able to give the following details of the progress of Science, or rather of the machinery for the cultivation of Science, in Vienna, from a letter addressed by Prof. Haidinger to Dr. E. Döll, the editor of the *Realschule*, in the December number of which periodical the letter appeared. It was written by Prof. Haidinger in commemoration of the establishment, on the 8th November, 1845, of the first Viennese association for the cultivation of pure science, the twenty-fifth anniversary of which the writer thought deserving of celebration even in the midst of the exciting events of the disastrous war then waging between two of the most advanced of European nations; events the results of which, as he justly remarks, do not constitute the highest objects of human life, but on the contrary, evils, originating only from our still imperfect civilisation.

Before the year 1845 it appears from this letter that the only scientific societies established in Vienna dealt solely with the applications of scientific knowledge. Thus the Imperial Agricultural Society was founded in 1807, with the warm interest of the late Archduke John. Its progress was interrupted by the war of 1809, and its statutes were not confirmed until 1812. In 1836 the Imperial Medical Society was founded, followed in 1837 by the Imperial Horticultural Society, and in 1839 by the Industrial Association of Lower Austria. Gatherings of German naturalists and medical men took place at Vienna in 1831, at Prague in 1837, and at Gratz in 1843.

In 1835 a step of the greatest importance was taken in the establishment of the Imperial Mineralogical Collection, which took the name of the Imperial Montanistic Museum in 1843. The instruction given at this institution was supplemented by the forms of a society.

In the year 1845, the period of the Industrial Exposition produced considerable excitement, and on the 8th of November in that year a number of young miners, medical men, and naturalists, met in the Museum, and established an association under the title of the "Friends of the Natural Sciences in Vienna." The list of those present at the first meeting includes the names of several men who have since risen to the highest reputation. Haidinger himself, then president of the Montanistic Museum, took the warmest interest in the success of the nascent society, and endeavoured to bring it into a perfect form, but, for some reason, without success. The meetings, however, were continued until the year 1850, and the subscriptions of the members enabled Prof. Haidinger to publish seven volumes of "Proceedings" in 8vo., and four volumes of "Memoirs" in 4to. The association was broken up after the foundation of the Imperial Geological Institute in 1849, and the library belonging to it was subsequently presented to that institution.

1846. The 30th of May is the date of the Imperial decree for the foundation of an Academy of Sciences in Vienna.

1847. On the 14th of May the statutes of the Imperial Academy of Sciences were promulgated, and the first forty members nominated. On the 29th of June the first functionaries of the Academy were nominated, and on the 2nd of December its first meeting took place.

1848. On the 2nd of February, the Imperial Academy of Sciences was solemnly opened by its curator, the Archduke John, and after this meeting the first part of the "Proceedings" was issued, forming the commencement of a long series of most important works in all branches of Science. The Academy of Sciences is not a society formed by the spontaneous action of its members, but rather an exclusive corporation founded by authority.

1848. On the 8th June, the Austrian Society of Engineers was founded, and is the first spontaneously-formed Society.

1849. The Imperial Geological Institute was established on the 15th November, under the Minister von Thinnfeld, on the foundation of the Montanistic Museum. This Institution partakes of the nature of a school of instruction, combined with that of a society.

1851. The 9th April witnessed the foundation of the first spontaneously-formed Natural History Society in Vienna, namely, the Zoologico-botanical Society, which owes its establishment to the exertions of Georg von Frauenfeld, who opened its first meeting on this day with an introductory address delivered in the hall of the museum of the Botanic Gardens. Frauenfeld was the first secretary of this society, a position which he still continues to hold.

1851. In this year also the Imperial Central Institute for Meteorology and Terrestrial Magnetism was founded under the direction of Karl Kreil, as a sequel to the labours of a Meteorological Committee of the Academy, appointed on the 18th January, 1849.

1853. The Antiquarian Society was formed under the presidency of Prince Aloys of Liechtenstein, on the 23rd March. On the 29th March, 1854, Dr. T. G. von Karajan was elected president. This is an independently-formed society.

1855. On the 1st December another independent society was established, namely, the Imperial Geographical Society, which held its first meeting on this day in the rooms of the Imperial Geological Institute. On this occasion the first president, Prof. Haidinger, delivered an address.

In the year 1856 much excitement was produced among the naturalists of Vienna by the meeting of German naturalists and physicians in that city, and a great impulse was given to Natural History studies by the voyage of circumnavigation performed in the years 1857-1859 by the frigate *Novara*, under the auspices of the Archduke Ferdinand Max, afterwards, to his misfortune, Emperor of Mexico. During this period also there was a movement in favour of publicity in the medical and philosophical faculties of the University, and some series of public lectures were delivered.

1860. On the 6th December the Society for the Diffusion of Physical Knowledge commenced its proceedings in the apartments of the Imperial Academy of Sciences. The first general meeting took place on the 13th May, 1861, in the hall of the Musical Society, when Prof. Eduard Suess delivered a foundation address. The proceedings and lectures may properly be carried back through the agency of Dr. J. Grailich and his associates to the year 1855, when they commenced in the meeting-room of the Imperial Geological Institute. This society has continued to give lectures on Natural Science in two places, in one of which the old forms of a society are retained, whilst in the other lectures find interested listeners.

1861. The Photographic Society, independently formed, held its first meeting under the presidency of Prof. A. Schrotter in the green saloon of the Imperial Academy of Sciences on the 22nd March. The first photographic Exhibition in Vienna was opened on the 17th May, 1864.

1862. The Austrian Alpine Club was established, its first constituent general meeting being held on the 19th November.

1864. The Lower-Austrian Society for "Landeskunde" held its first constituent meeting on the 16th December.

1865. The Austrian Meteorological Society was founded on the 16th November, with an address from Dr. Karl Jelinek.

1866. The year of the war. Prof. Haidinger retired in consequence of ill health from the direction of the Imperial Geological Institute, and was succeeded by Franz von Hauer.

1869. A section of the German Alpine Association held its meeting in Vienna.

1870. The Chemico-physical Society established under the presidency of Prof. H. Hlasiwetz.

1870. The Anthropological Society founded on the 13th February. Its opening meeting was held in the Consistorial Hall of the University, when the president, Prof. Karl Rokitansky, delivered an address.

1870. The Numismatic Society established.

It is with a considerable pride that the venerable Prof. Haidinger describes the rapid advance that has been made in the scientific progress of his native city, and dwells upon the fact that the first impulse to this movement was given by the association of the "Friends of the Natural Sciences," in which he took so much interest. The cause of the failure to form a well-established society from such a promising commencement he finds in the unfavourable conditions of the time; and doubtless the spasmodic political movements which so closely followed the year 1845 may well have distracted the attention of German men of science. The nascent society seems, however, to have merged into the Imperial Geological Institute, which has already done so much good work, and Prof. Haidinger is probably in the right when he claims for the "Friends of the Natural Sciences" in their new capacity a vigorous influence in the establishment of other scientific societies both in and out of Vienna.

The 8th November, 1845, may therefore well be "a day of joyful commemoration" with Austrian scientific men, for although, as Prof. Haidinger remarks, any retrospect reaching so far back must bring with it serious thoughts of the many participators in the first labours of the period who have disappeared from the scene, there is yet a higher point of view of a satisfactory nature, namely, that this period of twenty-five years has raised Austria to a far higher scientific position among nations than could have been claimed for her before, and, as he says, "Peaceful progress is certainly the highest and worthiest object of human endeavours."

LAUGHTON'S PHYSICAL GEOGRAPHY

Physical Geography in its Relation to the Prevailing Winds and Currents. By John Knox Laughton, M.A., F.R.G.S., &c. (London: J. D. Potter, 1870.)

THIS work is designed to show that the whole atmosphere, relatively to the surface of the earth, continually moves or tends to move from west to east, and that the permanent local variations from this direction are either eddies or deflections, formed in accordance with the principles which regulate the motion of fluids (p. 312). In the course of the discussion, Mr. Laughton has done good service by showing that prevailing opinions respecting the circulation of the atmosphere are very far from being in accordance with many well-ascertained facts; and by insisting on the dependence of oceanic on atmospheric currents, which is confirmed in every case where the facts are tolerably well known. The book also contains the best popular account we have of the prevailing winds over large portions of the ocean. But he is not so happy with respect to prevailing winds over the land, and in the reasoning he employs in proof of a general motion of the atmosphere from west to east.

It is stated that from Japan northwards the prevailing winds in summer are westerly (p. 136); and to the influence of these winds, blowing across the northern opening of the narrow seas of this part of the earth, are ascribed the southerly winds on the coast of China, and thence southward to the equator (p. 280). Now the prevailing winds of this region are not westerly in summer, the direction being E.S.E. at Tong-chow, S.S.E. at Pekin and New-Chwang, S.S.W. at Nangasaki, S.E. by S. at Chacodate, E. at Nicolajewsk near the mouth of the Amoor, and N.E. at Ajansk, S.W. also prevailing to some extent at the last place.

We are told that the wind blows almost constantly from the west on the north-west coasts of the Old and New Worlds respectively (p. 154). Now, whilst in winter the prevailing wind in Vancouver Island is S.W., at Sitka it is E.S.E., easterly winds being to westerly as 4 to 1, and E.N.E. at Ikogmut. Again, over the whole of the west of Norway, the prevailing winds in winter are either S. or S.S.E., or S.E.; at Christiania they are N.E., easterly winds being to westerly as 8 to 3—a relation which could not hold if prevailing winds in this region were westerly. At Copenhagen the direction is S.S.W. In accordance with these facts the rainfall of the west of Scotland is much greater than that of the west of Norway.

Mr. Laughton maintains that there is, throughout the sea, no trace whatever of the air being drawn in to any place of greatest heat, &c. (p. 303), and adduces many instances in support of this opinion; but, curiously enough, every case adduced confirms the opposite view. This arises from a misapprehension of the principle involved in Buys Ballot's Law of the Winds, which may be thus put: Winds flow in towards the space where pressure is weak, vorticosely, so that in the N. hemisphere, standing back to the wind, the weak pressure is to the left. To take one case as an illustration:—Because the winds at Aden and south of Arabia do not blow directly in upon the heated sands of that region, but blow from W.S.W., it is concluded that there is no trace whatever in this part of the sea of the heated air influencing the winds. Now, if it be the case that there is at this season a weakening of the pressure by an ascending current of heated air from the surface of Arabia, then, according to Buys Ballot's law, the direction of the wind ought to be what observation shows it is, viz. W.S.W., and the more powerful the ascending current, the stronger will be the W.S.W. winds on the surface.

Mr. Laughton regards it as established presumptively that a westerly upper current everywhere prevails (p. 97). This he has shown generally to be the case as respects upper currents of tropical regions and a portion of the United States, but not as respects other parts of the globe. The explanation of these upper currents is easy. If the pressure of the atmosphere, at a uniform height of say, 10,000 feet, were ascertained, the highest would be immediately over the belt of calms, and the lowest over those regions where the mean temperature of the whole stratum, 10,000 feet thick, happens to be the lowest; this result being a simple consequence of the increased tension of air by heat, and diminished tension by cold. Since the pressure at the height supposed will continually diminish on receding from the Equator on either hand, it follows from Buys Ballot's Law that the prevailing upper currents ought to be westerly within the tropics, or, stated more exactly, W. by S. north of the Equator, and W. by N. south of it. It is during the winter months that westerly upper currents prevail in the United States. Since during this season the mean temperature rapidly sinks in advancing northwards over America, atmospheric pressure will be much weaker over British America at a height of 10,000 feet than over the United States, and hence by Buys Ballot's Law the upper current of the United States ought to be westerly. It may be remarked that since westerly upper currents are flowing towards colder regions, condensation of their vapour into cloud must frequently take place, thus rendering them visible; whereas since easterly currents flow towards warmer regions, condensation will seldom take place, and hence they must frequently flow past unobserved.

The broad zone included between latitudes 30° N. and S. comprises half the superfices of the globe. Over by far the greatest portion of this zone winds are easterly, there being no season when any approach to a preponderance of westerly winds is apparent. When to these are added the easterly winds in extra-tropical regions, already referred to, a predominance of easterly winds in the north of Siberia and in the east of Australia, in their respective summers, and in a large part of the Arctic regions at all seasons; and though not a predominance yet the frequent occurrence, of easterly winds in the S. hemisphere, south of latitude 50°, it is plain that if there can be a preponderance of westerly winds over the globe viewed as a whole, it cannot be very great. A. B.

THE SEWAGE QUESTION

The Practical Solution of the Great Sewage Question, by a Combination of the Irrigation and Precipitating Processes. By William Justyne. (John B. Day.)

IN this little book the author tries to show that the A B C process is the only one which promises any hope of success among the methods which have been proposed or adopted for purifying sewage and utilising its valuable manurial constituents. "Miscellaneous schemes," including the dry-earth system, Liernur's process, and all the precipitation plans, except the favoured one, are dismissed very summarily, and the irrigation system is made to look as unsuccessful and as dangerous as its opponents have ever represented it to be, while the immense weight of indubitable facts and evidence on the other side is entirely passed over.

Then the A B C process is minutely described, and the scientific public is asked to believe that sewage, which contains suspended and dissolved organic and inorganic matters, and the chief value of which consists in the large amount of dissolved nitrogen, especially in *the form of ammonia*, which it contains, can be so purified by precipitation with a mixture of alum, blood, clay, animal charcoal, and what not, as that the effluent water shall contain no putrescible matter, and that the *precipitate* shall contain the manurial constituents!

What we do know with regard to the A B C and all other precipitating processes is, that they none of them separate out anything like all the dissolved (few of them nearly all the suspended) organic matters, and that the ammonia is necessarily all lost in the effluent water; indeed, several of these processes have been known actually to increase the amount of ammonia in the effluent water, by decomposing some of the organic matters. The only precipitating process which had the merit of attempting to throw down the ammonia was the phosphate process, which has been tried and abandoned so many times in this and other countries; the reason of its failure was very simple: the ammonio-magnesian phosphate which it was expected would be found in the precipitate, requires an excess of ammonia in the solution to render it at all insoluble, so that if any of this salt were contained in the sediment it would merely show that the effluent water still contained an excess of ammonia.

We must comment especially upon two points in which a person seeking for information is likely to be misled as to matters of fact by consulting this book. On page 17 we observe a table of the average percentage composition of solid and fluid human excreta, from which one might easily be led to suppose that the solid is more valuable than the fluid (as it is *weight for weight*), but we nowhere find the important fact added that since for every ounce of solid excreta we have about 10 ounces of liquid, the total liquid excreted in a given time is worth about seven times as much as the total solid: so that as far as utilisation is concerned, the question which has to be solved is, how to deal with the liquid and its valuable constituent urea, or rather carbonate of ammonia, for it is as such that we find it in sewage.

The other point is the statement at the bottom of page 56, that "the analysis of the effluent water, after the sewage has been treated by the A B C process, is an analysis of the effluent water *solely;* whereas in almost every case where analyses have been made of the effluent water after the irrigation process, the effluent water has been diluted by at least double and sometimes three and four times its bulk, with perfectly pure spring and subsoil water." But then, how is it that the effluent water is, except during very wet weather, so very much less in quantity than the sewage sent on to the land? Mr. Justyne does not tell us this—we will tell him; it is because three-fourths, sometimes four-fifths, sometimes much more of the water has disappeared by evaporation from the leaves of the growing plants, and to a certain extent from the surface of the ground itself, so that to compare fairly the composition of the effluent water of the A B C process with that of an irrigated field, we must dilute the latter with distilled water until its bulk is equal to that of the sewage sent on to the land; even without this correction, the purity of the effluent water from irrigated lands has never been approached by any other method; with this correction, we think we may safely say that it is never likely to be approached.

Our author suggests that irrigation with the effluent water should be practised, when necessary, as a secondary and subordinate operation to the precipitation process; we submit that it has never yet been shown that it would not always be necessary to irrigate with this effluent water, both from a sanitary and an economical point of view, and it is plain that if to purify sewage, irrigation must be resorted to, any preliminary process which separates more than the offensive suspended matters, must be disadvantageous, by lessening the value of the liquid sent on to the land.

That the suspended matters should be to a great extent separated before the sewage is sent on to the land, we hold to be indisputable, but this should be done by some simple method, which leaves the sewage as *rich as possible* in dissolved manurial constituents.

In conclusion, from a chemical point of view, we object to reliance being placed on any precipitation process for the removal of its valuable constituents from sewage, and from a sanitary point of view we object to the effluent water from precipitation tanks being sent at pleasure into streams whose water is afterwards used for domestic purposes.

W. H. C.

OUR BOOK SHELF

The Honey Bee; its Natural History, Physiology, and Management. By Edward Bevan, M.D. Revised, Enlarged, and Illustrated by William Augustus Munn, F.R.H.S., &c. (Van Voorst, 1870.)

In this new edition of Dr. Bevan's well known work, Major Munn has given a full account of all the improved hives and methods of management, and of the most recent discoveries in the economy and physiology of bees. The old and the new matter are, however, so interwoven, that it is impossible for the reader to separate them; and as the original author and his editor both speak in the first person, we find ourselves continually at a loss to know whether we are reading "Bevan" or "Munn," except in those cases where some reference to dates enables us to decide.

An interesting experiment is detailed, proving that the business of a hive may go on a long time with perfect regularity without the presence of a queen. On the 13th June a swarm was put into a mirror hive. On July 1st, whilst the queen was laying drone eggs, she was taken away, yet the bees showed no agitation, but continued their work as usual. They formed several royal cells, and examined them continually to see if eggs had been deposited in them. All through the summer work went on as usual, honey being plentifully stored; but no attempt was made to raise a queen by artificial food, nor were the drones massacred. By the middle of November all the drones had died, and the working bees then began to diminish, and by December 31st they had also died. As all the workers had been born before July 25th, this gives about six months, or not much less, for the duration of their lives.

The fortifications and barricades of the bees against the incursions of the Death's Head Moth are said to be due to reason rather than to instinct, because it has been observed that they do not commence these fortifications on a first attack of the Sphinx, nor until they have been robbed of nearly their whole stock of honey. "This is a case in which the insect is taught by experience, and which admits, in all its particulars, of a direct comparison with human reason and contrivance. A colony that had been thus attacked one year, and was tardy in its defensive operations, having derived instruction from the past, constructed fresh ramparts speedily on the reappearance of the Sphinx three years afterwards, and thus guarded itself from an impending danger. Since the lives of the working bees do not extend beyond six or seven months, it is evident that the information of the colony above referred to must have been traditional, or else derived from a queen which had reigned over them three years previously." This "tradition" through some six or seven generations seems highly improbable, and that the knowledge of how to act was derived from a queen not less so. Do not the facts rather indicate that bees differ considerably in intellectual capacity, and that some hives contain directing bees more capable of acting promptly on the defensive than others?

Much information is given on the different kinds of foreign bees, and their peculiar modes of building. The importance of bees in fertilising flowers, and the use of nectar and of the colours of flowers as attractions, are fully recognised; but the recent discoveries of Darwin on this subject are not alluded to. So, in the discussion on the hexagonal form of the cell, the "circular" theory is opposed, and Mr. F. Smith is quoted against it; but the beautiful experiments of Mr. Darwin, as described in the "Origin of Species," with the satisfactory theory founded upon them, appear to be unknown to the author. "Darwin," it is true, is very frequently quoted, but it is always Doctor, not Charles, Darwin.

The book is illustrated by woodcuts of the various kinds of hives, and of the apparatus used by Apiarians. There are also some very scratchy but characteristic etchings of the different kinds of bees and of their anatomy, and several coarse coloured lithographs of varieties of comb, royal cells, &c., all executed by Major Munn himself. Though with some deficiencies of style and arrangement, the work abounds with information useful to the bee-keeper, and interesting to the naturalist.

Alfred R. Wallace

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Eozoön Canadense

I CANNOT understand Mr. T. Mellard Reade's right to fling the taunt at those who maintain the foraminiferal nature of Eozoon, that "each disputant takes up a different position, and shifts it as occasion requires."

I have never taken up any other position than this: that *the best-preserved specimens of the Canadian Eozoön exhibit an unquestionably foraminiferal structure.* I am supported in this by every British naturalist with whom I am acquainted, as specially conversant with foraminiferal organisation, viz., by Messrs. H. B. Brady, T. Rupert Jones, W. K. Parker, and Prof. W. C. Williamson; whilst the most eminent authorities in micro-mineralogy and pseudomorphic structure, viz., Messrs. David Forbes, T. Sterry Hunt, and H. Sorby, altogether disown Eozoon as a mineral.

I have further asserted, and I do not in the least "shift" my position, that *the character of the Canadian Eozoon is altogether independent of that of later ophites.* The occurrence of true Eozoic structure in the newest Tertiaries would only show that Eozoön, like Lingula, has maintained its continuity through a long succession of geological epochs. On the other hand, the occurrence of minerals presenting superficial resemblances to true Eozoönal structure, can be of no account to such as really understand the latter.

If the Skye ophite, for example, possesses a true "nummuline layer" in combination with other characteristic Eozoönal features, its presence in a formation of later "geological time than the Laurentian," furnishes no argument whatever against its organic character.

If, on the other hand, the supposed "nummuline layer" in the Skye ophite is nothing but a lamella of chrysotile, the existence of such a pseudomorph can only affect the opinions of such as are incompetent to distinguish the two by those microscopic tests on which experienced observers feel perfect reliance.

Since I do not feel called upon to expend valuable time in giving to Mr. T. Mellard Reade the instruction which he requires to qualify him for discussing this question, I now leave him to the enjoyment of his own opinion. Whenever he shall have shown, by work of his own, his competence to criticise the observations of others who have made a special study of the subject he discussed, I shall be most happy to afford him the same opportunity of forming his judgment as to the organic nature of Eozoon, by an examination of my preparations, that I have given to the many eminent naturalists, who have thus fully satisfied themselves of the justice of my conclusions.

W. B. CARPENTER

Dr. John Hopkinson on "The Overthrow of the Science of Electro-Dynamics"

As I see you have reprinted at length Dr. Hopkinson's paper with the above title, in which he criticises severely, not to say ungenerously, some papers of mine published in the *Quarterly Journal of Science* and *Chemical News*, you will think it only fair to publish my reply, in which I think I shall show that, in the course of his short paper, Dr. Hopkinson has committed mistakes at least as grave and important as any he imputes to me. Let us see if this is not the case.

Dr. Hopkinson quotes one of my articles as follows: "They (that is Joule and Scoresby) calculate the maximum theoretical power of a grain of zinc to be 158 foot-pounds, and yet using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electromagnets never approaching the permanent magnets nearer than $\frac{1}{4}$ of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at haphazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 foot-pounds out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased tenfold." On which he observes, "It is hardly credible, but the above looks very like a confusion between Force and Work! The author seems to assume that if the forces in operation in an engine are greater, that the engine will necessarily produce more work from the same quantity of fuel. In these experiments the quantity of zinc $(a-b)$ used to produce work, W is observed; if the engine was made more powerful, if the permanent magnets were four times as strong, and the electromagnets passed $\frac{1}{8}$ of an inch from them, doubtless W would be greater, but so also would $(a-b)$, and it does not follow that $\frac{W}{(a-b)}$, with which we are concerned, would be at all changed. What becomes, then, of the dogmatic assertion that the duty of a grain of zinc would be increased tenfold?"

Why he should say, "It is hardly credible, but the above looks very like a confusion between Force and Work," I know not. I cannot plead guilty to having made the slightest confusion between the two. I do think the total of the force used is *a measure* of the work produced. But Dr. Hopkinson tries to persuade us that a well-constructed engine would do no more duty than an ill-constructed one, and consequently, I presume, that the magnets might possibly be weakened *ad infinitum*, and removed to ever so great a distance, without necessarily affecting the efficiency of the engine. And then he ventures to criticise my papers as full of fallacies! I retort that it is hardly credible, but that the above looks very like a confusion between $(a-b)$ and b! In these experiments of Joule and Scoresby's the quantity of zinc used to produce work W, is represented by the authors not as $(a-b)$ but as b, and therefore the duty per grain of zinc is not $\frac{W}{a-b}$ but $\frac{W}{b}$; and when the permanent magnets are stronger, and the electromagnets are passed nearer to them, not only does W increase but b also diminishes. So that was I not justified in saying that the duty of a grain of zinc could in a better-constructed engine be probably increased tenfold? And if it be increased only twofold, or even half as much again, then, allowing for waste, I have proved my point, and disproved Joule's mechanical equivalent of heat. Might I not retort fairly on Dr. Hopkinson that the Manchester Literary and Philosophical Society "never ought to have permitted this paper to appear in their Proceedings?"

Next let us take Dr. Hopkinson's next criticism. My argument is this:—That if the doctrine of the mechanical equivalence of heat be, *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, and if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold, not heat, the doctrine of the mechanical equivalent of heat cannot be universally true. To this argument Dr. Hopkinson replies that the facts I quote as paradoxes are simple deductions from the two laws of thermo-dynamics. Quite true; but this only shows that one of the laws of thermo-dynamics is inconsistent with the doctrine of the mechanical equivalence of heat. Might I not retort again on Dr. Hopkinson that "a hostile critic should at least understand the meaning of what he criticises"?

If I said anything which *seemed* to imply that a minimum of work in an engine was inconsistent with a maximum of duty, I freely retract the expression; and I also acknowledge that the argument drawn from the fire-syringe had better have been omitted. But my point was proved abundantly without it.

But still, as the maximum of work done by a battery before it is worn out is only a multiple of the maximum duty of a grain of zinc, I do think it is a *startling* thing, though not mathematically impossible, that this maximum of work should prove to be no work at all.

Perhaps you will allow me to add that I have read Sir W. Thomson's paper read before the British Association in 1852, to which your own reviewer referred me. No doubt you will think it presumptuous in me to say so, but I think that in that paper he has mixed up two totally distinct questions, namely, the cold produced by the decomposition of water into its elements at two electrodes, and the heat produced by the resistance of the film of hydrogen or oxygen or oxide, to the passage of the current. The first is a fixed determinate quantity; the second an accidental one depending on the character of the surface of the electrode, and the ease with which it throws off the film of hydrogen or oxygen. These two points affect the question, as well as the polarisation, and the specific power of retaining or transmitting heat exercised by various electro-motive combinations. M. Favre suggests the formation, sometimes, of peroxide of hydrogen; but this supposition is unnecessary, and, moreover, would not remove the difficulty, for peroxide of hydrogen is so unstable a compound, that it would soon be resolved into oxygen and water, and thus exactly undo the thermal effect of its formation. I trust that in this letter, at least, you will find nothing unfit to be published in a scientific journal.

H. HIGHTON

P.S. I hope to discuss the question *vivâ voce* at the meeting of the Manchester Literary and Philosophical Society next week.

MR. HIGHTON has very effectively shown the uselessness (to him) of my review of his speculations. I wish I could withdraw it, and allow his letter to speak for him without any comments of mine. When a man can make the remarks he has made on Sir W. Thomson's paper of 1854, his case is hopeless—he is incapable of being taught even by the "grand founders" of the science. *Requiescat.*

YOUR REVIEWER

Quinary Music

HAVING hitherto been under the impression that all the varieties of time in music might be regarded as made up of groups of two or three, and never having seen a single piece of music which in any way contradicted this view, Mr. J. Mullen's letter on "Quinary Music" in NATURE for the 9th inst. surprised me very much indeed. It is true that sometimes we meet with passages in which five notes are to be played in the time of four, or seven in six, &c.; but a piece of music in five time is a thing I at least have yet to see, and I should feel obliged to Mr. Mullen it he will kindly tell me the name and composer's name of this novelty.

BEACON LOUGH

The Experimental and Natural Sciences in Trinity College, Dublin

I TRUST you will allow me to make a few remarks upon the article signed "W." which appeared in your last week's number.

In the first place, there are one or two inaccuracies which may be corrected. Thus, your correspondent states that a student "in his third and fourth years *must* devote himself, to a certain extent, to the study of Experimental Physics, including heat, electricity, magnetism, and chemistry, and pass examinations on these subjects, even at the ordinary term examinations." The sophister students are indeed *allowed* by the Board to substitute such a course for classics, but, it may be added, comparatively very few avail themselves of this permission. Again, it is declared that "the chemical and physical laboratories leave nothing to be desired." No such thing as a "physical laboratory" exists in the University; and, seeing that it is only lately that such things have been introduced at the other side of the Channel, it is scarcely likely that we shall have one here for some years to come.

But what I think especially calls for criticism is the general tone of the article, leading the reader, as it appears to me, to suppose that the Experimental and Natural Sciences occupy a high status in Trinity College, Dublin. Now, this is far from being the case; there can be no doubt that they are still generally looked upon in the University as subjects of quite secondary importance.

A.

Dublin, March 14

Science in Schools

I SHOULD be greatly obliged if you would kindly let me know of any school adapted for young boys whose parents wish to give them an education embracing the physical sciences and modern languages, on some such plan as that of the Realschule of Germany.

W.

Dr. Donkin's Natural History of the Diatomaceæ

THE remarks of "K." on my review of the above-named work (*vide* NATURE, vol. iii. p. 348) fall under five heads. Allow me to briefly notice each. 1. There is a difference of opinion between us as to the execution of the plates in Part 1 of Dr. Donkin's work; my copy came direct from the publishing office, it is therefore presumably a fair specimen. I have once more examined the plates, as well as shown them to several competent judges, and the unanimous verdict of all is that expressed in my notice, *i.e.* the execution of the plates in this part is disappointing. I gave instances of apparent inaccuracy in detail in some of the figures. "K." passes these by, but cites figures that are accurate; surely there is nothing contradictory in this. My criticism related for the most part to the *execution* of the plates, and I confess that even comparing them to those illustrating Dr. Donkin's or Mr. O'Meara's papers on Diatoms in some of the late volumes of the *Quart. Journ. Micr. Sci.*, I would prefer the lithographic plates to the engraved ones.

2. "K." agrees with me about the synonymy. I agree with him as to the difficulty of this portion of the subject, but what value would this or any work on species have without synonyms?

3. "K." says the desirability of giving habitats in full is questionable, "three or four localities are sufficient." Unquestionably it is not desirable to give more than three or four localities for common forms, but I think it is equally unquestionable that when Dr. Donkin could have given as many localities for interesting and not extremely common forms, he was wrong not to have done so; and I alluded to the absence of Irish localities in the hope of removing an evident defect in an otherwise useful work.

4. "K." is, without doubt, right in referring the species given by him to Gregory, and not to Ehrenberg. I cannot imagine how so great a blunder on my part originated.

5. "K." asks, "who is Cleeve?" Cleve (not Cleeve), next to Heiberg of Copenhagen, is one of the best northern investigators of the lower Algæ. His monograph of the Swedish species of the Zygnemaceæ is well known, and in addition to his papers on Desmideæ and Oedogonium, he has published "Diatomaceer fran Spetsbergen" (1867) and "Svenska och Norska Diatomaceer" (1868). He naturally follows the arrangement of the "Conspect. Crit. Diatom. Danicarum." It is not without interest to observe how well acquainted these botanists are with the literature in our language relating to the Diatomaceæ.

W.

Lenses for Vision Below Water

IN a communication on the Dioptrics of Vision which appeared in your impression of the 15th December last, I described a form of air-lens for vision beneath the water. Further experience has shown me that the measurements I then gave were not so accurate as they might have been. Thus, the radius of curvature of the glasses in the air-lens to form a lens with a 2 in. focus in water is not $1\frac{1}{4}$ in. as first stated, but 1 in. only.

Again, I somewhat underestimated the magnifying power of the anterior lens of our eye, formed by the aqueous humour, when I set it down as a lens with a focus of 2 inches. $1\frac{1}{2}$ inch is more correct. In accordance with this, I find that for the most perfect vision under water, we require a glass lens of $\frac{3}{8}$ in. focus in air (in place of 1 in. as formerly stated), or an air-lens formed with two segments of a hollow glass globe $1\frac{3}{4}$ in. in diameter, placed concavities outwards. Both these lenses have in water a focus $1\frac{1}{2}$ in. long.

These lenses are for fresh water. Sea water having a greater refractive power than fresh water, requires for perfect vision a somewhat *more convex* glass-lens and a somewhat *less concave* air-lens. I find that an air-lens made with segments of two glass globes of the diameter of 2 inches and $1\frac{3}{4}$ inches respectively, when immersed in sea water forms a lens of $1\frac{1}{2}$ in. focus. But I should observe that good vision under water is obtained by lenses of various magnifying powers, ranging from $1\frac{1}{2}$ to 2 inches focus; but for the distinct vision of small text-type under water, the higher magnifying power is required, and it also is the best for distant vision under water.

53, Montagu Square R. E. DUDGEON

Petrography

THE few English geologists who take an interest in petrography will be thankful to Mr. Geikie for the communications from him which have appeared in NATURE. It is too true, as he observes, that our progress in this branch of geology has for many years been simply *nil*, but there are now manifest signs that this unsatisfactory state of things is drawing to a close. Some of our working geologists are quite aware of the necessity which exists for the application of the microscope to the examination of rocks, and they do not doubt that the result will be proportionately as great as it has been in other branches of inquiry.

It may in the first place be observed that the unsatisfactory nomenclature at present in use is due to the fact that a considerable proportion of the igneous rocks have been named without any precise knowledge of their mineralogical composition, mere chemical analysis being quite inadequate for the solution of the problem; and now there is an evident tendency towards the partial adoption of a chronological classification, founded, no doubt, on the prevailing belief in an essential difference between the "plutonic" and "volcanic" rocks, and on the notion that the age of such rocks may be determined by their mineral constituents, just as sedimentary deposits may be recognised by their included fossils. The microscopical examination of many hundred thin sections of the older melaphyres and more recent basalts establishes the fact that no such essential difference exists, but that, on the contrary, the same minerals are the constituents of both; a difference there undoubtedly is, but the microscope shows that it has been produced by chemical action operating under more or less favourable circumstances during long periods of time. Numerous specimens of the so-called melaphyres from the coal-fields of Scotland and the midland counties are unquestionably composed of the same constituent minerals as the tertiary basalts from the coast of Antrim, Auvergne, and the Rhine.

On comparing the least altered portions of the older rocks with some of the basalts, no difference whatever is observable; triclinic felspar, magnetic oxide of iron, augite, and olivine are the chief ingredients of both. The latter mineral has been regarded as characteristic of the more recent basalts, but, as I have shown elsewhere,* it exists quite as frequently in the melaphyres.

The fact, however, on which it is most important to insist, and which has not hitherto been recognised, is that the difference now existing between old melaphyres and recent basalts is due to chemical action subsequently to the formation of the rocks; not only have most of the amygdaloids been thus formed, and numerous microscopic cavities filled up, but the felspar is frequently much altered, and pseudomorphs of olivine, augite, and hornblende have been formed; thus producing a marked change in the colour and hardness of the rock. Pseudomorphs of olivine are the most abundant, and are of great interest, as crystals in various stages of alteration may frequently be observed.

Prof. Zirkel has shown that in some basalts the various constituents have crystallised firmly together, and lie in actual contact with each other without any intervening cement, while in other cases there is an amorphous glassy substance in which they are embedded. Precisely the same facts may be observed in many melaphyres, and notably so in some of the rocks from the Glasgow coal-field. Many of these rocks from the Scottish carboniferous strata are of great importance in these investigations, as their age has been satisfactorily determined by the valuable labours of Mr. Geikie and his brother.

In revising the nomenclature, it will be for petrologists to decide whether or not two rocks originally of identical composition should receive different names because one has undergone a certain amount of alteration. In most cases there is undoubtedly a difference in their appearance, and it might be convenient to recognise the fact in the nomenclature; but Mr. Geikie's anticipation that "there is such an insensible gradation that no sharp line can be drawn between them" will certainly have to be recognised.

If this method of microscopical analysis is carefully carried out on a sufficiently extensive scale, there can hardly be a doubt that we shall soon acquire a more satisfactory knowledge of all the older rocks; some little has already been done with the melaphyres, and I hope shortly to submit the results to the judgment of those interested in the subject. S. ALLPORT

Tin

THE discussion of the isolation of St. Michael's Mount subject has now branched off into a subsidiary question, which should not pass unnoticed. A writer dwelling on the abundance of tin found in Britain, argues that this natural product of our soil has given a name to our island home. I have met with this suggestion elsewhere, but have never been able to accept it. Our word *tin* is of comparatively modern formation. The Welsh word is *ystaen*, which corresponds so closely with the Latin *stannum* as to lead to the inference that the one form is derived from the other, although we may not be able precisely to say which is the elder of the two.

Now, all things being equal, our modern word *tin* might be accepted as a corruption of either of the above forms; but that it really is more nearly allied to the Teutonic forms of the same word, as found in Saxon, Danish, German, Swedish, &c., all being equally traced to a primitive root preserved in the Sanscrit word *tan*.

It will thus appear almost certain that our word *tin* is of Teutonic origin, and not used in this island so early as the argument for its forming a particle of the word Britain requires. My objection being thus stated, that the word *tin* is a comparatively modern word with us, if of Teutonic origin; yet, on the other hand, if it be assumed as a plausible corruption of the Welsh *ystaen*, or the Latin *stannum*, it appears to me that the primitive words for Britain ought to be found spelt in such variety of form as to lend some countenance to this idea, if it be really founded on fact. But it is not so; consequently we must not indulge the fancy that that useful metal "tin" has any place in the construction of the word Britain. A. H.

PAPERS ON IRON AND STEEL

No. III.—The Bessemer Process*—(*continued*)

RETURNING to the Bessemer flame, we now reach what I have described as its second stage, when its dimensions and brilliancy reach their maxima. We know that carbon must be burning there, and in no small quantity. The average of above three per cent. shown in the analyses, gives in a charge of six tons more than 3·6 cwt. of carbon, requiring for its complete combustion into carbonic acid nearly half a ton of oxygen, or about two tons of atmospheric air. There need be a mighty roar to pour forth all this, and the 14cwt. required for the silicon in the course of about twenty minutes. An interesting problem now presents itself in the whiteness and brilliancy of the flame. It is totally different from the carbonic oxide flame which is produced by the combustion of carbon *per se*. Whence comes this whiteness? Is it due to the combustion of iron in addition to that of carbon, to solid particles of carbon, or is there any important quantity of hydro-carbon present?† The latter explanation is forcibly suggested by the appearance of the flame, and is, I think, to some extent, confirmed by the spectroscope. There is still, however, some red smoke above the flame, which, though less abundant now than in some other stages of the blow, is sufficient to indicate that some iron is burning, probably small particles mechanically ejected into the flame by the force of the blast.

It is during this period of the blow that the lines which have been figured and described by Dr. Watts and Dr. Roscoe as the spectrum of the Bessemer flame are most distinctly displayed. This spectrum includes some of the iron lines, the lines of lithium,‡ sodium, and potassium, and the red band of hydrogen seen as a black band, besides the very complex series of lines which have been designated "the carbon lines" of the Bessemer flame. These lines probably include a hydro-carbon spectrum,—I say "probably," because they do not exactly correspond with the hydro-carbon spectra with which they have already been compared. Nitrogen lines are also displayed, but whether these are due to cyanogen, or to any other compound including carbon and nitrogen, has not yet been determined. What then is the particular compound of carbon which is burning in the Bessemer flame? Is it a hydro carbon, and if so, with which of the many known varieties of hydro-carbon does it correspond? Do the nitrogen lines belong to any compound of nitrogen whose spectrum may be identified?

These are questions of considerable philosophical and practical interest which, I think, the spectroscope may be made to answer. Hitherto, the spectroscopic investigations of the Bessemer flame, though skilfully and laboriously conducted, have been curiously barren of philosophical results, and for practical purposes altogether a failure. If the above questions were answered it would be different.

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* Geol. Mag. vol. vii. p. 159.

* We regret that, owing to the pressure on our space, we have been compelled to keep this article, and the one printed last week, in type for some weeks; similar conclusions have in the meantime been arrived at by other observers.—ED.

† I speak of solid particles of carbon as quite a distinct case from hydro-carbon flame, believing in the soundness of Frankland's conclusion that the brilliancy of a hydro-carbon flame is *not* due to the combustion of solid particles of carbon. My own experiments on the transparency of the *white portion* of common coal gas flames strongly confirm Dr. Frankland's view.

‡ I have only found the lithium occasionally. In many instances I have watched a blow from beginning to end without observing any appearance of the red lithium band which, when seen at all, is so unmistakeably brilliant.

During the first part of the blow a large proportion of the graphitic carbon of the grey iron becomes converted into the condition of "combined carbon" such as exists in steel and white cast-iron. What then is the condition of this carbon if it yields a cyanogen or hydro-carbon spectrum? Nitrogen has been found in considerable quantities in cast-iron which is rich in carbon. Steel makers know that organic compounds containing nitrogen as well as carbon are far more efficacious in cementation than carbon in a state of comparative purity. Thus, bone dust is more effectual for case-hardening than wood charcoal, and ferrocyanide of potassium still more effectual than bone dust. Every steel maker is the proud possessor of a profound secret, a "physic" which he furtively buys from a distant druggist or drysalter, and having disguised its yellow colour by grinding it with lamp black, locks the physic in a strong box and the secret in his own bosom. As this profound secret, like so many others, is perfectly well known to all whom it may concern, I perpetrate no breach of confidence in describing the "physic" as the ferrocyanide of potassium. Its value is unquestionable, but *how* it acts is still a mystery.

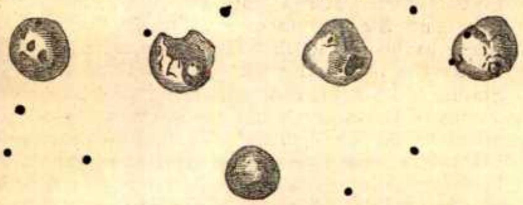


FIG. 1.—"BOMBS" PRODUCED IN THE BESSEMER PROCESS.

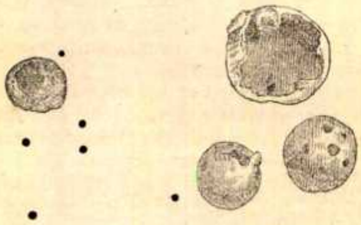


FIG. 2.

Some chemists have maintained that a nitride of iron is essential to the production of steel, and others have hinted at the existence of a cyanuret. As we know that carbon exists in the same condition in white iron as in steel, a question of considerable interest is offered for spectroscopic solution, viz., Is cyanogen burning in the Bessemer flame? Again, we have another set of workshop facts and laboratory experiments which go to show that for the production of steel, hydrogen in the form of hydro-carbon is necessary. It is well known that coal gas, paraffin, and other hydro-carbons, are more efficient cementing agents than pure carbon. Dr. Percy found that the charcoal of sugar, which retained some hydrogen or hydro-carbon, readily converted iron into steel, but that the same charcoal failed to produce steel under similar circumstances, after it had been deprived of its hydro-carbon. It is well known that wood charcoal which has been several times heated in the cementing furnace, loses some of its power of cementation, and this has been attributed to the driving off of the hydro-carbon contained in the fresh charcoal.

Again, it is found that when, by means of an acid, we dissolve the iron of steel or white iron away from its carbon, the residue is not simple solid carbon, but an unmistakeable liquid hydrocarbon, an oil which, like other hydrocarbons, burns with a smoky flame. In this case it is possible that the hydrogen may be supplied to the carbon by the water or the acid. If so it presents an interesting case of the formation of what we usually regard as organic matter from inorganic materials.

If, on the other hand, the hydrocarbon exists ready formed in the steel and the white iron, the conversion of grey iron into white iron, *i.e.* of graphite into this hydrocarbon, is a still more remarkable case of the same kind.

It is true that the hydrogen may be detected by a direct combustion analysis, but this does not reveal the mode of its existence. The information thus afforded is analogous to what we obtain by similar means respecting nitrogen.

The last change that occurs in the flame, that which announces to the foreman the time for stopping the blow, requires but little explanation; but it is, nevertheless, instructive if thoughtfully examined. The contraction of the flame and loss of brilliancy is evidently due to the exhaustion of the carbon. The change which occurs is very similar to that which is observed when air is admitted to a jet from which coal gas is burning.

If to such a jet, supplied with a constant quantity of coal gas, atmospheric air be admitted, so that it shall mix with the gas before burning, the white or luminous portion of the flame will contract in proportion to the quantity of air supplied, and if a gradually increasing quantity of air be admitted, this contraction will progress until the white flame is totally extinguished. Mr. Jonathan Wilkinson, of Grimesthorpe, who has been recently investigating this subject with a view to practical photometric applications, finds that the quantity of air required thus to extinguish the white flame is proportionate to the quantity of carbon combined with the hydrogen, or to the illuminating power of the gas,—that for every standard "candle" of illuminating power about 0·2 of air is required. Thus fifteen-candle gas will require three times its own volume of air at the same temperature and pressure for the extinction of the white flame, seventeen-candle gas 3·4 of air, and so on.

In the case of the Bessemer flame, we have a constant supply of air to a diminishing supply of carbon, and therefore we may expect that there should occur in the white portion of the flame due to hydro-carbon a change corresponding to that which would occur in Mr. Wilkinson's photometric flame, if, instead of a constant supply of coal gas mixed with an increasing supply of air, he maintained the air in constant flow and gradually closed the gas-cock. In this case there would not only occur a gradual diminution of the brilliancy but also of the dimensions of the flame.

Such is the change which takes place in the Bessemer flame towards the end of the blow, and it so far confirms the hypothesis that a *considerable portion* of the white flame is due to hydro-carbon. If it were due to the combustion of iron the white flame should increase towards the end of the blow, for it is then that the iron, when no longer protected by the more combustible carbon, begins to burn in a serious degree, just as I have shown that the full combustion of the carbon takes place after the bulk of the silicon has been oxidised.

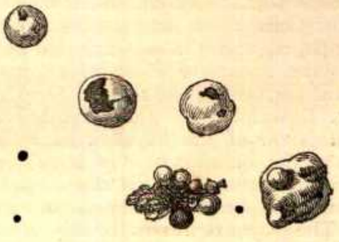


FIG. 3.

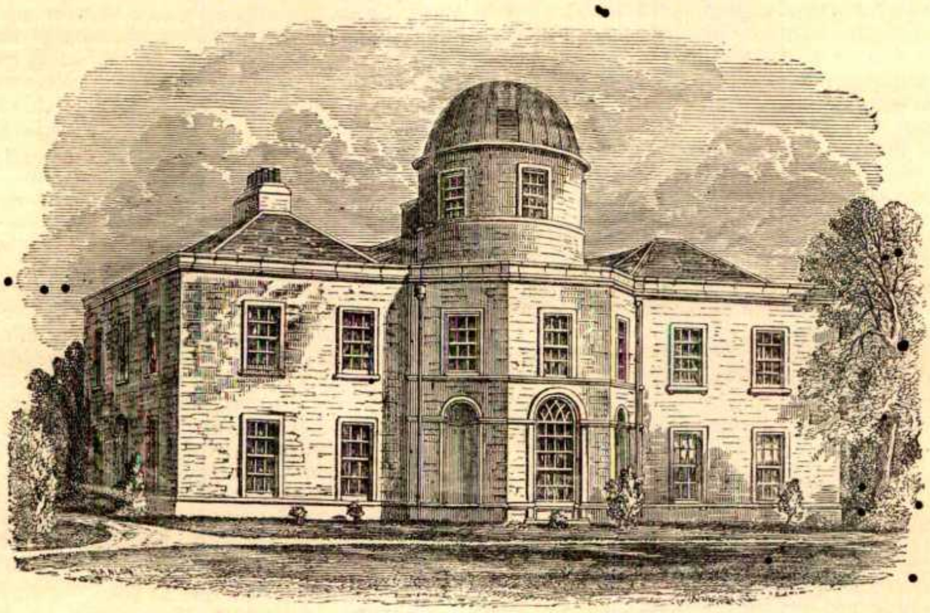
W. MATTIEU WILLIAMS

THE OBSERVATORY OF TRINITY COLLEGE, DUBLIN

IN 1774, Dr. F. Andrews, Provost of Trinity College, Dublin, died, having bequeathed to the College the sum of 3,000*l.* to build and furnish an Observatory, and a further annual sum of 250*l.* as payment of a Professor of Astronomy. Unfortunately, litigation arose, and it was not until about 1781 that it was settled. In 1783 Dr. H. Ussher was appointed Professor; in 1788 the present site of the Observatory was purchased and the buildings erected. Ramsden was commissioned to build the transit instrument, but innumerable delays occurred, and it was not set up until 1808, eighteen years after Professor Ussher's death, and during the professorship of Dr. Brinkley. In 1791 a License of Mortmain, together with a statute for regulating the duties of the Professor, were obtained, and the College expended out of their private funds several thousand pounds, so as to carry out the intentions of their late Provost. The competition for the professorship is open to all the astronomers of Europe. Since its foundation four have been appointed: Dr. Ussher, in 1783; Dr. J. Brinkley, of Cambridge, in 1790; Sir William Hamilton, in 1827; and Dr. Francis Brünnow, in 1865. The Professor of Astronomy is also Astronomer Royal for Ireland. Since 1831 the stipend granted to the Professor is 700*l.* per annum, which includes the pay of an assistant and gardener.

The Observatory is situated about five miles from the College, at Dunsink, to the north-west of Dublin, beyond the Phœnix Park. It lies amid extensive fields and pasture lands on the summit of a hill nearly three hundred feet, close to the sea level. The accompanying woodcut represents the principal front of the Observatory. By the Statute of 1791 it is enacted, among other things, that a fair copy of the observations made every year shall be presented to the College, and also that the observations shall be printed every year at the expense of the College, the Professor to supervise the press, and copies to be sent as presents to the principal observatories, academies, libraries, and eminently-learned persons both at home and abroad; the remaining copies to be sold, and the profit given to the Professor for his care and trouble in supervising the printing of the observations.

We welcome with great pleasure the first part of a series of "Astronomical Observations and Researches made at Dunsink" by the present Astronomer Royal for Ireland, which has been printed and circulated within the last few weeks by the College. It consists of a quarto part of eighty-eight pages, and it is accompanied by three plates. It embraces the results of observations made with the "south" refractor, from June 1868 to October 1869, and contains—1. Description of the "South" Refractor. 2. New determination of the Parallax of α Lyræ. 3. Determination of the Parallax of 61 σ Draconis. 4. Micrometrical measurements of double stars. 5. Observations of α Lyræ and companion; and 6, Observations on 61 σ Draconis. The plates represent the new dome erected for the "South" Refractor, a sketch of the wheels on which the dome revolves, and the interior of the dome and sketch of the "South" Refractor.



THE OBSERVATORY OF TRINITY COLLEGE, DUBLIN

The object glass of this Refractor was given in 1863 to Trinity College, by Sir James South. It was given with a valuable collection of astronomical instruments on the accession of the Right Hon. the Earl of Rosse as Chancellor of the University. This glass is 11¾ inches diameter, and was purchased by Sir James South forty years ago from Cauchoix in Paris. The good qualities of the glass had already been fully appreciated by Arago, who recommended the French Government to purchase it for the Paris Observatory. Shortly after the appointment of Professor Brünnow, the Board made arrangements for procuring a mounting, and for erecting a suitable dome. The wall of the dome is of stone, neatly panelled on the inside with wood; the dome is of timber covered with copper; the diameter of the building is 27 feet. The machinery for moving the dome is very ingenious, and was designed and executed by Mr. Grubb. The dome can be started by applying a force of six pounds, while five pounds suffice to keep it in motion; its opening is 2½ feet wide, and extends from the horizon to a little beyond the zenith. The support, or the mounting, is solid pier-built on the limestone rock. The equatorial mounting was made by Mr. Grubb. The motion of the clock is very steady, keeping a star steadily on the wire. The Filar micrometer was made by Pistor and Martins, of Berlin.

The present series of observations show what work can be done with this excellent instrument in the hands of the present Professor of Astronomy, while they also reflect credit on the Board of Trinity College for their large but wise expenditure.

W.

THE SUPPOSED FUNGOID ORIGIN OF CHOLERA *

THIS report contains a first instalment of the scientific portion of the Cholera inquiry now proceeding in India, the instructions for which were drawn up by the Army Sanitary Commission. It is limited to facts bearing on the Cholera theories of Hallier and Pettenkoffer. In the pursuit of these facts Dr. Lewis has been able to clear the ground for future progress, while at the same time he has added considerably to our knowledge on some obscure points of microscopical science. The report is fully illustrated with engravings of microscopic slides, executed with remarkable beauty and correctness in the office of the Surveyor General of India. Indeed whether we look at the engravings or the typography, we cannot help feeling that our own artists have something to learn from their brethren in India in these matters.

The subjects of examination are divided by Dr. Lewis into three classes, *Cysts*, *Spores*, and *Micrococcus*, the three elements of Hallier's theory; and the examinations have been conducted partly by direct observations of immediate choleraic discharges, in comparison with observations on other media, partly by the use of reagents, time and temperature, and partly by cultivation experiments. In this way, although the microscopic pathology of cholera has still to be inquired into, Dr. Lewis has been enabled to give a satisfactory account of a number of objects which have formed the bases of preceding theories. He has shown what these objects *are not*, and he has shown what many of them *are;* and this he has done with so much scientific caution that we cannot help feeling that this most important inquiry has fallen into proper hands.

In the year 1866 Hallier discovered in cholera discharges, yellowish-coloured cysts of spherical or oval form, enclosing yellowish shining spores varying in size, also groups of swollen spores surrounded by minute molecular matter (so-called *micrococcus*) proceeding apparently from the rupture or breaking up of spores.

These minute molecules were seen to adhere to various objects in the fluid, on which they appeared to feed; they exhibited signs of germination, groupings, filamentary arrangements, and, finally, branching filaments with *macroconidia* and *cysts*, the relations of which to each other were considered as established by cultivation experiments. The resulting fungus, a *polycystus*, was considered by Hallier to resemble the rye fungus in Europe, and probably to be present in diseased rice in India; and he held that this fungus introduced into the intestinal canal and there passing through the various stages of its existence, caused the phenomena of cholera by its action on the intestinal epithelium.

This brief sketch is sufficient to show how much solid fact was necessary to fill up the slight frame-work of Hallier's hypothesis. Here Dr. Lewis's work begins, and every step in it is illustrated by engraved slides.

We have first an examination into the nature of the so-called "cholera cells" discovered by Drs. Swayne, Brittan, and Budd, in 1849. These objects appear to have been of various kinds; some certainly not of fungoid origin. Selecting the most marked of the objects of which engravings are given, Dr. Lewis shows that objects, as nearly as possible similar to those figured by the Bristol observers, are found in discharges in India, and that they are ova of *acari* and of intestinal worms (*trichocephalus*).

As regards Hallier's *cysts* Dr. Lewis states that he has never met with any in fresh cholera discharges, but that he had repeatedly developed them. The other cyst-like bodies proved to be either fragments of tissues or ova, none being peculiar to cholera. Cultivation experiments with cholera-discharge containing cyst-like bodies yielded branching fungi with *macroconidia*, which gave place to *aspergillus;* in other cultivations, however, the only products were *penicillium* and *aspergillus*. Dr. Lewis admits that cysts distinctly resembling those of Hallier may be developed by cultivation from cholera discharges, but that he had found them only three times in more than a hundred cultivations.

Their development is therefore not a constant phenomenon, and Dr. Lewis further shows that cysts of the same character can be developed in discharges not choleraic. Bodies resembling "*spores*" are very common in cholera discharges, and Dr. Lewis bestows much pains in demonstrating their true nature. He illustrates every step of the inquiry by slides, and classifies the bodies under the four following heads:—(1) Globules, of a fatty nature; (2) altered blood cells; (3) corpuscles, embedded in a tenacious substance; (4) globular condition of certain infusoria. The corpuscles in Class 3 are amœboid in character, and are probably due to effused blood plasma. There is no evidence of the presence of spores of fungi.

The last subject inquired into was the so-called micrococcus, the supposed "germ" of cholera, which in Hallier's view might pass into the human body in water or air, and then give rise to cholera by developing itself at the expense of nitrogenous material, especially intestinal epithelium.

Dr. Lewis shows that minutely divided matter is not more prevalent in choleraic than in other discharges, indeed less so, but that attempts to produce "*micrococcus*" by cultivation had entirely failed, possibly on account of the many sources of fallacy in such experiments.

He gives the results of a number of observations made with infusions and decoctions of animal matter, including cultivations with cholera discharge, and shows that in spite of every care in the manipulations, very different forms of life will make their appearance in substances derived from the same source, and under apparently identical conditions. His general conclusions on this first stage of the inquiry are:—

1. That no cysts exist in choleraic discharges which are not found under other conditions. 2. That cysts or "sporangia" of fungi are very rarely found under any circumstances in alvine discharges. 3. That no special fungus has been developed in cholera discharges, the fungus described by Hallier being certainly not confined to such. 4. That there are no animalcular developments, either as to nature or proportionate amount peculiar to cholera, and that the same organisms may be developed in nitrogenous material even outside the body. Lastly, that the supposed *debris* of intestinal epithelium is not of this origin, but appears to result from effused blood plasma.

Unless these conclusions are materially modified on subsequent inquiry, they must be considered as disposing of Hallier's theory of cholera. Should, however, Dr. Lewis's further investigations prove that Hallier's fungus is present in choleraic discharges and in diseased rice as a *constant*, we should still require scientific proof that cholera was caused by the action of this fungus and by nothing else.

Pettenkoffer's theory of cholera connects the prevalence of the disease with certain conditions of damp subsoil and subsoil water besides the presence of a "germ," favourable meteorological conditions and personal predisposition. Little has been done as yet in this portion of the cholera inquiry. What has been done is very interesting, although it does not support the theory. Observations regarding it have been made at Allahabad, Cawnpore, Lucknow, Fyzabad, Agra, Morar, Meerut, and Peshawur. The subsoil water experiments do not appear to sustain Pettenkoffer's views, but the examination of soils has yielded several important scientific facts of general interest. The amount of air in specimens of soil taken at different stations varies from 33 to 66 per cent.

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* A Report on the Microscopic Objects found in Cholera Evacuations, &c. By T. R. Lewis, M.B., Assistant Surgeon H M. British Forces.

by measure. The amount of organic matter in soils, when compared with the amount, weight for weight in the water at the same stations is from 10 to 20 times greater; one instance is given in which it was 40 times greater. But the most interesting scientific facts are those connected with the development of lower forms of life in infusions of soils in water. Besides a few *algæ*, the prevailing forms are—*Monas*, *lens*, *Paramecium*, *Monera* assuming the most fantastic outlines, *Vibriones*, *Amœba*, *Englenæ*, &c.

We look forward with great interest to further instalments of this important inquiry, which we trust may add largely to our knowledge, and by this means enable human life to be saved.

FOSSIL CETACEA

DURING the recent extensions of the fortifications of Antwerp, which have occupied some years, very fine opportunities have been offered, as is well known to geologists, for studying the Crag formations (Diestien and Scaldisien systems) of Antwerp, of which we have remnants on our own east coast. It is not, however, so well known that the Belgian Government during the excavations used every care to preserve the remains of Cetacea and other marine mammalia thus disinterred. The workmen were instructed to give up all such remains for the Government, and were not allowed to sell them. Parts of the Black and Grey Crags proved to be a complete charnel house—so abundant were the remains—and these have been quietly brought together and placed under lock and key for the last eight years. The richness of the fauna disinterred may be judged from the fact that it is stated that eight new genera of Ziphioid Cetaceans are indicated besides sixteen new species belonging to known genera. Many of the forms are represented by far more complete portions of the skull than have hitherto been known from these beds, also portions of the trunk, limbs, and lower jaw in connection with these. Portions of the skull of the fossil Walrus, tusks of which occur in the Suffolk bone-bed and have been described as Trichecodon, have been obtained, as well as remains of seals. All these specimens are under study by the Vicomte du Bus, and are not open to the inspection of even professional palæontologists. They are being carefully and freely engraved, and will soon, it may be hoped, be made known to the world.

PARASITES

PROF. VON BENEDEN, as we have before noticed, has distinguished true parasites, which live *on* their host, from commensals, those which live merely *with* their host, the thieving impostor from the respectable lodger. In an admirable work on the "Fishes of the coasts of Belgium, their Commensals and Parasites," published by the Academy of Sciences of Brussels, he now further classifies parasitic organisms. The commensals are either 1, *Oikosites*, fixed; or 2, *Coinosites*, free. The Oikosites fish for their own living, and merely ask a free passage from their hosts. They are either fixed in perpetuity, as *Coronula*, *Cochliolepis*, *Modiolaria*, *Mnestra*, and *Loxosoma*, temporarily as the *Remora*, *Anilocra*, *Praniza*, or only in the young state, *e.g.*, *Caligus* and *Anodon*. The Coinosites, on the other hand, never give up their liberty; they occasionally leave their host, and between Coinosite and host there is often an exchange of good-offices, one furnishing a solid house or a strong claw, the other a sharp eye, and they may share their prey in common. The digestive canal is occupied by the following Coinosites:—*Fierasper*, *Stegophyle*, *Stylifer*, *Phronimus*, *Hyperia*, the mantle by *Pinnotherus* and *Pagurus*, the exterior by *Myzostoma*, *Cyamus*, *Pycnogonon Caprella*, and *Chætogaster*.

The true parasites cannot live without assistance, they are divisible into several categories. Some, such as the leech, fleas, and some dipterous insects, suck the blood of their victim, and then quit him to take their after-dinner nap in the open air; others, such as the ichneumon flies, do not quit their host till they have become adult, and have in the process exhausted the last drop of blood of their unfortunate prey. The greater number lead a free life when young, and merely attach themselves to a host at the time of reproduction, such are the Bopyrian and Lernæan Crustacea. There is a further very interesting group, who enter a host while yet young, simply in order that they may get carried by its means into a second host, where they will ripen their eggs. Often whilst waiting in their first host (sometimes vainly waiting no doubt) for him to be devoured by their second and ultimate victim they reproduce *agamically*. Such parasites are the Flukes and many Tapeworms. These divisions are thus tabularly set forth:—

Parasites free			
during all their life.	during a part of their life they pass through		
	a single host.		several hosts whilst immature.
Leeches Fleas and Flies. *Caliga.*	whilst immature	when mature.	Distomata. Cestoids.
	Ichneumons. *Mermis.*	Bopyrians. Lernæans.	

The parasites of the first category which are free during all their life, Professor Van Beneden calls Phagosites, and compares them to the *habitués* of a hotel who avail themselves of the *table d'hôte*, but do not have a bedroom in the building. The other parasites which have both board and lodging are divisible into three principal categories. 1st. Xenosites—who are pilgrims in transit—voyaging with a distinct but distant object in view. They are always agamic, lodge in such closed organs as the brain, muscles, and serous membranes, and wait patiently till they get into the stomach of the animal where they are destined to breed. The stomachs and appendages of fishes swarm with parasites, and those which have the largest *clientèle* are by no means the least healthy or thinnest. Often one fish, having swallowed another, is swallowed by a third, and thus Xenosites find themselves set free in the wrong fish's stomach, for the stomach acts like a filter, straining out and retaining the parasites, while the flesh is digested. Such erring Xenosites merely wait, and may often pass through several "hotels" before they reach their destination. 2nd. Nostosites—those who have reached their destination, and now can abandon themselves to generation. Whilst the Xenosite was obliged to put up often with an uncomfortable cramped lodging, biding his time, the Nostosite occupies the most eligible organs for parasitism—in fact, the most vast and commodious chambers of the hotel. The 3rd division are the Pilgrims, who have lost their way hopelessly, and are in worse plight than even in Giant Despair's castle. Such are the agamic worms which are found often in the Plagiostomous fishes, and who ought to have got into some Teleostean fish, there to fructify—a happy fate for ever lost to them when by unlucky chance the host in whom they trusted was swallowed by a remorseless shark. They never quit this retreat.

Professor Van Beneden gives directions for searching an animal for its parasites, and justly claims a high interest for the study of the fauna of individual species, and urges such neat and sharply-limited zoological inquiries on those who do not feel prepared to study the fauna of a geographical region—to the philosophy of which, indeed, the study of parasite-faunæ may furnish important suggestions. Ninety-three species of fish, with their parasites and commensals, are cited in this work from the author's own observation. Eight plates illustrate it.

E. R. L.

NOTES

IN the second report of the Royal Sanitary Commission, just published, the Commissioners appear to be under the impression that no branch of science other than that of medicine is to any great extent involved in sanitary questions, and therefore recommend that the 4,000 medical men appointed under the Poor-law Board should be the inspectors under the proposed new sanitary department; mentioning, however, the probability of a variety of officers being requisite for scientific purposes only. We think that, in order to carry out sanitary reform efficiently, the new department should have the means of consulting the highest authorities in most of the branches of physical science, and here, as in many other cases, we see the necessity for a Board of Science, whose duty it should be to advise the Government on all scientific questions.

WE regret to state that the work at the new buildings at Burlington House for the learned societies has come to a standstill, owing, we are informed, to the failure of the contractors.

OUR Paris correspondent reports the death, at the age of 80, of M. Becquerel, the celebrated electrician. He died in Normandy during the siege of Paris.

THE series of afternoon scientific lectures to be delivered in the Lecture Theatre of the Royal Dublin Society is as follows:—March 25—G. J. Stoney, A.M., on the Sun. April 1—Professor Traquair, M.D., on the Vertebrates of the Coal Period. April 8—Dr. C. Cameron, on the Source of Muscular Force. April 15—Professor J. R. Greene, on the Phenomena of Sleep and Dreams. April 22—Professor W. T. Dyer, on Recent Additions to our Knowledge of Fossil Plants. April 29—Professor T. Andrews, F.R.S., on the Continuity of Liquid and Gaseous States of Matter. May 6—Professor P. Redfern, M.D., Illustrations of the Advance of Physiology. May 13, Dr. J. E. Reynolds, on the Chemistry of Milk, and a new mode of testing its quality.

WE are glad to hear that the Hackney Scientific Association is in an active and prosperous condition. In another column we print a very short abstract of a paper read at a recent meeting on the fossil remains of mammalia found in the Lea Valley.

THE *Moniteur Scientifique*, edited by Dr. Quesneville, is the only scientific paper which continued to be published in Paris regularly throughout the siege. Since the 1st of October its circulation has been necessarily confined to its subscribers in Paris; but we have now received a parcel of ten of its fortnightly numbers, and heartily commend it to the notice of men of science in England.

THE following is the substance of a communication on the periodicity and heliographic distribution of sun-spots, addressed by M. Zollner to the *Astronomische Nachrichten* of March 2nd:—The sun-spots are slaglike by the radiation of heat on the glowing and liquid surface of the sun; the products of the cooling having again dissolved, in consequence of the disturbance of equilibrium produced by themselves in the atmosphere. When these disturbances are not only local, but generally distributed, the formation of new spots is but little favoured at the times of such general motion of the atmosphere, because then the most essential conditions of the surface are wanting for a severe depression of temperature by radiation, namely, the rest and clearness of the atmosphere. But when the surface has again gradually become quiet after the dissolution of the spots, the process again recommences, and acquires in this manner a *periodic* character, in consequence of the mean relationships of the surface of the sun, which may be considered as attaining an average in long periods. The distribution of the spots in area must, according to this theory, be determined by the zones of greatest atmospheric clearness, which, as has been shown, generally coincide with the zones of the greatest abundance of spots.

THE following are our American notes for the week, for which we are again indebted to *Harper's Weekly*:—The eighty-fifth number of the Proceedings of the American Philosophical Society, lately published, and completing the eleventh volume, is, like many of its predecessors, nearly filled with important communications from Professor Cope, whose industry in publishing accounts of new, recent, and extinct zoological forms is untiring. One of the most important of these communications is an article upon certain fresh-water tertiary fishes from Idaho, collected by Mr. Clarence King, and embracing twelve species of six genera. These all belong to the Cyprinidæ, with the exception of one species of the trout family. With these fish were three species of *Astacus*, also described by Professor Cope in another communication.—We have already referred to one of the papers of Professor Cope, in which he describes a new species of mosasauroid, called *Liodon dyspelor*, based upon specimens from New Mexico in the museum of the Smithsonian Institution, and which, according to Professor Cope, probably exceeded one hundred feet in length, and may be considered as the longest reptile of which we have any account.—We have frequently called attention to the interest and value of the dredging operations conducted by Count Pourtales in behalf of the coast survey, in the deep seas adjoining the southern coast of the United States. The results of these labours are being published by the Museum of Comparative Zoology at Cambridge, Massachusetts; and there has just appeared an elaborate memoir upon the crustaceans by Dr. William Stimpson, of Chicago. A large number of new genera and species were detected in the collection, and a portion of these are enumerated in the report referred to, which embraces only the Brachyura, the remaining families being reserved for a future memoir.—The Commissioners of Fisheries for the State of New York have lately announced in the public papers their readiness to furnish, free of expense, living black bass, cat-fish, white bass, rock bass, roach, perch, sunfish, and pike-perch, for stocking the waters in any part of the State of New York, provided parties desiring them will send an agent to receive and take charge of them. All of these are now bred at the State establishment at Caledonia, and applications for them are to be made to Seth Green, Rochester.—One result of the completion of the Pacific Railroad has been the introduction into Eastern markets of Western game. We see it stated that two hundred antelope were sent to Boston during a single week, and three hundred saddles of deer, of both the white-tailed and black-tailed species. The antelope brought from fifteen to twenty cents per pound wholesale, and the venison from twenty to twenty-five.

THE first annual report of the Association for the Improvement of Geometrical Teaching has just been issued. Gentlemen who may desire to have a copy of the same, or to receive information on matters connected with the Association, will perhaps be glad to know the names of the local London secretaries, who are Mr. C. W. Merrifield, F.R.S., Royal School of Naval Architecture, South Kensington, and Mr. R. Tucker, M.A., University College School, W.C.

THE Proceedings of the Royal Asiatic Society of Bengal for January contains a drawing of a remarkable case of polydactylism in a horse from Bagdad. Mr. Wood-Mason, who exhibited the specimen, remarked that the splint-like rudiments of the metacarpals of the fourth toe on each fore foot had given rise to a supernumerary digit provided with the regular number of phalanges, and encased in an asymmetrical hoof, the asymmetry of which was such that the presence of another of the same shape internally to it would have formed a symmetrical pair like the cleft hoof of a ruminant. The metatarsals of the fourth toe on each hind foot were, by the law of correlation, similarly affected; but the supernumerary hoofs of these were stouter and more irregular in shape. The monstrosity appeared to present an interesting reversion in the direction of the extinct and fossil *Hipparion*.

One of the "A B C Despatch-boxes," patented by Jenner and Knewstub, has been forwarded to us. We hail it as a most useful invention for all who have papers to keep in order—a condition which largely obtains in the case of scientific men, and we commend it accordingly.

On the night of the 26th January some severe shocks of earthquake were felt at Accra on the West Coast of Africa. As three series had been felt in five months there was considerable alarm.

Mr. Hyde Clark will bring before the Anthropological Institute on Monday the result of his researches on the ancient history of civilisation, and the development of comparative mythology in Western Asia and Europe, previous to the Aryan period.

Coal has been found at Sarawak in a district easy of access, and where native labour is easily obtainable.

Indian papers, in reporting an earthquake shock in Assam on January 27th, give two native theories of the causes and origin of earthquakes. The one is that when the world becomes sinful, a kind of large serpent, on which the world rests, turns on its side, and so causes them. The other is, that earthquakes are caused by periodical leaps of the mountain gods from one mountain to another.

The Folkestone Natural History Society has issued its third annual report, from which we gather that its progress is still satisfactory. The number of members now reaches 150, showing an increase of thirty-two during the past year. The attendance both on field days and at the *conversazioni* has been, on the whole, very good. The expenses attendant on the reorganisation of the museum have been somewhat heavy, and an appeal is made for the formation of a museum fund, which it is hoped will meet with a suitable response. The report contains a selection from the papers read during the year, those published being: "On Primroses and their Fertilisation," by the secretary, Mr. Ullyett; "On Arctic Botany," by the president, Dr. Fitzgerald, who accompanied Lord Dufferin on his voyage to the North Pole some few years since; "On the Special Characteristics of Seaside Plants;" "On the Yeast Plant;" and "Local Botany." We are somewhat disappointed that more prominence is not given to local natural history; but, with this exception, the report is very creditable, although a little more attention might have been advantageously bestowed upon the printing of the scientific names. A list of the books given and lent to the library concludes the report.

Dr. Fayrer, in India, has been experimenting to correct the popular error that a snake cannot kill a snake. He took a young and very lively cobra fourteen inches long, and which was bitten in the muscular part of the body by a krait forty-eight inches long. The krait had not bitten for some days before. From a detailed report by Dr. Fayrer, it appears that the cobra was bitten at 12.50 P.M., at 1 P.M. it was very sluggish, at 1.3 P.M. so sluggish that it moved with difficulty, could be easily handled, and made no effort at resistance. At 1.20 it was apparently dying and its movements were scarcely perceptible, and at 1.22 it died, thirty-two minutes after the attack. Dr. Fayrer has found that the water-snakes of India are deadly poisonous. In the Bay of Bengal they swarm, and it is noted as ominous that lately it was proposed to erect a sea bathing establishment for Calcutta at Barwar, under the assurance there were no sharks. It is remarked that sharks need not be noticed when a bather may have deadly water-snakes swimming after him.

The *Correspondenzblatt* of the Naturalists' Society of Riga, 1870, contains an abstract of a paper by Herr Teich, "On the influence of climate on the size, colour, form, and number of species of butterflies:"—An account of a discovery by Prof. Nauch that thin glass tubes when they have a ball blown at their ends give out a distinctly audible and clear note on cooling so long as the relation of the size of the ball to the length of the tube does not pass a certain limit. The sound is ascribed to vibrations set up by the inrush of air consequent upon cooling. Some remarks by the same professor on a lightning tube found at Ilgezeem:—A long paper on cell life by C. A. Hengel:—A communication from Herr Thieme to the effect that he has found that *Dracæna paniculata* kills flies, particularly when the plant stands several feet from the window. The dead flies hang to the under side of the leaf:—A paper by A. Noschel on the Trevelyan instrument, the sound produced by which he considers due to friction between the two metals—the one contracting, whilst the other expands, the vibratory movement being considered as secondary. The author calls attention to the fact that when the instrument has been in action for some time a bright spot is formed on the metal at the point of contact, which he considers as evidence in favour of his theory. Baron Horyingen Huene gives an account of a plan of sugaring for insects with apples cut in three soaked in solution of honey for a day, then strung on string, and suspended between adjacent trees, which he has found very successful. The journal contains many other communications, but without much original matter in them.

A report comes from Bangalore in the Madras presidency, that coal and paying gold have been found.

An Australian is said to have raised a sum of money by a false representation of the existence of coal at Midnapore in Bengal.

A slight shock of earthquake was felt at Guyaquil in Ecuador on the 9th January. The movement was from the interior towards the coast.

An eruption of the Colorucco volcano in Mexico in January has done much damage to plantations and villages. An eruption of Mount Orizaba is expected.

On the night of the 31st January an earthquake was felt at Bombay, which extended over a large tract of country. It is stated that on New Year's Day Northern Guzerat had a like visitation.

Besides the objects brought from the Guano Islands of Guanape, on the coast of Peru, by Mr. Josiah Harris, and exhibited at the Ethnological Society last year, we have now the report of a large find. The most interesting objects are rude representations of the human figure, cut in very hard wood. On the north island, beneath forty feet of guano, a cavity was come upon, which, on the removal of the guano, was found to be a cave, leading downwards further forty feet. This was a kind of Pompeii, but blocked with bird dung instead of volcanic ashes. It had been evidently frequented by man, and contained many handwrought works, and also well-preserved seafowl and other birds, lizards' eggs, but all petrified, as it were, in the guano. In many cases the colour of the eggs is preserved. The cracks and fissures in the walls of the cave were found filled with solidified ammoniacal salt. Two pieces of earthenware vases were found, bearing figures, also two gold earrings, and a bundle of medicinal herbs tied up in woven cloth. Local antiquaries consider the objects as far older than the time of the Spanish conquest. The point of interest is the accumulation of guano above the surface.

WE learn from the *British Medical Journal* that Dr. Crace Calvert, of Manchester, having been requested to carbolise a quantity of charpie for the use of the ambulances at the seat of war, found that charpie was unsuitable for the purpose; and after trying several textures, finally hit upon oakum as the most excellent. The oakum is first soaked in Burgundy pitch, and then rendered antiseptic by the addition of carbolic acid. This application has been a good deal used at the Manchester Infirmary, and with good results.

A NEW undertaking of interest to the philosopher is the Arequipa Railway in Peru just opened for traffic. It is a great engineering work, carried out with English capital by American enterprise, and it penetrates the western chain of the Cordillera of the Andes to reach the table lands of the interior, Arequipa, the terminus, being 7,800 feet above the level of the sea. Now at this elevation the rarefaction of the air is such that the ordinary workmen could not be employed, the suffering being in some cases intense. The works were, however, pushed on with vigour, and Mr. Meiggs imported above 16,000 labourers for his works, and for this purpose chiefly Aymara Indians from Bolivia. Mr. David Forbes, F.R.S., has, in his memoirs on the Aymaras in the Journal of the Ethnological Society, described the abnormal structure of the chests of these people, and it is astonishing to see them employed in a task which most effectually insures their subjugation. To foreign troops it was always difficult to scale these regions, but now the railway does the work, though to soldiers and passengers the journey is not always without discomfort.

THE Bunya-Bunya (*Araucaria Bidwilli*), a native of the northern district of New South Wales, is of considerable interest, as being the only hereditary personal property possessed by the natives, who greedily devour the fruit, either raw, or roasted and made into cakes. This fruit is only plentiful every third year; and at the proper season the aborigines assemble in considerable numbers for the purpose of obtaining it. Each tribe has its own set of trees, and each family its particular individuals among them; and these are handed down from generation to generation. The right of ownership is almost universally respected; but occasional depredations occur, when a fight ensues, the sympathies of the bystanders going with the lawful proprietor.

WE learn from the *Grocer* that experiments recently carried on in India have proved that coffee pulp will yield, upon distillation, 9 per cent. of its own weight of spirit, equal in strength to Scotch whisky. Nothing is said as to the flavour of this spirit in its raw state, but it appears to realise on the spot a price nearly equivalent to 4*s*. 6*d*. per gallon.

AT the end of January, no date named, a shock of earthquake was felt in the Sanjak of Kartal, in Northern Asia Minor, which lasted several seconds and did slight damage.

A LARGE and valuable deposit of limestone has been discovered by Mr. Read in the Sonthal Pergunnas, in Bengal, in the Banslo River. There is good communication with Calcutta by water or railway.

SOME fair pearls have been brought down to Durban, in Natal, from the River Vaal. They were found in mussels.

NATURAL history and dancing and the Police. Such is our announcement from Madras. In consequence of a fatal case, the Commissioner of Police has ordered that the dancing girls shall not dance in the Hindu temples with cobra snakes thrown round their necks. This will cause great disappointment to the pious votary and the interested amateur. It will tend, however, to lessen the reverence for the cobra, and may bring his tribe into greater danger of repression.

IMAGINATION IN SCIENCE

PROFESSOR Tyndall will eventually have much to answer for. He has lent his authority to the admission of imagination in the pursuit of science, and there is every prospect that people whose imaginative faculty is stronger than their habit of observation will give us all plenty to do. We shall not only have to question nature, but we shall have to eliminate imagination, and thus have two battles to fight for truth. Our medical friends have not always walked in the ways of rigid observation and induction, but if any one desires to see how easy it is for the imaginative faculty alone to tell us all we require to know, we commend to his perusal the *Mobile Daily Register*, of Dec. 18th, 1870, in which there is a communication from Dr. Cochrane on the subject of yellow fever, well written, and interesting, and giving what may be called an account of yellow fever from the imaginative side. The author justifies his position by the example of European names, tells us candidly that he states only "what he believes but does not know," and then takes his flight into the unknown. He imagines "the yellow fever poison to be composed of living germs in innumerable number, living organisms of inconceivable minuteness, which eat, and drink, and multiply their generations under the sun, just as other living creatures do with which we happen to be familiar." He connects his speculations in these matters with similar speculations about "contagia" and disease "germs" which are well known on this side of the Atlantic, and without paying any attention to facts regarding yellow fever and other diseases which are left untouched by any extant doctrine, he tells us truly that "the visions of modern science are more wonderful than the visions of Eastern fable." This may be true, and the visions themselves may be true; but, for people who feel that they must walk over the earth in search of truth, nutriment of this kind is by no means sufficient for mental sustenance.

We have no desire to undervalue the importance of the imaginative faculty in scientific pursuits; but papers such as the one before us raise some very important primary questions. Are we to live, scientifically, in the same way as alchemists and astrologers did in the Middle Ages? and are we to ignore all that Bacon and Newton have done for us? If it be true that there is no royal road to knowledge on the firm earth, it is certain there is no such road through the air. Let us use the imaginative faculty by all means; but, in doing so, let us take our stand on the firm ground of the known before we venture ourselves into the unknown.

THE ROYAL SOCIETY'S SOIRÉE

WE are indebted for the following account of the most interesting objects exhibited at the Royal Society's *soirée* on Saturday evening last to the *Standard*, from which paper it is abridged.

In the foremost ranks of notable attractions were the heliotype process of printing photographic plates for plates for book illustration, by Messrs. Edwards and Kidd; the solar eclipse photographs, and the twelve-inch equatorial telescope, with its photographic feed apparatus (Mr. Browning's), by which they were obtained by Lord Lindsay; the musical vibration figures shown in Mr. Spottiswoode's new apparatus; the electrical experiments of Mr. Varley; Commander Harvey's sea torpedo (made by Vavasseur); Dr. Norris's soap-bubble experiments; Mr. Haviland's fine maps of the geographical distribution of cancer and heart disease (very recently published by Mr. Keith Johnston); the gold-hardening process, by Mr. Roberts of the Mint; and Mr. Francis Galton's pantagraph and resultant plates for the publications of the Meteorological Office.

The soap-bubble experiments, performed with great adeptness by Dr. Norris, were intended to illustrate the physical principles concerned in the formation of rouleaux in the blood and in the passage of the corpuscles *de toute pièce* through the walls of the minute blood-vessels, without rupture of the latter, as observed by Waller in 1846 and Cohnheim in 1867. A film of soap solution was taken by a metal ring of a foot or more in diameter, and upon it a soap-bubble blown from a pipe was thrown; the bubble was caught by the film, and held suspended midway or along the equator of the thin hollow sphere. The bubbles were then forced through and drawn through without rupture of the films. An orange was dropped, and glass rods and other solid objects, with wetted surfaces, were passed in like manner without rupture of the films.

The three experiments by Mr. Cromwell F. Varley were exhibited for the first time in public. Two of them were in illustration of some investigations into the nature of electric discharges through gaseous media, described in a paper read before the society in January. In a Geissler's tube, containing highly rarefied hydrogen, a small filament of talc was hung by a single horizontal fibre of silk. Two aluminium rings, separated an inch and a quarter, formed the electrodes inside the vacuum. This tube was placed longitudinally with and over the horizontal poles of a large very powerful iron horse-shoe electro-magnet, made of a bar four inches in diameter and four feet in length, and wrapped with nearly 2 cwt. of thick copper wire. A small induction coil sent electric discharges from one ring to the other, producing a brilliant blue light around the negative pole, the positive pole being dark. The moment the magnet was charged, by means of thirty cells of Grove's nitric acid battery, each cell containing twenty square inches of platinum foil, the electric luminosity in the tube, which beforehand was diffused, gathered up into an arch extending one and a half inches beyond each ring, forming altogether a well-defined arch about four inches in length. This luminous arch follows exactly the course of those magnetic rays which traverse through the negative pole. By shifting the tube the piece of talc can be brought at pleasure in or out of this luminous arch. Neither the electric action nor the magnet *per se* produce any motion upon the talc; but when the tube is so placed that the luminous arch strikes against the talc the talc is repelled as much as 30° from the perpendicular. The electric current is passing simply from one ring to the other inside the tube, but the luminous arch in question where it strikes the talc is on the other side of the ring and where no electricity is flowing.

Mr. Spottiswoode's musical vibration experiment consisted of the visible representation of the forms actually assumed by a musical string when producing a note or its harmonies. To show this it is required that the string should be kept in a perfectly uniform state of vibration. This was very ingeniously accomplished by means of tuning forks kept in vibration by electro-magnets, these forming their own breaks in cups of mercury.

The singular action of nuclei in promoting crystallisation has long been known, but recent experiments by Mr. Chandler Roberts, chemist of the Mint, have imparted additional interest to the subject. Minute traces of lead, antimony, bismuth, or arsenic, render the alloy of gold and copper known as "standard gold" crystalline, intensely brittle, and totally unfit for the purpose of coining. This remarkable effect is produced even when the amount of obnoxious metal does not exceed the $\frac{1}{1000}$ part of the mass of standard gold. Mr. Roberts exhibited beautiful specimens of crystalline standard gold and illustrations of the process of toughening brittle gold by means of chlorine recently introduced in the Mint, the adoption of which has afforded a satisfactory solution to a question of considerable importance connected with the manufacture of coins.

SCIENTIFIC SERIALS

THE *Mittheilungen der naturforschenden Gesellschaft in Bern* for 1869, published last year, contain many important papers.—M. E. Schär publishes a valuable contribution to the knowledge of some cyanogen compounds, and a memoir of considerable length on peroxide of hydrogen, and its relations to ferments.—M. A. Gruner communicates a short but interesting paper on the luminosity of the so-called "touchwood," in which he details several experiments, and comes to the conclusion that ozone is to be regarded as the principal cause of the phenomenon.—In geology we find some valuable memoirs by M. C. von Fischer-Ooster, especially a paper on the Rhætic stage in the neighbourhood of Thun, which includes an account of the beds, and a list of the fossils occurring in them, with descriptions of some new species, and many figures. The same author also contributes several smaller papers on the occurrence of a Liassic zone between the chain of the Moleson and the Miremont in the Canton of Freiberg, on the narrow Flysch zone from the Hongrin towards Jaun, on the geological age of the so-called Taviglianа Sandstone, and on the stratigraphical conditions near the Kiiblisbad.—M. J. Bachmann publishes some remarks in opposition to M. Renevier's geological observations on the Alps of Central Switzerland compared with the Vaudoise Alps, and M. A. Rytz a notice on the erratic formations in the Kanderthal.—M. Theophil Studer notices the occurrence of Foraminifera in the Alpine chalks, detected by the examination of thin slices, and also describes a new Swiss form of the genus *Tropidonotus*, for which, however, he does not venture to propose a specific name.—Dr. R. Henzi reports upon the attempts made by him to cultivate *Saturnia Mylitta* and *S. yama maya*, and M. G. Hasler describes and figures an apparatus for giving telegraphic intimation of the height of water in reservoirs, &c.—The Proceedings of the society also contain short notices upon various subjects.

THE *Atti della R. Accademia delle Scienze di Torino* for the first six months of the year 1870 (vol. v. parts 3–7) contains numerous papers on various branches of science, but principally on subjects connected with physics. Of zoological memoirs we have a notice of some new and little-known species of birds collected on the voyage of the *Magenta* by MM. Giglioli and Salvadori, the new species being *Acridotheres leucocephalus* and *Leptoptila chlorauchenia*; a paper by Dr. Giglioli on the phosphorescence of the sea, with notices of the various animals observed by him to be luminous, and descriptions of two new species of the genus *Noctiluca* (*N. omogenea* and *N. pacifica*); descriptions of new species of birds by M. Salvadori, namely, *Saxicola allo-marginata* from the Sahara, *S. Brehmii* from Nubia and Abyssinia, *Brachypus urostictus* from the Philippines, and the type of a new genus allied to *Malacopteron*, *Homochlamys luscinia* (Finsch MS.) from the Philippines or China; and a critical revision of Antinori's descriptive catalogues of birds collected by him in North Central Africa.—M. Cavalli, in a memoir on a gunpowder uninjurious to cannon, maintains the superiority of large grains, especially if made spherical and more regular and hardened at the surface.—A new form of mercurial barometer is described by M. Faà di Bruno, and the barometric formula of Count Paolo de Saint-Robert is discussed by Prof. Dorna, who also presents what he calls a loghypsometrical table for use in applying the barometric formula worked out by him in the determination of altitudes.—Prof. Govi describes a new method of obtaining sensitive flames, consisting in the application of a wire net with meshes about one millim. square to an ordinary gas jet, and lighting the gas after its passage through the meshes. The same author also publishes a note on the influence of sonorous vibrations upon cold and ignited gas jets.—Prof. Boccardo notices the fall of an earthy shower at Genoa on the 14th February, 1870. He gives an analysis of the material, which consisted chiefly of sand with oxide of iron and carbonate of lime, and contained 6·611 per cent. of nitrogenous organic matter. Under the microscope, it was found to contain frustules of Diatomaceæ and fragments of other simple Algæ. No windstorm had occurred immediately before the fall of the shower, which the author considers to have probably come from Egypt.—Prof. Dorna has a note on the scientific importance of Soperga and the Sacra di San Michele to the Observatory of Turin, and upon their respective differences of level.—M. Richelmy communicates some notes on the construction and operation of toothed wheels.—In a paper on nitroglycerine, nitromannite, and pyroxyline, Prof. Sombrero vindicates his title to be regarded as the discoverer of the first of these compounds, and notices the properties and mode of preparation of the other two. M. L. F. Menabrea furnishes some explanations of his views on the principles of elasticity, which are disputed by MM. A. Parodi and G. Barsotti. M. Codazza describes an apparatus devised by him to act as an electrical indicator to give notice of the attainment of the maximum or minimum limits of temperature between which it is required to keep any substance. Prof. Denza describes an aurora borealis observed in Piedmont on the 5th April 1870.—Prof. Govi indicates that Thenevot was the inventor of the spirit-level with a bubble of air. The same author communicates a paper by Prof. Chiò on a barometric formula.—M. Gastaldi notices a collection of stone weapons and instruments from the neighbourhood of the Baltic, and also some ancient weapons and instruments of stone, bronze, or brass, from Egypt. Several of these are figured; one of them, a long, chisel-shaped, bronze instrument, is attached obliquely to a mallet-shaped handle, in such a manner as to serve as a small axe.—Prof. Luvini publishes a long paper on the adhesion between solids and liquids.—M. Genocchi notices some papers ascribed to A. Cauchy.—Prof. Dorna describes the instruments and methods employed at the Observatory of Turin for the measurement of time.—M. A.

Gras communicates a paper on some botanical synonyms. The plants referred to in the last-mentioned paper are *Lindernia palustris* Crantz, anterior to *L. pyxidaria* Linn., and identical with *Anagalloides procumbens* Krock.; *Scirpus quinqueflorus* Crantz; *Stellaria graminea* Linn.; *Galeopsis segetum* Neck.; *Euphorbia sequieriana* Neck.; and *Statice cordata* Linn., said to be distinct from the species so named by Gusson, which is here noticed as *Statice Gussonii.*

THE Proceedings of the Bohemian Society of Sciences contain several papers by M. Emil Weyr on subjects belonging to the higher mathematics, the titles of which it would be useless to give here. One of his papers, however, is on the curves of maximum and minimum electro-magnetic action.—Dr. A. Grünwald also communicates a paper on some differential equations with variable coefficients, and Prof. Blagek a short notice on the triaxial ellipsoid. The titles of several natural history papers are given; one by Dr. Schöbl on the termination of the sensitive nerves in newly-discovered terminal corpuscles in the wing membrane of the chiroptera, and on the minute structure of the membrane is printed in full. This paper has appeared with illustrations in Siebold and Kölliker's "Zeitschrift fur wissenschaftiche Zoologie."—A short notice is given of a lecture by M. Wocel on the significance of stone and bronze antiquities in the primitive history of the Sclavonic tribes, founded on the study of a large collection of casts of such objects from the Ural, Altai, Caucasus, &c.

THE *Verein für Erdkunde* in Dresden published last year its sixth and seventh annual reports, including its proceedings for the sessions 1868–69, and 1869–70. The abstracts of proceedings contain a multitude of short notes upon the results of travels made by members of the Society, and a report upon the doings of the sections of the Society in furtherance of its objects. Besides these, we have in an appendix three memoirs of some importance, namely, contributions to the knowledge of the Hottentots, by M. T. Hahn, relating especially to the language of the "Nama" tribes, but containing besides much interesting matter; a geographical sketch of the Murray and Darling district in Australia, by Dr. H. Beckler; and a curious contribution to the history of geography during the latter half of the Middle Ages, giving an account of the maps and charts of the seafaring peoples of Southern Europe up to the first printing of Ptolemy's Geography, by M. Heinrich Wuttke.

SOCIETIES AND ACADEMIES

Royal Society, March 9.—"Results of Seven Years' Observations of the Dip and Horizontal Force at Stonyhurst College Observatory, from 1863 to March 1870." By the Rev. S. J. Perry.

The object of the present paper is to bring further evidence to bear upon an important question of terrestrial magnetism.

The existence of a sensible semi-annual inequality in the earth's magnetic elements, dependent on the position of the sun in the ecliptic, was deduced by General Sir Edward Sabine from a discussion in 1863 of a continuous series of the monthly magnetic observations taken at Kew. A previous reduction of observations made at Hobarton and at Toronto had first suggested the idea, and a new confirmation of the results has lately been obtained by Dr. Balfour Stewart from subjecting a second series of Kew observations to the same tests as before. The observations, which form the basis of the present discussion, extend over the period from March 1863 to February 1870, during which time the same instruments have been in constant use. These are a Jones unifilar and a dip-circle by Barrow, both tested at Kew, and a Frodsham chronometer. Sir Edward Sabine, who made the Stonyhurst Observatory one of his magnetic stations in the English survey in 1858, greatly encouraged the undertaking of monthly magnetic observations, and the Rev. A. Weld procured in consequence the instruments still in use. Only occasional observations were made with these instruments for some years, and it was only in 1863 that a continuous series of monthly determinations of the magnetic elements was started by the Rev. W. Sidgreaves. He observed regularly until September 1868, when I returned to my former post at the Observatory, and have continued the same work ever since.

A stone pillar was at first erected for the magnetic instruments in the open garden, and this remained in use from 1858 until the beginning of 1868, when a most convenient hut of glass and wood was built for the instruments in a retired corner of the College garden. This alteration was rendered necessary from the placing of iron rails in the vicinity of the old pillar; and although it introduces into the results a correction for change of station, it has the great advantage of securing immunity from disturbance for the future.

Considering the object in view in drawing up this reduced form of the dip and horizontal-force observations, I have judged it advisable to adhere strictly to the tabular forms in which the matter has been presented in previous discussions of a similar nature. Each element is the subject matter of these tables. In the first are the monthly values of the element, the deduced mean value, and its secular variation. Next in order comes the calculation of the semi-annual inequality. The residual errors, and consequent probable weights of the observations and results, compose the third and last table.

The yearly mean values of the horizontal force are found to vary progressively from 3·5926 to 3·6178 in British units, the mean for Oct. 1st, 1866, being 3·6034, with a secular acceleration of 0·0042. Calculating from the monthly tables the mean value of the horizontal force for the six months from April to September, and for the semi-annual period from October to March, we find the former to be 0·0005 in excess over the latter, showing that this component of the intensity is greater during the summer than during the winter months. Treating the dip observations in a precisely similar way, we obtain 69° 45′ 21″ as the mean value of this element for October 1st, 1866, subject to a secular diminution of 1′ 49″·2; the extreme yearly means being 69° 48′ 47″ and 69° 37′ 52″. The resulting excess of 10″ for the winter months in the computed semi-annual means is so small, that the observations tend mainly to show that the effect of the sun's position is not clearly manifested by any decided variation in the dip. Deducing the intensity from the above elements, we obtain for the summer months the value 10·4136, whilst that for the winter months is 10·4128. The intensity of the earth's magnetic force would thus appear to increase with the sun's distance, but the difference is not large enough to have more than a negative weight in the question under discussion. This weight, moreover, is lessened by the slight uncertainty arising from the probable disturbing causes at the first magnetic station.

It is hoped that a second series of observations at the new station will throw greater light on the fact of the sun's influence on terrestrial magnetism, by either confirming the results obtained above, or by adding fresh weight to the conclusions arrived at by the President of the Royal Society.

"Preliminary Notice on the Production of the Olefines from Paraffin by Distillation under Pressure." By Dr. Thorpe and John Young.

"Contributions to the History of the Opium Alkaloids. Part I.—On the Action of Hydrobromic Acid on Codeia." By C. R. A. Wright, D.Sc.

Mathematical Society, March 9.—Mr. W. Spottiswoode, F.R.S., president, in the chair. Mr. C. R. Hodgson, B.A. Lond., was elected a member: and the following gentlemen were proposed for election:—The Hon. J. W. Strutt, Major F. Close, R.A., and Mr. James Stuart, Fellow and Assistant-Tutor of Trinity College, Cambridge. Two models of surfaces were exhibited by Prof. Henrici, which had been exhibited and described at previous meetings of the society. Prof. H. J. S. Smith, F.R.S., read a paper on "Skew cubics." The secretary (Mr. Tucker) then read a communication from Prof. J. Clerk Maxwell, F.R.S., entitled "Remarks on the Mathematical Classification of Physical Quantities." The classification referred to was founded on the mathematical or formal analogy of the different quantities, and not on the matter to which they belong. Thus a finite straight line, or force, or velocity of rotation, &c., are quantities, differing in their physical nature, but agreeing in their mathematical form. The two methods of classification, the one just referred to and the obvious classification founded on that of the sciences in which the quantities occur, may be distinguished by calling the first a mathematical and the second a physical classification of quantities. The secretary afterwards read a "Note on the History of certain Formulæ in Spherical Trigonometry," communicated by Mr. I. Todhunter, F.R.S., in which the formulæ usually known as Gauss' Analogies were claimed for Delambre. Dr. Hirst presented ten "Memoirs by M. Chasles" to the library of the society.

Entomological Society, March 6.—Mr. A. R. Wallace, president, in the chair. Baron de Selys-Longchamps was elected an honorary member, the Rev. T. A. Preston an ordinary member, and Mr. G. C. Champion a subscriber. Mr. Jenner Weir exhibited a small collection of butterflies from Madagascar. Mr. F. Smith exhibited two small branches of ash, from which a hornet had been observed in the act of removing the bark. He said that Réaumur had recorded a similar observation, and was of opinion that the insect was trying to reach the sap for food, and was not obtaining building materials. Mr. Smith further made some remarks on the disputed luminosity of Fulgora, and expressed himself in favour of the opinion that these insects are occasionally luminous.—Mr. Müller read notes on a gall-making Cecidomyia upon *Campanula rotundifolia.*—Mr. Lewis called attention to cases of antennal malformation in Lepidoptera.—Mr. Butler exhibited forms of *Cœnonympha Satyrion* from the opposite sides of the Gemmis, showing marked variation—Dr. Sharp communicated notes on some British species of Oxypoda.—Mr. Lowne read a paper on "Immature sexuality in insects." The author thought that species sometimes originated from the maturity of the sexual organisation before the acquirement of adult characters; a conclusion he had arrived at in consequence of the early development of the organs in the embryo and larva. He further stated that, in his opinion, the larval and pupal conditions were probably acquired, and not direct, stages of development.—Mr. Briggs detailed experiments upon Lepidoptera, undertaken with a view of testing if the numerical proportion of the sexes, or sex itself, were dependent upon the food of the larva; the results negatived such suppositions.

Zoological Society, March 7.—Prof. Flower, F.R.S., in the chair. Mr. P. L. Sclater read the first part of a series of "Notes on rare or little-known animals now or lately living in the Society's Gardens."—These he had drawn up while engaged in preparing a new edition of the List of Vertebrated Animals in the Society's collection.—Dr. A. Gunther read a List of the known Lizards belonging to the family *Sepidæ*, to which were added notes on some of the species.—A communication was read from Mr. F. Moore on some new species of Insects collected by Dr. John Anderson, during the recent expedition to Yunan.—A communication was read from Mr. A. G. Butler, containing descriptions of some new species and of a new genus of Diurnal Lepidoptera of the family *Pierinæ*, with a monographic list of the species of the genus *Ixias.*—Mr A. D. Bartlett, Superintendent of the Society's Gardens, read notes on the birth of the Hippopotamus which had lately taken place in the Society's Gardens.

Hackney Scientific Association, February 28.—"The Fossil remains of the Mammalia found in the Lea Valley," by Mr. R. E. Olliver. Some extensive excavations in this district have recently exposed to view numerous beds of shell-marl, peat, loam, sand, and gravel, and, what is more interesting, have brought to light the remains of an ancient mammalian fauna. These remains chiefly belong to living species, as may be inferred by the deposit being assigned to the Post-Tertiary epoch. The mammalian remains which have up to the present time been determined are—Human remains, wild horse, red, fallow, and reindeer, elk, beaver, fox, wolf, goat, great fossil ox, Celtic shorthorn or small fossil ox, domestic ox, wild hog, gigantic round-antlered deer, mammoth, great cave bear, and the remains of an ox closely resembling *Bos longifrons*, which the writer strongly suspects to belong to a disputed series named *Bos frontosus*. The fossil bones attesting the existence of these mammalia are found principally in the shell-marls and peat mosses, with the exception of the mammoth, cave bear, and great round-antlered deer, which are found in the grey sub-angular gravels, and rarely at a less depth than ten feet from the surface. The human remains, consisting of several skulls, possibly belong to that class named by Prof. Huxley the river-bed-skull. These skulls certainly belong to that period when the great fossil ox and elk were living, and not to that of the mammoth and cave bear. If, however, the evidence of the workmen is reliable, several skulls were found at a depth of thirty feet.—"A Catalogue of Variable Stars, with remarks upon their physical constitution."

CAMBRIDGE

Philosophical Society, February 27.—Communications made to the Society:—By Mr. W. H. H. Hudson, St. John's College, "On Observations made at San Antonio on the Solar Eclipse of Dec. 22, 1870." By Mr. Clifford, Trinity College, and Mr. Moulton, Christ's College, "On the Solar Eclipse of 1870." Mr. Hudson described the observations made at San Antonio during the eclipse, and with regard to his own with the polariscope came to the following conclusion: that observations of polarisation made with telescopes not specially prepared for the process are worthless, and that such polarisation of the corona as he was able to detect was due to the intervening atmosphere; appearances of polarisation being produced when light shines through a thin cloud. Prof. W. G. Adams exhibited with the lime light some photographs of the corona, and commented upon them. Mr. Clifford, who had been with the Sicilian party, described his experiences, and differed to some extent from Mr. Hudson as to the instrumental defects and the absence of polarisation: while Mr. Moulton corroborated Mr. Hudson's conclusions, and read a very interesting communication from Father Perry, summing up the results of the spectroscopic observations at the various stations.

WINCHESTER AND HAMPSHIRE

Scientific and Literary Society, February 22.—The Rev. G. A. Seymour, M.A., and Beresford N. Earle, M.A., M.B., Fellow of the Cambridge Philosophical Society, were elected members. There was a good show of objects of interest in natural history, amongst which, exhibited by the president, was a collection of British land and fresh water shells, and a specimen of recent chalk dredged by Dr. Carpenter from a depth of 2,435 fathoms in the Atlantic. Microscopic slides of the same were exhibited by Mr. F. I. Warner, showing the unbounded amount of animal life at that enormous depth. Dr. Heale showed, under the microscope, the curious movement of the spiral vessels of the Collomia seed.—The Rev. W. Awdry, M.A., read a learned and interesting paper entitled "Some Ideas worked out in Gothic Architecture."

GLASGOW

Geological Society, February 2.—Mr. John Young, V.P., in the chair. "On the Boulders found in Cuttings on the Beith Branch Railway, considered in Relation to their Parent Rock; with observations on the local character of the Boulder Clay," by Mr. Robert Craig. The lines of railway referred to, now in course of formation, run nearly south-east from Beith, changing as it reaches Waterland to direct east. The striations upon the glaciated rock-surfaces in the district have a general bearing, per compass, of nearly N.E. to S.W.; the line accordingly, at its western terminus, crosses them almost at right angles. The cuttings run nearly parallel to the southern termination of that range of trap hills which extends from Gleniffer to Beith, and at the distance of little more than a mile from it. The carboniferous strata crop out along the southern boundary of this trap range, and consequently about a mile to the north of the railway. In the trap range four well-marked varieties of porphyrite occur, which, with the easily distinguished beds of the Carboniferous limestone, gave the geologist an opportunity of classifying the boulders and tracing them to their source with an exactitude not always attainable. Mr. Craig then referred to the boulders from a distance, consisting of Old Red sandstone, clay slate, mica and chlorite schists, quartz, gneiss, granite, found associated and intermixed with the local boulders. Of these the Old Red sandstone figures highest, and consists of two varieties—one of a highly red colour, the other of a dark grey. Both of these are found *in situ* along the shores of the Firth of Clyde, and in other parts of Scotland, flanking the Highland mountains. He considered these erratics might be accounted for without either drift-ice or submergence, simply by the operation of land-ice, or glaciers, bearing these fragments in passing from the Highland mountains, whose tops, many of them, would be 1,000 feet above the ice, even allowing the sheet to be 2,000 feet thick. It was well-known that ice, in moving over uneven ground, became rent into fissures and crevasses, down through which the stones it was carrying on its surface found their way to the bottom, and thus became mixed up with the "foot-board moraine." In further proof of this, he gave the highest points above the cuttings whence the local boulders could have been derived, showing that these boulders could not have fallen upon the surface of an ice sheet thicker than 200 feet; and, besides, that many of them came from beds scarcely higher than the position in which they are now found. The perfectly local character of the boulder-clay, with the exception of the erratics, he thought was demonstrated; and from some sections in which he had followed the course of the ice-stream, he found that there was a change in the boulders every three to five miles, less or more, according to the roughness or evenness of the ground.—A collection of phosphates from Charleston, U.S., was exhibited by Mr. Potts and Mr. Naismith, together with some large fossil teeth, vertebræ, &c., from the same locality. Mr. Potts stated that large quantities of these phosphates are being used in America, and also imported into this country, for the manufacture of artificial manures. The deposit from which they are taken is found along the banks of many of the rivers in South Carolina, and immediately under the surface soil of the land lying between; and is supposed to underlie a large portion of the coast and sea-island region of that part of America. It consists of layers, varying six inches to several feet in thickness, of irregularly rounded nodules, mixed up with an immense quantity of bones—ribs, vertebræ, tusks—of various species of animals, all more or less petrified. The nodules yield 50 to 60 per cent. of bone phosphate; while from some of the bones as much as 80 to 85 per cent. of this fertilising substance had been obtained. The Chairman said there could be no doubt this remarkable deposit of phosphates belonged to the Tertiary period; and probably its earlier division, the Eocene. The Tertiary formation is largely developed along the southern coast of North America, stretching in a belt of considerable breadth from North Carolina to the Gulf of Mexico, and leaving the coast-line only at the delta of the Mississippi. The whole series of fossils, like those before them, indicated a much warmer climate than now prevailed in that part of the world, and showed that the waters of the sea were teeming with large and powerful forms of life.

DUBLIN

Royal Dublin Society, February 20.—Dr. Croker King in the chair. Prof. Macalister delivered a discourse "On Recent Advances made in Comparative Anatomy." He alluded to the investigations of Kowalersky published in the Transactions of the Imperial Academy of St. Petersburg as to the relationship that exists between the Ascidians and Vertebrates; also to the many recent investigations into the comparative myology of the upper and lower extremities, and alluded to a very simple yet neat nomenclature of the muscles of these parts.

Natural History Society, March 1.—Rev. Dr. Haughton, F.R.S., in the chair. A paper was read by Mr. G. H. Kinahan on ferns observed in Yar or West Connaught, the part of Co. Galway that lies west of Loughs Mask and Corrib, with localities of a few rare ferns in south-west Sligo.—Mr. W. Andrews read a paper, "Notes on the Ichthyology of the South-West Coast of Ireland." It is always of interest to bring to notice any new facts either in the zoology or botany of a country or district, but more especially so when any discovery can be recorded which presents new features of animal or vegetable life, the existence of which had not been previously known. The chief remark conveyed in this paper is with reference to the capture in January last on the south-west coast of several specimens of a fish of extremely rare occurrence, the *Trichiurus lepturus*; the occurrence of *Centrolopus pompilus* in numbers off the coast of Dingle was also mentioned, and also that of a fine specimen of the tunny caught in Brandon Bay, October 1869. The author gave many details as to the structure of the *Trichiurus*, and concluded with stating that he felt confident that we have not yet gleaned the extent of interest that exists in the marine zoology of our shores, or of the deep water of our coasts.

Royal Zoological and Geological Societies of Ireland, March 8.—Prof. Hull, F.R.S., in the chair. The Rev. Dr. Haughton, F.R.S., read a paper on the Mechanism of Flight in the Albatross (*Diomedea exulans*), considered in relation to its muscular anatomy.—Prof. Macalister read a paper on some Parasites found on Animals from the Dublin Zoological Gardens.—Rev. Dr. Haughton made some remarks on the recent death of four lion cubs in the Society's gardens.—Prof. Traquair exhibited a collection of carboniferous ganoid fishes from Wardie, in Scotland.

HALIFAX, NOVA SCOTIA

Institute of Natural Science, February 13.—J. M. Jones, F.L.S., President, in the chair. Dr. Honeyman, F.G.S., read a paper, "On the Geological Formation of the Picton Coal Field," which was a continuation of his record of geological discovery in Nova Scotia, delivered at the November meeting of the Institute. The present paper showed that the Devonian was absent, and that the underlying formations were Upper and Middle Silurian, arranged in several anticlinals, synclinals, and monoclinals. The lithological character of these formations at the time of the deposition of the overlying Carboniferous strata was as it now exists. The strata were more or less metamorphic, and the system of folds was, as has been indicated, and were washed on the north, south, and west, by the seas of the Carboniferous period. Lower Carboniferous conglomerate and grit succeeded by gypsum were deposited on Lower Helderberg strata (Upper Silurian), highly fossiliferous at Irish Mountain. Argillaceous shales with overlying limestones were formed on Lower Helderberg strata (very fossiliferous), and rather higher in the series than the strata of Irish Mountain. In Springville there are two localities where the Silurian strata on the same horizon as Irish Mountain (possibly a little lower) are overlaid *directly* by Lower Carboniferous limestone. Farther up the river there are three localities where Lower Carboniferous limestone overlies Silurian strata; in two cases, a breccia is formed with the limestone and metamorphic slates belonging to the lowest part (or nearly so) of the Middle Silurian age. In the remaining case, in the river at Pleasantville a great band of Lower Carboniferous limestone lies unconformably on Lower Helderberg or Upper Silurian, highly metamorphic. In Cross Brook, Irish Mountain, Lower Carboniferous sandstones with limestone overlie Clinton strata of the Irish Mountain Silurian series with fossils, and also the denuded axial greenstone. At McLellan's Brook, Lower Carboniferous grits succeeded by limestone come up against the back of Clinton strata of the other side of the Irish Mountain anticlinal. At the lowest falls of Sutherland's River, Lower Carboniferous conglomerate comes up against the back of the Medina slates, metamorphic, apparently non-fossiliferous of Wier's Mountain of the monoclinal series. Higher up Sutherland's River at McPherson's is the passage from the Picton into the Merigomish Carboniferous area. Here Lower Carboniferous grits containing a brine spring come up against the back of Medina strata, very little altered with characteristic Petraia and Lingulæ. These are succeeded by Clinton shales with cone in cone concretions and lingulæ nodules in abundance. In this district are two mountains with porphyry. Conglomerate, grit, argillaceous shale, and limestone of Lower Carboniferous age, have thus been formed contemporaneously. Then succeed alternations of sandstone and limestone and gypsum. Thus are formed limited areas of limestone in different geological positions and entirely disconnected. The order of formation of the representative beds of limestone appears to be as follows:—1. The Black pyritiferous limestone in closest contact with the Lower Helderberg slates having obscure brachiopoda visible, particularly by weathering. 2. The Lithostrotian limestones of Springville and McLellan's Brook. 3. Limestone strata with intercalary shales, below Springville Factory on the river, highly fossiliferous, containing Orthocera, Bellerophon, Gasteropoda, Conchifera, Crinoidea, and Fucoids. 4. Thick bedded limestone formed of an agglomeration of minute Conchifera and Crinoidea. Above this are strata with teeth of cochliodus. This limestone appears to be of the same age as similarly formed limestones on the West Branch having Conularia. In the Silurian formation on separate anticlinals are—1. An apparently valuable bed (?) of limonite. 2. A bed of fossiliferous iron ore of considerable thickness. 3. Pockets of specular iron ore. These, although separated, are of the same geological age—Clinton. The Medina and Lower Helderberg strata of the area examined, in their passage across rivers and brooks, form numerous waterfalls, some of which are mill sites. Fine exposures of Lower Helderberg fossiliferous strata in McLellan's Brook and Sutherland's River, form the foundation of a mill dam in the brook and formidable rapids in the river. The Picton Coal Field is well known through the writings of Dawson and others. Its thick coal seams have been represented by columns in the International Exhibitions of London, 1862, Dublin, 1865, Paris, 1867. In the discussion which followed, the President called attention in an interesting zoological fact mentioned by Dr. Honeyman to one part of his paper, viz.—the common cattle of the neighbourhood in which he had found a brine spring, resorting to such fountain and greedily drinking its waters. The same habit was also to be observed in the wild buffalo of the western prairies, which annually resorted to certain well-known "salt licks" for a similar purpose.

BERLIN

Berlin Society of Anthropology, Ethnology, &c.—Prof. Virchow continues (*Zeitschr. fur Ethnologie*, 1870, vi.) his communications on the subject of the urns with sculptured faces, already treated by himself, by Nilsson, and by Kupfer; and also read a paper on "Settlements belonging to the Stone age in Nieder-Lausitz," &c., and a third on a visit to the Westphalian bone caverns. Among the same transactions may be noted a letter from Fisch on the *Framea*, in which he derives the word from the old Gothic *fram* (the English *from*) and explains to mean a weapon thrown *forwards*,—*telum missile*: the subject was again discussed at a late sitting (*ib.* iv. p. 347). A paper was read by V. Martens on the implements used by the Dyaks of the interior of Borneo: a letter from Kupfer on the remarkable vases with human faces which were the subject of Virchow's communication to the same society last March; and a long paper, with woodcuts, on the vitrified stone sites of Oberlausitz by Prof. Virchow, which shows with what accustomed ability the great pathologist pursues the inquiries which amuse his holidays.

Paris

Academy of Sciences, March 6.—M. Henry Sainte-Claire Deville proposed to his colleagues to enlarge the scope of their proceedings, and to deliberate upon any subject relating to social matters which can be promoted through the instrumentality of science. This idea was warmly advocated and supported by M. Bouley, M. Dumas, and others, and met with only one opponent, M. Combes, general inspector for Ponts-et-Chausées, who said that such a step was contrary to the rules of the Academy, and that the Academy besides had no right to pronounce on a communication from its own members. But ultimately the discussion of the proposition was referred to a secret committee, which is to be held probably next week. This sitting was rife with discussion.—M. Faye having referred to a special committee the project of a railway to be constructed between America and Europe through the Aleutian Islands, General Morin protested, saying it was merely wasting time to refer such wild schemes to members, and that the committee was, at all events, not bound to give any opinion at all. In spite of this protest, M. Faye referred the communication to a member in order to know from him if it was worth being sent before a special commission.—M. Delaunay and M. Bertrand disputed what was the precise opinion of M. Hopkins on the thickness of the solid crust of the earth. M. Delaunay said moreover that calculations proved it had no influence on the revolution of the earth, as no movement of the internal fluid could possibly have any effect whatever.—M. Bouley declared that the flesh of animals infected with the cattle plague was not unwholesome for food. It is true that such flesh is served out to poor people supplied from municipal benevolent institutions. But such is not the view taken by the paying public, as the purchase of beef is falling, and the sale of mutton or pork increasing daily. The rinderpest is raging so severely in the herds destined for the revictualling of Paris, that very often carts loaded with carcasses are seen passing through the streets of the city.—M. Janssen was present at the sitting, and he is going to London, where he will arrive by the end of the present week. Many of the *francs-fileurs* have resumed their seats without any remark having been passed upon them. M. Becquerel the elder, the celebrated electrician, died in Normandy during the investment of Paris. He was about 80 years of age. The water-tubs and towels disposed in case Prussian shells should set fire to the building have not yet been removed from the hall.

BOOKS RECEIVED

English.—The Natural History of the Strait of Magellan: R. O. Cunningham (Edmonston and Douglas)—The Story of Aristæus and his Bees: R. M. Millington (Longmans)—A Few Poems: E. Smith (Dunn and Fry). - The Ancient Geography of India: A. Cunningham (Trübner and Co)—The Chronological History of Animal Plagues from B C. 1490 to A.D. 1800: G. Fleming (Chapman and Hall).

Foreign—(Through Williams and Norgate)—Die elektrische Doppelhebel: J. N. Czermak—Untersuchungen aus den Institute für Physiologie u. Histologie in Graz 2^tes Heft: A. Rollett.—Echinologie Helvétique: Désor et de Loriol.

DIARY

THURSDAY, March 16

Royal Society, at 8.30—Description of Ceratodus, a Genus of Ganoid Fishes, recently discovered in Rivers of Queensland, Australia: Dr. Günther, F R S.—On the Formation of some of the Sub-axial Arches in Man: G. W. Callender.

Society of Antiquaries, at 8.30—On Photographs of Armenian Antiquities: Captain Lynch—On an Undescribed Expedition to Britain in the Reign of Augustus: W. H. Black, F.S.A.—On certain inscribed leaves of lead in the British Museum: W. De Gray Birch.

Linnean Society, at 8.

Chemical Society, at 8.

Royal Institution, at 3—Davy's Discoveries: Dr. Odling

London Institution, at 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers, M.A.

FRIDAY, March 17.

Royal Institution, at 9—On the Eclipse: J. Norman Lockyer, F.R.S.

Royal College of Surgeons, at 4—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, March 18.

Royal Institution, at 3—Spirit of the Age: Mr. O'Neil.

SUNDAY, March 19.

Sunday Lecture Society, at 3.30.—The Total Eclipse: J. N. Lockyer.

MONDAY, March 20

Anthropological Institute, at 8—Adjourned Discussion on the Racial Aspects of the Franco-Prussian War.—On the Migrations of the Georgians, Circassians, and Amazons, and their connection with the Tibeto-Caucasian Race: Mr. Hyde Clarke

Entomological Society, at 8—On Additions to the Atlantic Coleoptera: Mr. Wollaston.

Victoria Institute, at 8—On some Curiosities of Ethnology:—Rev. J. H. Titcomb.

London Institution, at 4—On Astronomy: R. A. Proctor, F.R A.S. (Educational Course.)

Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

TUESDAY, March 21.

Zoological Society, at 9—Report on additions to the Society's Menagerie during the month of February 1871: P. L. Sclater.—On the Birds of Santa Lucia, West Indies: P. L Sclater

Statistical Society, at 7.45.—On Statistical Returns required by Parliament: Mr. Purdy.

Royal Institution, at 3.—Nutrition of Animals: Dr. Foster.

WEDNESDAY, March 22.

Geological Society, at 8.—On the Passage beds in the neighbourhood of Woolhope, Herefordshire, and on the Discovery of a new species of *Eurypterus*, and some new land-plants in them: Rev. P. B. Brodie, M A, F G S.—On the Cliff-sections of the Tertiary beds west of Dieppe, in Normandy, and at Newhaven, in Sussex: W Whitaker, B.A., F G S.—On New Tree-ferns and other Fossils from the Devonian: Prof. J. W. Dawson, F.R.S.

Society of Arts, at 8.—On Drill, the Complement of the Present School Teaching: Major-General Eardley-Wilmot, R.A.

Royal College of Surgeons, at 4.—On the Teeth of Mammalia: Prof. Flower.

THURSDAY, March 23.

Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

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Errata.—Page 370, first column, line 10 from bottom, for "show us traces" read "show no traces", second column, line 15, for "limit" read "limb."

THURSDAY, MARCH 23, 1871

BOTANICAL MUSEUMS

THE keeping up at the public expense of two great rival National Botanical Establishments, the one in London the other at Kew, in a state of continual competition with, instead of aid to, each other, whilst a third independent one, also national, may occasionally come into collision with one of them, seems to be a waste of public money, without any advantage to science or to the public, and attended with many inconveniences.

At the same time two great Botanical Museums and Herbaria, the one in connection with the Natural History Museum in London, the other with the Botanical Gardens at Kew, working in harmony with each other, but for different purposes, and separated by a clear line of demarcation from the Economic Museums of South Kensington, would always be productive of great benefit to science and gratification to the public.

The main purposes of a Botanical Museum and Herbarium may be said to be threefold—the Study of plants, their Comparison, and their Exhibition; the first purely scientific; the second sometimes scientific, sometimes popular, the third chiefly popular. For the first, Kew affords incomparable advantages, the second and third would probably be best promoted in town, provided always that the two establishments work in perfect harmony, with unity of plan, both in general arrangements and in matters of detail.

1. For the close Study of plants,—the only sound foundation upon which the science of Botany can be usefully established,—for their accurate determination and practical classification, the requisites are: that the herbarium should be as rich as possible not only as to genera and species, but as to variations of all sorts and repetitions of the same form from different localities and stations; that the herbarium should be a single one, the geographical arrangement being kept in subservience to the scientific classification, and without any detached smaller herbaria, except such definite historical ones as only require occasional reference like the books of a library; that there should be good accommodation for the sorting of unnamed collections and fresh arrivals, ample means for the dissection and examination of specimens not only by the staff of the establishment, but also by scientific botanists in general, who, under special regulations, are allowed to work in the herbarium, and store-rooms for duplicates required for exchanges, &c.; that there should be in the same suite of rooms as the herbarium a botanical library, as complete as possible, and a series of drawings of plants, also as complete as possible; that the herbarium should be in close connection with the national collection of living plants; and that it should be under the keepership of a resident scientific botanist, with the requisite staff of scientific assistants. All these essentials are at present afforded by the Herbarium at Kew, in a degree far beyond what can be met with in any other establishment at home or abroad.

2. The Comparison of plants—their practical and rapid determination without dissection, or the obtaining a general idea of natural groups from the Order down to the Species, as required by the general naturalist, by the follower of sciences in immediate connection with botany, especially the palæontologist, or by the mere amateur—demands a very different herbarium and museum from that of the working establishment. It should consist of accurately named select specimens, representative of as many species or well-marked varieties as possible, without duplicates in the same collection. It might be advantageously divided into two separate collections, one a general typical one, the other geographical. Separate collections also of leaves and of fruits, all accurately named, and so arranged as to enable them to be rapidly glanced over, would be most useful to the palæontologist. Such a museum would require no space for the sorting and determining of unnamed collections, nor for the storing of duplicates, and no provision for the dissection of specimens except for the personal use of the keeper and his assistants, being supplied only with such tables or other appliances for consultation as are usually required in a library. Its library should be extensive, but select rather than complete, and should include various palæontological and other works on kindred sciences, not required in the working herbarium. It should be in near connection with the National Museums for kindred sciences, especially with other palæontological collections. The keeper should be a scientific geologist, as well as botanist, and would require probably but one scientific assistant.

3. The Exhibition of plants, or rather of botanical specimens, is for the purpose of exciting the interest and gratifying the curiosity of the general public, and for this a herbarium, strictly so-called, is of no use—the public would never look beyond the outside of the cases. It requires the display in glass cases of such selected specimens of plants or their parts, accompanied by explanatory notes and diagrams, as may give at a cursory glance some idea of the characteristic features of the principal groups of plants; and to these might be usefully added a few specimens remarkable only for their beauty or singularity, for the purpose of attracting the eye, and riveting the attention of the observers. As these specimens, when once placed, require no further handling, and no care beyond the inspection of an ordinary assistant, and as the objects of visitors to such a Museum would be much promoted by a ready connection with the public Museums in other branches of natural history, it would seem highly advantageous that it should be attached to the herbarium for comparison, and form part of the London Botanical Museum, in close proximity to the National Museums of Zoology and Geology.

We have now no Museum in any degree adequate to these two purposes of Comparison and Exhibition, but were the two national collections of the British Museum and Kew combined, all unnamed plants, duplicates, and specimens of interest only to the scientific botanist, removed to Kew, and in return, from the immense mass of materials there accumulated, the London herbaria completed by accurately-named representative specimens, there would result collections richer in species and far more useful than any actual Continental ones; and as science advances and materials increase, these collections would be constantly kept up to the mark by named specimens from Kew, whilst their scientific arrangement and application to use could not be under a direction better qualified than that of the recently-appointed keeper of the botanical department of the British Museum.

In this London Botanical Museum would be also appropriately placed various pre-Linnean and other botanical collections, having only a historical or other adventitious interest, but there would be little use in attempting there anything corresponding with the Museum of Economic Botany, which has acquired so much importance, and is so well placed at Kew. That could only come into competition with the economic collections at South Kensington, but all prejudicial collision between the two is clearly avoided, and each one will increase its own practical utility by strictly adhering to the rule that at Kew the products are arranged according to the plants they are derived from; at South Kensington, according to the uses they are put to.

POPULAR ORNITHOLOGY

Cassell's Book of Birds. Translated and adapted from the text of the eminent German Naturalist, Dr. Brehm, by Thomas Rymer Jones, F.R.S., Professor of Natural History and Comparative Anatomy in King's College, London. 400 woodcuts and coloured plates. Parts I.—XIV. (London: Cassell, Petter, and Galpin)

PERSONS wishing to be misinformed on the subject of Ornithology should obtain and read the "Book of Birds" now in course of publication by Messrs. Cassell, Petter, and Galpin, and recommended by them to "everyone who wishes to know all that is known about birds." The advertisement whence these words are quoted also tells us that the work, when completed, is to contain "upwards of 400 engravings, embracing every species of birds known to exist;" but as on a moderate computation some 12,000 species of birds have been described, it is pretty clear that to fulfil that promise each engraving should represent 30 species or thereabouts. The most cursory inspection of the portion published (and we have the fourteenth part lying before us) will show that nothing of the kind has been done, and that many groups are left without an illustration at all.

Furthermore, the work is announced as "translated and adapted from the text of the eminent German naturalist, Dr. Brehm, by Thomas Rymer Jones, F.R.S., Professor of Natural History and Comparative Anatomy in King's College, London," a collection of assertions which we take the liberty of questioning. We are aware of the recent existence of no fewer than four German naturalists of that name, all of them, we believe, entitled to the doctorial prefix. Of these four, which is the one whose labours are chosen for the exercise of Prof. Jones's industry in translation and ingenuity in adaptation? *The eminent* Dr. Brehm ought, of course, to be the answer; but then the most "eminent"—that is the best known and most prolific writer of the four—was Dr. Christian Ludwig Brehm, who, having attained great notoriety as a "splitter" of species, died at an advanced age some half-dozen years since, leaving two of his three bedoctored sons behind him. Now, Dr. Brehm, the father, among his many works certainly never published one which could be "adapted" to the form of Messrs. Cassell's "Book of Birds:" nor did Oscar Brehm, the son, who died in his father's lifetime. The question is therefore narrowed to the works of the survivors. Of these Dr. Reinhold Brehm has contributed several ornithological papers to journals, but none of any great importance, and there is no need to accredit him with the authorship of any work at all resembling the present. It seems therefore that Dr. Alfred Brehm must be in the eyes of the English publishers and translator "the eminent Dr Brehm." We are inclined to believe that the production we are now reviewing is his offspring, whether he deserves to be called "the eminent German naturalist" or not, and that it has not hitherto been printed, since an examination of his work, "Das Leben der Vögel," from which some of the illustrations in the present book are taken, fails to show that its text furnishes the groundwork for "Cassell's Book of Birds."

Having thus justified, as we hope, our doubts as to the "Book of Birds" originating from "the eminent Dr. Brehm," we must further express our doubts as to Prof. Jones being the translator and adaptor of it from the German of another naturalist of the same name. Here our doubts, it may be thought, do not rest upon so satisfactory a base; but the meritorious work by which Prof. Rymer Jones is best known, his "Outline of the Animal Kingdom," shows that its author is gifted in no common degree. The character of Professor Jones's volume was and is caution and accuracy, the character of the "Editor's Introduction" to the "Book of Birds" is the reverse. Here is an example. Its writer says (p. 17): "In order to render the following account of the structure of a bird's skeleton intelligible to the non-scientific reader we have delineated that of the Goose," and a reference is added to "Fig. 12," which faces these words. Now we scarcely expect that we shall be believed, but it is an undoubted fact that there is no figure of a Goose's skeleton at all, and that "Fig. 12" represents the skeleton of a bird so entirely different as a Pigeon; while so far from the inference being true that the editor has "delineated" the subject for the express purpose of enlightening his readers, we must declare that the woodcut in question is a very bad enlargement of what has been for years a stock-figure in anatomical handbooks. We do not pretend to know its origin, but we have now before us a far better copy of it in a Swedish work,* and it has been repeated in many other books. That Prof. Rymer Jones has been guilty of such a blunder, to say nothing of such a *suggestio falsi* as this, we hold to be incredible. Again we have close by another woodcut (p. 22), which we are told represents "A young chicken shortly after its escape from the egg." Now we cannot believe that such an explanation was written by Prof. Rymer Jones, for he must well know the figure to be that of a young Blackbird assuming the first or nestling plumage, as it is rightly said to be in the "Catalogue of the Physiological Series" of the Museum of the College of Surgeons, where (vol. ii. Part II. p. 312, pl. xlv. fig. 4), the original of the woodcut may be found. Those who can believe that Prof. Rymer Jones does not know the difference between a Goose's skeleton and a Pigeon's, and between a Chicken newly hatched and a Blackbird just about to leave the nest, may believe it, we unhesitatingly declare we do not.

But it might be urged that all these matters are of little consequence; that 12,000 birds obviously cannot be figured in 400 engravings, and the statement was only intended to mean that the work is profusely illustrated; that English readers do not care a button whether there are as many Brehms as birds, or what books they write; and that diabolical agency in the press may blunder as to woodcuts or their explanations. Suppose we grant all this, and consider the rest of the work. We have expressed doubts as to the authenticity of the "Editor's Introduction," but even if they are rightly founded, the affair may be condoned should the translator and adaptor have performed his task well. We imagine that the first duty of a translator is to give an exact rendering of every phrase or name in the original, and it becomes necessary to see how far this has been done in the present instance. If there is a foreign bird well-known in this country, it is the common Grey Parrot—here we are taught to call it the "Jako" (p. 35), which may well be its German name but is not an English one. So also the great South-American group of Tanagers are throughout (pp. 150-155) "Tangaras" A similar case is that of the Serin, which is left (p. 106) under its Teutonic appellation of "Girlitz," which common German idiomatic term is comically rendered (p. 107) "a little pair;" and the British public has to find out as it best can that by "Tree-Falcon" (p. 305) is meant our well-known summer visitant the Hobby, though under the latter name it has flourished for hundreds of years. Indeed, the translator's ignorance of his subject is manifest on almost every page, but nowhere is it so remarkably displayed as when he introduces (p 50) the word "Dronte," without the slightest apparent perception that by it is meant our old friend, the Dodo! Further proof of his incompetency for his task is shown by the mistakes (which cannot all be misprints) in the names of naturalists; thus we have "Jerdan" (p. 30) for Jerdon, "Speke" (p. 39) for Spix, "Boja" (p. 126) for Boie, and "Nardoi" (p. 300) for Nordvi. That a corresponding indifference to the niceties of scientific nomenclature should be exhibited is, of course, to be expected; but, setting aside such untechnical forms as "*Corys alauda arborea*" (p. 204) and "*Cecropis-Hirundo-domestica*" (ii. p. 105), we can hardly think that Dr. Alfred Brehm could have ever said that Linnæus named a bird *Derotypus accipitrinus* and *Derotypus coronatus* (p. 42), and we must suppose that the original sentence has been misunderstood.

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* "Grundlinier till Zoologiens Studium," af Karl Torin (Stockholm, 1870) 3d ed. i p. 87.

Another instance, if another be wanted, to prove the inaptitude of the translator for his work, is the confusion existing in his mind between words like "variety," "race," "tribe," "family," and even "order." However much naturalists may differ as to the limits or value of the groups thus designated, all are expected to have clear notions as to what the terms mean. The translator of the "Book of Birds" jumbles them together, and thus raises fresh doubts as to his identity with Prof. Rymer Jones. We have "varieties" spoken of (p. 92), where species are clearly meant; "race" (p. 55 and p. 150), when something at least as comprehensive as a family is intended; "tribe" (p. 47 and p. 122) without any definite meaning at all; and "family" (p. 43 and ii. p. 136) in a sense as obscure; while "order" is used so vaguely that in one place (p. 23) it includes all the parrots and some other birds, which last, when they come to be described (p. 83) are spoken of as forming an "order" of themselves, so that we have an "order" within an "order," whereat our reader will probably exclaim "Disorder!" Dr. Alfred Brehm may have some queer ideas as to classification (of which more immediately), but we feel sure he never committed such a high crime and misdemeanour as this.

Having thus noticed some of the shortcomings of the translator, we must say a few words on the author's treatment of the subject. We have just referred to his ideas on classification, but it is not easy to ascertain what they really are, for no scheme of arrangement is given. Enough however is to be gathered to show that they are somewhat peculiar if not original. We do not quarrel with him on this account. In the present state of ornithological science, its teachers, as well as its students, may well be pardoned for not adopting any plan already promulgated; but in a work intended for "everyone who wishes to know all that is known about birds," it is only reasonable to expect that the projects of prior systematists should receive some attention, and due cause shown why such and such arrangements are inadmissible or the contrary. Now we cannot find anything of the sort here. It is true that the author begins by remarking (p. 23) that it is usual in most ornithological works to place the Vultures first, but they are "the most disagreeable and least intelligent of the race" (admirable and philosophical reasons for deposing them!), and he recognises in the Parrots the "qualifications most fitted to entitle them to take precedence." For them accordingly he constitutes a "distinct order under the quaint but expressive names of Crackers (*Enucleatores*)"—we will not copy the printer's bad spelling of the last word. Many ornithologists will so far agree with Dr. Alfred Brehm, but we read on, and to our surprise find that this order is "by no means limited to the parrots; it includes various other seed-eating birds, chiefly belonging to the passerine tribes, the resemblance of which to parrots has been in some cases generally acknowledged in selecting the names ordinarily conferred upon them. Thus, the Crossbills have long been known in Germany as the Fir-tree parrots, and, on the other hand, the epithet of Sparrow-parrots, applied to some races of climbing birds, clearly shows the relationship that exists between these generally dissevered groups." Yet a few words more are needed before we conclude.

There is Science, and Science falsely so called. It is a rank offence to give the stone of science falsely so called, instead of the bread of true science, and this is what all concerned in the present work must be held to have done, while to back up the imposture by assertions which are palpably or presumably untrue, is an aggravation of the crime, and like all crimes, its commission is a mistake. There is no more erroneous belief than that a book to be popular cannot be scientific, for numerous are the scientific books which have attained to a high degree of popularity. But scientific books are under a heavy disadvantage when they have to struggle for existence amid a growth of specious pretenders. The young beginner full of enthusiasm knows not at first to distinguish the wheat from the tares which surround it. The duty, therefore, of everyone who does know the difference, is to point out and bind the tares for the burning, and in doing this he must not shrink from expressing his opinion of those who sow them. We have heard it said that it matters not how rubbishy a book of Natural History may be, for, provided that it be of such a kind as to command a large sale, it must foster a taste for the subject among the million. This is a most pestilent doctrine. If the tares occupy the ground, how can the wheat grow? and the publication of every book of spurious science precludes the publication of a really scientific book on the same subject.

OUR BOOK SHELF

The Arts in the Middle Ages, and at the Period of the Renaissance. By Paul Lacroix, Curator of the Imperial Library of the Arsenal, Paris. (London: Chapman and Hall, 1870.)

A TASTE for art usually comes to us somewhat late in life, because, in nine cases out of ten, the taste is not cultivated or developed till long after school life. We have, in fact, no regular art education in this country, although endeavours are being made at South Kensington to form Art Schools, and to accumulate art students. A love for high art is certainly more common in France and Italy than in England, and this is partly accounted for by the fact that some education in the first principles of the arts is given in all the government schools and colleges. The work before us is well calculated to foster such tastes. It discusses not alone the principal arts:—"We pass in review," says M. Lacroix, "all the Arts, starting from the fourth century to the second half of the sixteenth. Architecture raising churches and abbeys, palaces and public memorials, strong fortresses, and the ramparts of cities; sculpture adorning and perfecting other arts by its works in stone, marble, bronze, wood, and ivory; painting, commencing with mosaic and enamels, contributing to the decoration of buildings jointly with stained glass and frescoes, embellishing and illuminating manuscripts before it arrived at its highest point of perfection, with the art of Giotto and Raphael, of Hemling and Albert Durer; engraving on wood and metal, with which is associated the work of the medallist and the goldsmith; and after attempting to touch upon playing cards and niello-work, we suddenly evoke that sublime invention destined to change the face of the world—Printing." Although M. Lacroix speaks above of passing in review *all the arts*, we notice at once that he has mixed up the fine and the useful arts, and omitted some of each of them. Moreover, he has chiefly discussed what we call the "decorative arts." Poetry is omitted altogether, and the only account of music is given under the heading "Musical Instruments."

The book itself is gorgeous. It is well printed, and is full of good engravings and woodcuts. Moreover it contains nineteen excellent chromo-lithographs, by Kellerhoven, the most notable of which are the sixth ("Biberon of Henri Deux Faience") and the thirteenth ("The Dream of Life," a fresco by Orcagna). We have no book in our own language which satisfies the want, which must so often have been felt, of a work of this nature. It is a positive art-educator, and what with the appointment of Professors of the Fine Arts at Oxford and Cambridge, and the appearance of a few works of this kind, we may hope before long really to possess in this country some critical taste for all that is beautiful in art.

G. F. R.

Descriptive Travel and Adventures; or Hubert Preston Abroad. By Catharine Morell. Edited by J. R. Morell, formerly one of Her Majesty's Inspectors of Schools. (London: T. Murby.)

WE hope this is not a sample of "The Consecutive Narrative Series of Reading Books," of which it appears to be the 6th volume, for the sake of the unfortunate youths in whose hands they may be placed. We scarcely know a book which we should take greater pains to keep out of the hands of young people eager for knowledge. It is full of the grossest and most palpable blunders. We will quote the three first we came across, giving chapter and verse, as we hardly expect to be believed without affording our readers the opportunity of verifying our quotations for themselves, if they wish to. When we read (Chemistry, p. 83) that "quicklime is simple carbonate of lime taken from the limestone of your mountains!" we thought we had pitched upon a curious slip of the pen; when we found that "marsupials," (which by the bye, are known as being animals *that jump instead of run*) "are peculiar to Australia," and "*the tiger peculiar to the New World!*" (Growth of Plants, p. 173) we opened our eyes with astonishment; and when we were told that *the elephant chews the cud!!!* (The Elephant, p. 197), we closed the book in disgust. Surely any boy on the lowest form of any school which the gentleman who edits the book "formerly inspected," would have set him right on all these points. Seriously, it is very sad that at this time of day it should be found possible to circulate such rubbish under the name of instruction in science. If this is what is to come of inspecting schools, the less we have of it the better, till we have trained up a staff of inspectors acquainted with at least the rudiments of science.

B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Teaching of Science

I AM the principal of a private school. I have long taken an interest in science, and have, by proceeding very gradually and cautiously, succeeded in making Scientific Teaching for a limited time in the week a part of the regular school course. There are many more difficulties, however, in the way of this than some of your off-hand devotees of science seem to be aware of.

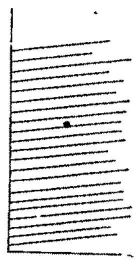
Not the least of these is the decided opposition of some parents, who object altogether to any portion of their sons' time being devoted to that, the direct practical use of which they do not see. My school may be divided into three classes; those who are preparing for one or other of the public schools; those who are preparing for special examinations at Woolwich, and those who will complete their education with me. The first two classes cannot afford to lose any time upon any subject that will not tell directly upon the examinations to which they are respectively looking forward, otherwise the result might be the loss of a scholarship in the one case, and in the other the loss of a place altogether. And how utterly void of all chance Science is, in the former case at least, you can well judge. So that until our Public Schools set us the example, it is very little scientific teaching that we can give to this class of pupils at all events. While with regard to the last class, all that parents wish is that their sons should receive a classical and general education; but any meddling with Science I can assure you some of them look upon as simply an amusement of the master's, obtained at the expense of their sons' time. Nevertheless, as I have already said, I have made Scientific Teaching a regular part of the school course for a short time every week, and am only prevented from extending it much further by the causes named above. The subjects we have taken up hitherto have been elementary physiology and chemistry. For the former we have used Dr. Mapother's "The Body and its Health," while one or two of the elder pupils have gone on to Huxley's "Elementary Lessons in Physiology." And when it is considered that the average age of our pupils is only about twelve or thirteen years, I think this is as much as could be expected. For chemistry, we have been very much puzzled to find a suitable text-book; for though there is no want of really first-class books, we have not been able to obtain what we wish. Both Wilson's (published by Chambers) and Roscoe's "Elementary Lessons" are too long, and (considering the objections of parents named above) too high in price for school boys. What we want is something about half the size and price of either of these, which might be made the groundwork for lectures. As it is, we have been compelled to depend upon lectures alone, illustrated by experiments, in which the pupils themselves are allowed to have as much share as possible. I should be much obliged to any of your readers who could direct me to any text-book likely to meet the want I have indicated. We are now intending to combine with the subject named above a little Natural Philosophy; and I may add, that we have already had, some time ago, a twelve months' course on electricity, with experiments.

I think, however, that the whole subject of Science Teaching in schools wants treating by some master hand; and if some such man as Prof. Tyndall, for instance, who, in addition to the highest scientific attainments, knows something about the practical difficulties of the matter, would enter into the whole subject in your columns, advising what to teach, how to teach it, and what books to use, he would, I am sure, confer a real benefit upon Science. We have heard enough about the *want* of it; we want now to be told by competent authority how the want may be best supplied. And I am able to say from my own personal knowledge that there are heads of schools quite convinced of the importance of the subject, but utterly ignorant how to set about remedying it. Upon this point I wish to be very emphatic, and indeed it was chiefly for the purpose of urging this that I began this letter, knowing as I do well, both from observation and experience, the practical difficulties of the subject; difficulties which are much, very much, greater than your enthusiastic philosophers have any idea of. I hope, however, that if the subject be taken up at all, it will be by some one practically conversant with it, who can give advice which will be worth taking.

M. A.

Forms of Clouds

CLOUDY formations worthy of being noticed have been observed by me during the week. On the 8th I chanced to take a walk with M. Gustave Flourens, who has since been sentenced to death by court-martial. At five o'clock we witnessed many ribboned clouds parallel to each other, and so long that they appeared to radiate from a common focus. These ribboned clouds termicated abruptly just over our heads, and their extremities were n*oalescent*, so that the appearance was one of a feather with the vanes of the quill emanating from one side. The wind was blowing perpendicularly to the vanes and parallel to the quill. This accumulation of matter was evidently owing to the purity of the air on the other side of the singular cloud-edge.



I witnessed again these phenomena on the 9th and on the 10th of March, but not so well. On the 9th and 10th I also observed two solar halos well defined. The halo had a peculiarity of its own. The clouds adjoining the edge were tinged most delicately with violet on the south-easterly side. The evening was stormy and rainy, which is consistent with the theory I have advocated that halos are a prognostic of bad weather.

W. DE FONVIELLE

The Limits of Numerical Discrimination

THE solution of the Problem "how many objects can a man count at once?" is not general, but depends especially on the grouping, position, angular distance, similarity and nature of the objects counted, as well as on the experience and health of the person who counts them.

(This is written under the supposition that the word "count" means "tell the number of," not "begin, one, two, three," which of course cannot be done simultaneously.)

As an example of the operations performed in counting, take the card "ten of diamonds." The player passes his eyes up and down it, recognises it to be the ten, discriminating it from the other cards, calls it by its name "ten," and then, if he likes, can count separately the pips on the card.

This is a case in which the number of the card is recognised as its name, and many others could be adduced in which much higher numbers arranged symmetrically could be recognised at a glance without counting.

A person habituated to counting would divide the objects into groups with which he was best acquainted, in a way depending on their position.

To show that running the eye over the object is not necessarily conscious, or the very operation of counting—If anyone on a fine sunny day looks through a latticed window for some time and then shuts his eyes, he will be able to count a great number of panes in the impression on his retina, or wherever it is. (Compare with this operation that of recognising a person after he has passed out of sight.)

Looking at a collection of objects in counting by groups is governed by the same laws as looking at a single complex object, and naming follows after the object has been properly discriminated. I can imagine that a person naturally gifted with a memory for form of a certain kind, could by practice at once recognise the number of a large quantity of coins scattered at random, inasmuch as the number would be sharply discriminated from the one higher and the one lower, just as a shepherd discriminates sheep, which to other eyes are alike, and if he can discriminate 36 from 35 and 37, there is no necessity for him to count 36 to say that 36 are there. That can be done afterwards. There should be no astonishment that anyone should possess this power, for after all what is it in comparison to the marvellous faculty we have of seeing highly complex objects at once, which we can analyse to a certain extent; but in no way resolve into the elements of the synthesis. The discrimination between red and yellow, between one note and the next, seems to demand much finer powers of the memory; but we are not astonished at it.

The explanation of many wonderful mental and manual feats depends on the same marvellous faculty of apprehending and considering as one that which formerly could be only considered as very many. Reading words, playing a musical instrument (whether with or without notes), writing, tying knots, doing needlework, the manufacture of every useful thing, all are acquired through the same faculty of changing several simple movements into one complex movement, which is treated as one, and can be named as one, even before it is analysed. It is only fair to infer that counting by groups is an art which may be learned, and, if worth the while, carried to a high degree of practical excellence.

Your correspondent "J. B." (March 9) illustrates Dugald Stewart's view by the examples of two beans and two eyes; these do not prove anything in regard to mental attention, but only that they were not both opposite the parts of the retina with which the observer could see most comfortably. If they had only a small but perceptible angular distance, and did not dazzle so as to tire the eyes, what he mentions would probably not have occurred. He could remember them both together and then count them as well as if he were actually looking at them.

Eccles, March 14 R. V.

Books Wanted

COULD you kindly inform me where I could obtain the following works mentioned by Sir John Herschel in his "Discourse on Natural Philosophy," viz.—Bracconot, "Annales de Chimie," and "Dr. Prout's Account of the Experiments of Professor Autenrieth, of Tubingen," Phil. Trans. 1827. My efforts to obtain these books have hitherto been in vain; if you could assist me, I should feel much obliged.

Newbridge, March 12 H. J. WATSON

Quinary Music

YOUR correspondent, Beacon Lough, will find a very effective specimen of this division in the concluding Allegro to the glee "The Gipsy," written by Wm. Reeve. W. R. M.

The Earthquake

THE earthquake, which caused considerable alarm throughout the North of England on the night of the 17th, was felt severely here between 11.5 and 11.10 P.M. The sky, which had been overcast during the evening, suddenly cleared up towards 11 P.M., but was again completely covered at 11.15. The barometer was heaving during the night, but no special disturbance is registered on the photographic curve; the corrected reading at 11 P.M. was 29·887, and 29·885 at $11^h 11^m$.

The rise of temperature was rather sudden just before the passage of the earth-wave, attaining its maximum, 43·4° at $11^h 11^m$, the wet bulb being then 42·4°.

For most of the afternoon the wind was W.S.W., and was changing from W.N.W. to S.W. between 11 P.M. and midnight; at the time of the shock it was due W. It was blowing gently at the average rate of some thirteen miles an hour from the previous midday, and at scarcely four per hour after midnight. At 10 P.M. its velocity was nine miles an hour.

The trace on the magnetic declination curve shows that the magnet was moving rather rapidly from W. to N. when the shock occurred, and a slight irregular movement at $11^h 1^m$ may be due to the earthquake. The magnets were very quiet before 10 P.M., and disturbed from 10 until morning.

The shock was felt very generally throughout the neighbouring villages.

The sound is generally described as that of a strong gust of wind, followed by a noise resembling the passage of an express train over a wooden bridge. This was followed by a very distinct rocking of the furniture, beds, and walls; the whole of the houses seemed to shake violently, and the floors to rise; the rooms swayed backwards and forwards several times. The motion was violent enough to awaken persons from their first sleep. Many thought that part of the building had fallen in, or that something heavy had tumbled down in a room over-head. The rushing sound and crash were followed by a rumbling noise. The motion appeared to begin suddenly, to grow stronger for a time, and then to die away. It was more regular and powerful than the shaking from a heavy waggon in the houses of an old street.

The time the whole disturbances lasted is generally estimated at about half a minute; but this, I should be inclined to think, is excessive.

The direction of the motion is supposed by most to have been from E. to W. S. J. Perry

Stonyhurst College Observatory, March 20

All who are acquainted with the North of England are aware that the districts comprising the counties of Northumberland, Durham, and Yorkshire, are physically divided from that occupied by those of Cumberland, Westmoreland, and Lancashire by a ridge or watershed, formed by the Pennine chain, which is a range of hills averaging 1,700 feet in height, composed of Lower Carboniferous strata; through the centre of which runs the Pennine or "Anticlinal Fault," which has the effect of throwing the strata in a downwards direction to the east and to the west, like the slopes of the ridge of a roof of a house.

To the west of this ridge, in Lancashire, are low undulating plains of Coal measures, and Triassic rocks, much faulted and covered with glacial drift, and in Cumberland and Westmoreland the high mountains and deep valleys of the Lake District intervene between it and the sea. These mountains, composed of Silurian rocks, existed as such, long before the anticlinal fault heaved the Pennine chain into existence, and long before the Oolite strata, forming the high Yorkshire Wolds on the eastern side of the watershed were deposited at the bottom of the sea. In the West Riding the moors have been cut, by the long-continued action of running water, into the deep ravines, or vales, which form so characteristic a feature of that district. It is an interesting question to observe how far this general arrangement of country, and the strata of which it is composed and the dislocations which the latter has suffered, appear to affect the direction, localities visited, and the distribution of the lines of greatest intensity, of the earth-wave which visited the northern counties on the night of the 17th inst. The tract over which it was felt, as far as at present known, would be comprised within a circle, with a centre about ten miles due east of Sedbeigh, the diameter of which would be a line drawn from Dumfries to Doncaster, the farthest limits to the north-west and to the south-east, respectively, to which the earth-wave extended. The greatest effects appear to have been experienced in a belt, about thirty miles broad, running inside this circle, the inner margin running along the towns of Scarborough, New Malton, York, Leeds, and Bradford, Preston, Longridge, Kendal, Penrith, Carlisle, Newcastle, and Sunderland, and thence probably passing out to sea and curving round to Scarborough. The outer margin, or circle before mentioned, runs by the Humber, Doncaster, Manchester, Salford, Roby, Huyton, Seaforth, Southport, and probably for some distance out to sea, Blackpool, west of Ulverstone and Coniston Lake, Dumfries, by the north of Tyneside, to the sea. If this belt be drawn on a map, it will be seen that that segment of the circle which occurs from Sunderland to Scarborough, falls entirely out to sea, and up to the present time the earthquake is not known to have been felt on that coast between these points. It would therefore appear probable that this earth-wave traversed the country in a circular belt, the entire north-eastern segment and the outer margin of the Lancashire portion being beneath the sea; that the area of greatest intensity was near the inner margin, but especially at Preston, Lancaster, Ulverstone, and Blaydon, near Newcastle; that the area in Yorkshire, within this belt, was not entirely free from the shock, as it was slightly felt in Wensleydale and Swaledale, on the eastern slopes of the Pennine chain.

At Preston, where the earthquake occurred at 11.4 P.M. Greenwich time, the motion I observed to be from south-east to north-west; the oscillation was considerable, and the hollow noise, which commenced and ceased with the vibration, resembled express trains running in underground tunnels. The air was close and oppressive, the wind S.W., the night starless and hazy, and the sky from the N.W. to the N.E. covered by a peculiar glare, resembling an incipient aurora, which lasted until 1.30 A.M.

In several places more than one shock is reported to have occurred: thus at Singleton Brook, Manchester, the first shock occurred at 10.56; the second, lasting two seconds, at 11.5; and the third, lasting four seconds, half a second after. Two shocks near together were also felt at Leeds, the second being the sharpest, which was felt at Armley, Headingley, Woodhouse, New Leeds, Chapeltown, and West Bar. Two shocks also ocurred at Kendal, the first at 6.20 P.M.; the second, which was the most severe, at 11.15, lasting twelve seconds, that experienced by myself at Preston lasting about seventeen. From Grasmere also three shocks are reported, the first being at 6.40, and the second and worst at 11 P.M. At Ambleside, the first shock was also felt at 6.30, the true time probably of the two noted above, the second being at 11.3 P.M. At Coniston, a slight shock was felt at 7.0 P.M. on the 17th, and another at 6.3 A.M. on the 18th.

At Hexham, the chief shock is recorded as taking place at 11.15; Ambleside, 11.3; Ulverston, 11.5; Preston (by myself), 11.5; Bowdon, Manchester, 11.4; Singleton Brook, Manchester, 11.4; Newcastle, 11.30; Leeds, 11.15; Penrith, 11.4; Liverpool, 11.15; Kendal, 11.15; from which it will be seen that localities, comparatively near together, often differ more as to the time of occurrence than some of those far apart, and thus there is, therefore, strong reason to believe that these various observations (from 10.30 P.M. to 11.30 P.M.) represent one shock, occurring practically at the same moment over the whole area about 11.5 P.M.

In the year 1786, on August 11, an earthquake which extended over nearly a similar area to the present, like it, slightly displaced the waters of Windermere and the Lake District was felt; and the same district was also visited by an earthquake on Feb. 22, 1867, which was particularly felt on the north shores of Morecambe Bay. It is curious to observe that the *northern* margin of the area of the earthquake, which was felt over the greater part of central and southern England, in 1863, exactly coincides with the *southern* margin of the present, and that the latter, in its course to the north-west, directly crossed the Pennine chain in two places.

H.M. Geological Survey, C. E. de Rance
28, Jermyn Street

On Friday night last, March 17, at 11 P.M., we had a slight shock of an earthquake. I was reading, when suddenly I imagined I heard a carriage and pair drive rapidly up to the house, then rapidly drive on, there being a pause of half a second at least between the two rumbling sounds. After the second sound had continued a second, the house began to shake to such a degree that I rushed out of doors. The only damage done was that all the ceilings on the ground floor show cracks in the plaster. No doubt you will hear more of this from other correspondents. Geo. H. Savage

Nent Head, Alston Moor, Cumberland

Lunar Halos: their Origin and Prognostic Significance

M. W. de Fonvielle is, no doubt, right in attaching importance to the study of these phenomena of refraction, depending as they do, upon the polarisation of atmospheric vapour. Rightly interpreted, they afford one of the most certain indications of weather change, especially in regard to rain; but the popular notion of their being precursors of storms is certainly exaggerated. In reply to the inquiry made by M. de Fonvielle, I may say that the distance between the observers of the singular forms of this phenomenon, and seen on January 4th (and of which a sketch, furnished by me, was inverted by the printer) was about two statute miles. I have ascertained that the two arcs in my own sketch corresponded with the largest and the smallest circles in the drawing of your other correspondent. I saw nothing of the intermediate intersecting arc, almost vertical* in his drawing.

M. de Fonvielle does not, however, remark on the fact that the great circle of 90° had the moon in its *circumference*. Allow me also to add that in my own sketch the apparent diameter of the moon and of the imperfect paraselene are exaggerated; the object of the drawing being to show the relative magnitude and position of the two circles. The innermost circle in both drawings was about 45° to 50°—in fact an ordinary lunar halo. All my observations (for many years) have pointed to the inference, which I may call a law, "*That halos indicate a change of temperature*, and are indicative of transition from dry to wet as well as from wet to dry." I shall be happy to forward M. de Fonvielle further information if desired.

Aigburth, Liverpool, March 20 SAMUEL BARBER

Science in Schools

In the last number of your paper a correspondent, "W.," asks for information respecting "any school adapted for young boys whose parents wish to give them an education embracing the physical sciences and modern languages, on some such plan as that of the Realschule of Germany." Will you permit me to state that the International College at Spring Grove was established with precisely this object, and to a prospectus of this college, which I send you, I would direct the attention of your correspondent. The scheme of science instruction for this college was drawn up by Professors Huxley, Tyndall, and Williamson, and for upwards of four years past has been carried into operation as closely as circumstances permit.

Isleworth W. F. B.

In reply to "W." will you allow me to forward you a prospectus of Craufurd College, Maidenhead, in which an education is given embracing the physical sciences and the modern languages. Having many years ago visited the Realschule of Offenbach, and attended the classes of several of the professors in that school, I have no hesitation in expressing my opinion that a comparison of the merits of the two schools would not be unfavourable to the former.

ANGLICUS

Morell's Geometry

As a considerable part of your number of February 23 is devoted to comments on a little publication just issued by me, "The Essentials of Geometry," I must request you in courtesy to insert these lines in order to set right one misconception. "The Reviewer" (p. 323) passes certain criticisms on the definitions and enunciations, as well as demonstrations, of the book, describing the former as having salient incongruities, and the latter as being nonsense. These are strong expressions, but my present purpose is not to expose the fallacy of the remarks in the review, but to point out the fact noticed in the preface, and overlooked by the reviewer, that all the proofs in the work are taken from French and German sources (p. viii.)

I may add that those sources are the most approved in neighbouring countries, and though I have not given my references in every case, I have done so in so many cases that any person of ordinary discrimination might have inferred that every statement and proof advanced had some high authority for warrant. It is to be regretted that the "Reviewer" overlooked this, for in his haste to condemn a method for which he has an antipathy, he has been betrayed into accusing some even of the leading British as well as foreign geometricians as guilty of salient incongruities, and of writing nonsense. Thus the definition of a plane angle, though condemned by "The Reviewer," is almost identical with that of Dr. Thomson in his edition of Euclid (1835), Def. 7, and the Note to it which runs: "A rectilinear angle is the degree of opening or divergence of two straight lines which meet one another." Nor does Euclid's original definition of an angle differ in conception from that given by me, *γωνία ἐστὶν ἡ πρὸς ἀλλήλας τῶν γραμμῶν κλίσις*; for this word *κλίνειν* contains the notion of revolution, that is, of more or less. Compare my second definition of an angle.—*Essentials*, No. 68, p. 40.

Again, the enunciation and demonstration of the two fundamental theorems of parallels are qualified as sheer nonsense, and yet the whole passage is textually the same as Amiot's, including the parts printed in italics. Further, the proof of the equality of triangles at p. 44, condemned as a violation of the common rules of logic, is based on the previous pages 42, 43, overlooked by "The Reviewer," and agrees almost word for word with Legendre, and absolutely with M. Bos, Professor of the Lycée St. Louis at Paris, and successor of Amiot. (See his "Memento du Baccalaureat es Lettres," 1866-68, p. 183. Partie Scientifique.)

It would take up too much time and space to go further into the matter in dispute, but I wish it to be clearly understood, without denying the right of "The Reviewer" to attack the book in any way that is fair and reasonable, that it is neither one nor the other to make Mr. Morell the object of all the attacks when he is far too honoured in being treated as the substitute for many of the first geometers of the present age on whom the punishment descends.

Every statement and proof in the work has for its warrant some high authority, and the basis of the work and most parts of it to which no special references are given in foot-notes are taken from a digest published by University examiners and Doctors of Science on the Continent.

Now, Sir, as the present letter does not presume to enter on a discussion of the merits or demerits of the work, but is simply an explanation of an essential point underlying the whole question and overlooked by "The Reviewer," I must, as I have said before, request these remarks to be inserted in NATURE to set right the mistake about the authorship and authority of the book.

If NATURE will have the courtesy to give me a little more space on a future occasion, I hope to show on my own authority that I have good arguments for what has been advanced.

March 15 J. R. MORELL

Work and Force

As I hope to hear more of Mr. Highton's arguments at the meeting of the Literary and Philosophical Society of Manchester before this is printed, I will content myself now with noticing but two points.

The first is his attempt to defend himself from the charge of confusing Work and Force; there are other passages in his writings which lead to this somewhat serious conclusion; but the vagueness of the expression "the total of the force used" would suffice to make anyone suspect some such confusion. I presume that a "total of force" is still force, and can therefore be no more equivalent to work than to a time or a space.

The other point is the sentence "this only shows that one of the laws of thermo-dynamics is inconsistent with the doctrine of the mechanical equivalence of heat." If Mr. Highton knew that the first law of thermo-dynamics simply asserted this equivalence he would surely have expressed the proposition differently. As it stands in form it is very much the same as if he had said that one of Newton's laws of motion was inconsistent with the principle that a particle acted on by no forces will move uniformly in a straight line.

If he had known what the laws were, he would hardly have said that they were inconsistent with the very principle which the first asserts, and which the second, as usually stated, involves.

Of course, these lines are not meant as an answer to Mr. Highton's letter, but merely to show that he really does not quite understand the theory he criticises.

March 18 J. HOPKINSON

INVINCIBLE ignorance is said to be excusable. This must be my plea, when I say that I have read over again Sir W. Thomson's paper in the "Philosophical Magazine" of Feb. 1854, and that I cannot see but that it leads to perpetual motion more than anything I have ever written.

H. HIGHTON

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* See NATURE, Jan. 26.

AMERICAN NOTES

WE are again indebted to the early sheets of *Harper's Weekly* for the following:—At a late meeting of the Boston Society of Natural History a communication was presented by Count Pourtales in reference to the character of the sea-bottom off the coast of the United States, south of Cape Hatteras, and based upon the researches of the Coast Survey. According to his statement, the principal constituent of the coast is silicious sand from the coast-line to about the line of one hundred fathoms—a limit which coincides nearly with the inner edge of the Gulf Stream throughout the greater part of its course. Outside of this line is a whitish calcareous mud, containing globigerina, and extending probably over the greater part of the ocean. South of the Vineyard Islands, and to the eastern end of Long Island, the silicious sand is replaced by a kind of bluish mud, known as the Block Island soundings. A similar mud is found off Sandy Hook in a range of depressions known as mud holes, which form a leading mark by which to find the port of New York in thick weather. A few rocky patches are found east of the neighbourhood of New York, and a rocky bottom occurs, sparingly, near Cape Fear; but otherwise the sand is pretty uniform, varying only in the size of its grain. On the inner edge of the Gulf Stream there is a deposit of greensand composed of the cast-off foraminifera.—According to late advices from Florida, Mr. N. H. Bishop, whose visit to that State in the interest of natural history we have already referred to, has started off in his yacht on a cruise down the coast for the purpose of making collections of Florida birds, &c. He hopes to penetrate into the Everglades and prosecute some inquiries in regard to certain species of birds said to occur there, and no where else in Florida.—We regret to have to record the death, at St. Paul, Minnesota, on the 13th of December last, of Prof. William Chauvenet, formerly Chancellor of Washington University, St. Louis. This gentleman has long been known in American scientific circles for his attainment as a mathematician and astronomer; and the various works published by him have occupied a high position as text-books and manuals of instruction. For a time connected with the Naval Academy at Annapolis, he was subsequently elected Professor of Astronomy and Mathematics in Washington University, St. Louis, afterwards becoming Chancellor, and remaining there until 1869. His death occurred in consequence of exhaustion of the nervous powers at the age of a little over fifty.—We regret to learn that in the course of a disastrous fire at Springfield, Illinois, on the 25th of February, the collection of the Geological Survey of the State of Illinois was in large part either destroyed or greatly injured, especially the fossil plants. This loss is the more severe, as the collection in question contained one of the finest series of Carboniferous fossils in the country, and embraced a large number of types of new species described by Messrs. Worthen and Meek. This should be a warning to all who have charge of valuable natural history collections, to give themselves no rest until their treasures are secured in fire-proof buildings.—We recently called attention to the excessive degree of cold to which meteorological observers on Mount Washington have been subjected during the present winter. Since then still greater inclemency has been recorded, during which the thermometer was fifty degrees below zero, while the wind had a velocity of one hundred miles an hour.—We have already, in a previous number, referred to the examination of a locality in California, by Prof. Marsh's exploring party, where numerous fossil trees had previously been discovered; and we learn that a detailed report may be looked for in the April number of the *Journal of Science*. The region in question is situated on a high, rocky ridge in Napa County, California, near Calistoga Hot Springs, and about ten miles from the summit of Mount St. Helena. The ridge itself belongs to the Coast Range series, and forms the division between the Napa and Santa Rosa valleys. It is about two thousand feet in height, and is composed of metamorphic rock of the cretaceous period, overlain unconformably by later tertiary strata, consisting of light-coloured, coarse sandstone, and beds of stratified volcanic ashes. A careful examination showed that the trees on the surface of the ground had been weathered out of the volcanic tufa and sandstone, and consequently were of the Tertiary age; and also that there remained still embedded in the volcanic tufa, &c., an extensive forest of very large trees, stretching over a great area. Some of the trees were of great size, a portion of one having been traced for a length of sixty-three feet, with a diameter of seven feet near its smaller end. Another tree indicated an original diameter of not less than twelve feet. All were prostrate, and had apparently been thrown down by the volcanic current which covered them. Many were much decayed internally and worm-eaten before they were buried. All of the wood was silicified, probably by means of hot alkaline waters containing silica in solution—a frequent result of volcanic action. A careful examination of the wood obtained at this locality showed no essential difference in structure from that of the modern redwoods of California (of the genus *Sequoia*). No other fossils were met with, which rendered it somewhat difficult to fix the precise epoch; but it is considered probable by the Professor that the trees belonged to the Pliocene period. The origin of the volcanic material which covered the forest could not be ascertained, although it was supposed to have been derived from Mount St. Helena, which is the nearest volcanic peak.

THE SCIENCE AND ART DEPARTMENT

WITH reference to examinations in large towns, the Department had previously issued the following regulation:—"In large towns or populous districts where there are three or more schools, and where numerous examinations are to be held, the Science and Art Department *may at its discretion require* a special local secretary to be appointed to manage the whole of the examination business. The Department will correspond with him alone on all subjects connected with the examination. He will be allowed a fee of ten guineas, and an extra fee of half a guinea for each night on which an examination is held."

The Department has now determined to place the conduct of these examinations as far as possible in the hands of officers appointed by the School Boards with the approval of the Science and Art Department. In such cases the School Board would determine, in concert with an Inspector from the Science and Art Department, the centres at which the examinations were to be held. They would appoint officers, one of them as special local secretary, with such a staff of assistants as would secure the presence of at least two, or if the number of candidates were very large, more officers at each examination. The examination papers would be sent to the officers appointed by the School Boards as they are now to the Local Committees. While these officers would be responsible for the conduct of the examinations, it would be expected that a few members of each of the Local Committees would visit the examinations and satisfy themselves with regard to the pupils of the classes they superintend. The same payment will be made to the special local secretary appointed by the Board as is now made to the special local secretary elected by the Committees; and they would make a payment to each assistant of 10*s.* for each night he was required to attend.

With regard to the number of Science Schools in which no fees are charged, or in which they are merely nominal, the Department thinks that the schools cannot be considered in a wholesome condition when students, a very large proportion of whom are adults in the receipt of wages, obtain their instruction wholly at the cost of the State and without any pecuniary contribution on their part. Nor is it probable that they will value as they ought what is given gratuitously. The directions in the Science Directory are very plain on the point. They state that "the payment of fees by the students can be looked upon as the only solid and sufficient basis on which a self-supporting system can be established and supported. Though the Department does not consider it necessary at present to lay down any rules making the payment of fees an absolute condition of the grants on account of Science instruction, yet as the payments from the State must be expected to diminish, and as aid on account of those persons who do nothing for themselves cannot be justified, committees of schools and classes and teachers are strongly urged (should it at present not be the practice) at once to impose as high a scale of fees as they consider can be raised not only on middle class students but also on artisans." The Department desires to call the serious attention of the Committees of Schools to this instruction where fees are not imposed. They find that in some places not only is there an entire absence of fees, but that there has even been an unseemly competition on the part of teachers to get students by any means to join their classes with a view of earning the payments on results. They therefore give notice to the Committees of Schools that unless they themselves take steps to remedy the present evils by imposing at least some small fees, which should be paid to the Committee direct, it will be necessary to reduce the amount of the payments on results. They have no wish to reduce the payments on results at present, and they would avoid as long as possible the imposition of new conditions which necessarily complicate the system of aid, and render the rules burdensome and difficult to work under, but the want of proper vigilance on the part of the Committees may render this step necessary.

AUGUSTUS DE MORGAN

THOSE readers of NATURE who are in the habit of examining the obituary column of the *Times*, will have regretted to see, on Monday morning last, the announcement of the death of the eminent mathematician, Augustus De Morgan. He had been seriously ill for the past two years. A disease of the kidneys, complicated with other disorders, had reduced him to a shadow of his former self, and rendered him incapable of any protracted exertion. This was the more trying as his mind retained all its former energy, and the doctors forbade his reading more than an hour or two in the day. He was, however, allowed to see his friends, and often amused and instructed them by the hour together from the stores of his extraordinary memory. During the last few weeks he had become considerably weaker, and on Saturday the 18th, at one o'clock in the afternoon, his spirit was released from the body which for so many months had been only a burden to it.

Augustus De Morgan was the son of a Colonel in the Madras army. He could trace his descent from the mathematician, James Dodson, author of the "Anti-Logarithmic Canon." He was born in the summer of 1806, in Southern India. While yet a school-boy, he showed his taste for mathematics by filling thick notebooks with "infinite series," which he interspersed with grotesque figures and quaint faces. In 1823 he went to Cambridge, where he entered at Trinity College; his rooms were in the south-east corner of the great court, then called "Mutton Hole Corner," which he affirmed was a contraction from Merton Hall Corner.

In the tripos of 1827 he was Fourth Wrangler, but he never proceeded to the degree of M.A., owing to his objection to subscribe to the tests, and it is sad to think that the same conscientious scruples debarred this illustrious man from a Fellowship. On leaving Cambridge he entered at Lincoln's Inn, and would have forsaken Mathematics for the study of the Law, but that in 1828, the London University, now University College, was founded, and he was offered the Professorship of Mathematics there, which he accepted, and remained a firm supporter of the College and its principle of no tests till the year 1866, when the Council, in making an appointment to the chair of Logic and Mental Philosophy, refused, as the Professor believed, one of the candidates on account of his religious opinions. Prof. De Morgan remonstrated, but his remonstrances were disregarded. He then thought it his duty to inform them that he must forsake the College if the College forsook its principles. But the Council turned a deaf ear; and Prof. de Morgan, who had for nearly forty years been the chief honour and ornament of their institution, left them, and, we are informed, never afterwards entered their gates.

To estimate the energy of the Professor we must look at him not only as a teacher of mathematics, but as a mathematician, an actuary, a logician, an historian, a biographer, and a bibliophile. First, then, as a teacher of mathematics perhaps no man has been more successful in training distinguished mathematicians. Amongst the latter we may mention the names of Prof. Clifton, Judge Hargreave, Mr. Routh, and Mr. Todhunter. Prof. Sylvester also attended his lectures, though the relationship of professor and pupil did not in this case last very long. He had a method of interesting his hearers in the subjects on which he lectured, and of making them love mathematics for its own sake, which few other men have ever attained to. He expended a great deal of work upon his classes. The subject-matter of every lecture which he delivered was entered in a note-book and sent into the library of the college for the benefit of his pupils while writing out and expanding their own notes.

As a mathematician, his work was so various that it is difficult for any one man to review it, and it would be out of place to attempt anything of the kind here; but we may allude in passing to his double algebra, which was certainly the forerunner of Quarternions, and contained the geometrical interpretation of the symbol $\sqrt{-1}$. Sir W. R. Hamilton, in the preface to his Lectures on Quaternions, p. 41, says, "But I wish to mention that among the circumstances which assisted to prevent me from losing sight of the general subjects, and from wholly abandoning the attempt to turn to some useful account those early speculations of mine, on triplets and on sets, was probably the publication of Prof. De Morgan's first paper on the 'Foundation of Algebra,' of which he sent me a copy in 1841."

As a writer of mathematical text books, he took the highest rank, his books being more suitable, however, for teachers than for pupils. They were characterised by extreme clearness, exhaustiveness, and suggestiveness. Perhaps those best known are his "Elements of Arithmetic," published 1830; his "Elements of Algebra," published 1835; and his "Differential and Integral Calculus, with elementary illustrations," which is a perfect mine of original thought, and in which some of the most important extensions which the subject has since received, are distinctly indicated, and it was published by the Society for the Diffusion of Useful Knowledge.

As an actuary he occupied the first place, though he was not directly associated with any particular office, but his opinion was sought for on all sides, by actuaries, on questions connected with the theory of probabilities as applied to life contingencies. In 1838 he wrote his "Essay on Probabilities," which still retains a high place among the literature of insurance offices.

As a logician he was well known, and his "Formal Logic," together with the Treatise of Mr. Boole, may be said to have created a new era in logical science. His controversy with Sir William Hamilton will long be remembered.

As an historian and biographer, the English *Encyclopædia* says of him that "he had a great affection for, and an extensive and minute erudition in, all kinds of literary history, biography, and antiquities." He was one of the most extensive contributors to the *Penny Cyclopædia*, many of the articles of scientific biography having been written by him, as well as most of the mathematical and astronomical articles. The lives of Newton and Halley in Knight's "British Worthies," were also from his pen.

As a bibliophile, his "Arithmetical Books from the Invention of Printing to the Present Time, 1847," and his "Budget of Paradoxes" will long remain celebrated. He was the possessor of a very large collection of old mathematical works.

In addition to this the Professor contributed largely to the *Philosophical Magazine*, the *North British Review*, the *Athenæum*, and the Transactions of the Cambridge Philosophical Society, in which he published most of his original investigations. He wrote a "Book of Almanacs," with an Index of Reference, by which the Almanac may be formed for every year up to A.D. 2000. with means of finding the day of the new moon from B.C. 2000 to A.D. 2000. He was also secretary and member of the Council of the Royal Astronomical Society for many years. He and his son, George De Morgan, also a mathematician of great promise, whose untimely death will be remembered, took the most active part in the foundation of the London Mathematical Society, of which he was the first president. Prof. De Morgan will be buried on Thursday, the 23rd, at Kensal Green, but his memory will long be cherished among a large circle of attached and admiring friends.

ARTHUR C. RANYARD

PAPERS ON IRON AND STEEL

IV.—THE BESSEMER PROCESS (*continued*).

THE magnificent shower of sparks which accompanies the turning over of the converter is easily explained. The blast has, of course, to be maintained during this turning over, until the whole of the melted material is clear of the openings through which the blast is forced. As these cover a considerable area at the bottom of the converter, the edge of the liquid passes them successively, and at the moment of thus passing the blast cuts the surface of the melted matter almost horizontally. But what is this melted matter? It is a pool of iron, on the top of which is floating a thick scum of silicate of iron, &c.—the "cinder." I use the term "silicate of iron" only in an approximate sense, as I doubt whether the silica is completely oxidised.

My reasons for doubting it are that the particles which are driven out of the converter by the blast are, to some extent, explosive, they are seen to burst with brilliant coruscations which are partly due to further oxidation; and when the granules which shower upon the floor are examined in the microscope, they present a very curious appearance. They are minute hollow spherules, miniature bomb-shells, varying considerably in diameter from one-tenth of an inch to one-fiftieth and less in diameter. The largest are more or less broken, commonly of a basin shape, shown in Figs 1 and 2.*

The smaller spherules are for the most part perforated. My friend, Mr. Joseph Bragg, who has carefully examined these, and to whom I am indebted for the drawings from which the engravings are copied, says, "I can hardly satisfy myself that any are quite without apertures, though some have no distinct round holes as most have, but in these cases there are minute openings between and under the welded scales or plates which often cover the spherules, giving them a rough surface." Conglomerate groups of these spherules, such as are shown in Figs. 3* and 4, are very common, and some are attached to irregular lumps of cinder, as shown by the right-hand fragment on Fig. 3. A few are pear-shaped (see Fig. 4). On the right of these pear-shaped specimens are shown some of the smaller spherules in which the perforations are less evident. In the smallest, as the agglomerated and attached specimens (Fig. 3), the perforations are very obscure or doubtful.

Sir Samuel Baker, in his "Nile Tributaries of Abyssinia," describes some natural products due to a similar action on a vastly larger scale, viz., the volcanic eruption of a flood of gaseous matter through fused silicates. He says, "Rows of broken hills, all of volcanic origin, broke the flat plain. Conical tumuli of volcanic slag here and there rose to the height of several hundred feet. We entered a dead level plain of orange-coloured sand, surrounded by pyramidal hills; the surface was strewn with objects resembling cannon shot and grape of all sizes, from a 32-pounder downwards; the spot looked like the old battle-fields of some infernal region. . . . I dismounted to examine the Satanic bombs and cannon shot. Many of them were as perfectly round as though cast in a mould, others were egg-shaped, and all were hollow. With some difficulty I broke them, and found them to contain a bright red sand; they were, in fact, volcanic bombs that had been formed by the ejection of molten lava to a great height from active volcanoes; these had become globular in falling, and having cooled before they reached the earth, they retained their forms as hard spherical bodies precisely resembling cannon shot. The exterior was brown, *and appeared rich in iron*. The smaller specimens were the more perfect spheres, as they had cooled quickly, but many of the heavier masses had evidently reached the earth when only half solidified, and had collapsed in falling. The sandy plain was covered with such vestiges of volcanic action, and the infernal bombs lay as imperishable relics of a hailstorm such as may have destroyed Sodom and Gomorrah" To a Lilliputian traveller about an inch and a quarter high the floor of a Bessemer shop would present about the same aspect as this volcanic plain presented to Sir Samuel Baker, and would appear on about the same scale relative to the traveller's own dimensions.

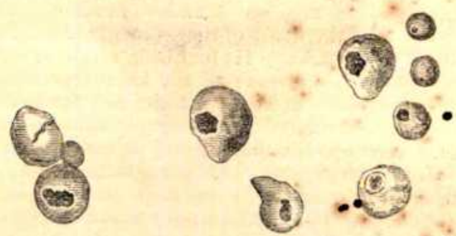

FIG. 4.

It may have been remarked that in the above I have never used the word "slag," which in chemical works is usually applied to the separated silicate of iron, &c., however it may have been separated. I have called it "cinder," in accordance with the nomenclature of the workshop, for in the use of these terms, slag and cinder, the workshop is more learned than the University, even in the matter of etymologies, which occupies so absorbing an amount of University attention.

Whenever the silicate is separated by fusion or the direct action of the fire he calls it "cinder," when it is squeezed out from a bloom or pile by the blows of the hammer he calls it "slag." Now the Scandinavian name of *fire* refuse or dross is *sinner*, the German for the same is *sinter*. The Scandinavian for a blow is *slag*, the German *schlag*. I have observed with much interest the constancy with which the workman adheres to the strictly etymological signification of these words, while learned writers utterly confound them. Of course the workmen are unacquainted with their origin, nor have I ever seen their distinctive etymologies pointed out by anybody else. They afford an interesting illustration of the technical continuity of modern English with its ancient Scandinavian basis. Our metal workers, like our sailors, still speak the strong tongue of the old Norseman. There are scientific as well as etymological reasons for the distinction between cinder and slag, and therefore I adopt the workmen's phraseology.

W. MATTIEU WILLIAMS

SCIENCE IN GOVERNMENT WORKSHOPS

THERE seems to be a singular antagonism between science and officialism. The Government has undertaken more than one special manufacture, and not without a certain measure of success, but even the best of Government factories are tainted with some perverse defiance of scientific principles. Why this should be is too large a question for present discussion, but the fact is beyond doubt. Take, for example, one of the most effective seats of national manufacture—the small-arm factory at Enfield, which is under the command of an officer who may fairly be credited with scientific intelligence. You will see there a considerable amount of what may be called imported science. Outside inventions in machinery, in rifle barrels, in locks, in breech-actions, and other branches of the work, have been appreciated, adopted, and improved, and this is so far good. The Snider is a very clever makeshift, and perhaps as good a converted rifle as could have been made out of the old Enfield. The projected Henry-Martini, again, has an excellent breech-action, though not quite the best that might have been selected. Its barrel is on a good and tried pattern, although one that has not very successfully competed with the Metford. In these respects, the design cannot be called unscientific, but it is said that one essential element of the new rifle—the sighting—if not absolutely left to be fixed by tradition and routine, will be in principle little better than the worthless sighting of the old Enfield. If a crack shot were offered the choice between a first-rate barrel with clumsy and unscientific sights and an inferior barrel fitted with perfect sights, he would certainly prefer to enter into a competition with the latter weapon. Errors from defective aim are, as a rule, much larger than those due to imperfections of rifling, and to fit a first-rate weapon with bad sights is to throw away nearly all the skill and money which has been expended upon it. This is just what the people at Enfield are doing now, and all for want of familiarity with one of the simplest maxims of geometrical science. When a mathematician, an astronomer, an engineer, or even a superior artisan, wishes to determine with accuracy the position of a point, he almost invariably does it by setting off its distance from each of a pair of rectangular co-ordinate axes. In very special cases and for very special reasons the advanced geometrician will occasionally employ oblique instead of rectangular axes; but whenever it is practicable, whether he is dealing with linear or angular measure, he uses as a matter of course rectangular axes. In a case where he has to measure independent variations in horizontal and vertical directions, he would think it simply absurd to refer the position of a point to any other than a pair of horizontal and vertical axes of co-ordinates. Thus the astronomer has his co-ordinates of azimuth and altitude, of latitude and longitude, the builder works with his plummet and square, and the most simple-minded carpenter, who wished a nail put in a particular spot on a wall, would order it to be driven in at so many feet from the floor, and so many feet from the side of the room. This elementary scientific method has in fact descended to so low a stratum of intelligent society that most people have assimilated it as if by instinct, and would open their eyes rather widely if they were told that when they practised it they were obeying the dictates of science. And yet, strange as it may seem, this extremely elementary, almost axiomatic, idea of rectangular co-ordinates, has not yet penetrated to the Government factory at Enfield. Consider what the sights of a rifle are for. In the simplest case, when you are aiming at an object the distance of which is known, and when there is no wind, you have nothing to do but to adjust the sights to the right elevation, and align them upon the object. But if your first shot falls low or high, or if you want to hit another object at a different distance, you must do one of two things, either slide your backsight up or down or else take a fuller or finer sight. In other words, you must correct the error in elevation, either by mechanical adjustment or eye-adjustment in the vertical direction. Practically, large and occasional changes are made by mechanical adjustment; small and frequent changes by eye-adjustment. So again, if there is wind to allow for, you must either give the sights an apparatus for lateral adjustment (which, of course, would be quite inadmissible in a military arm), or, you must make the necessary allowances by eye-adjustment in the horizontal direction. The occasions which require vertical and horizontal corrections are quite independent of each other, the one class being functions mainly of distance, and the other of lateral wind. This is, therefore, precisely the case where the position of the sight should be referred to vertical and horizontal lines.

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* Figs 1, 2, and 3 will be found in last week's number of NATURE, p. 389

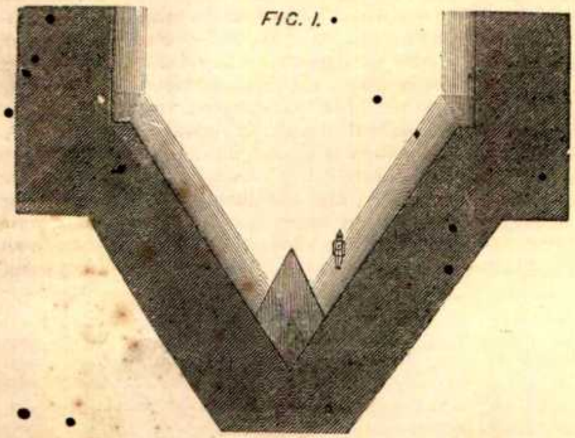
FIG. 1.

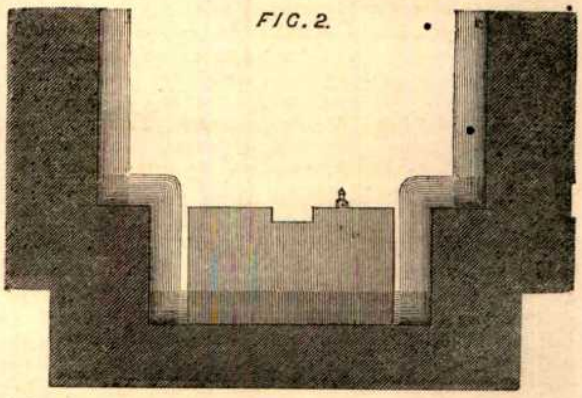
FIG. 2.

Instead of doing this, our Government manufacturers give you a backsight, bounded by two oblique lines at a certain inclination to the vertical, and they provide you with a foresight, bounded by two other oblique lines at a different inclination to the vertical. When they indulge in a great effort of imagination they sometimes dream of getting rid of the oblique lines on the backsight, still leaving the foresight as bad as ever. Anything more perversely unscientific can scarcely be imagined, though we daresay it will be stoutly defended on the plea that it is the same venerable system which contributed to the non-efficiency of Brown Bess.

To any mechanician, however humble, it must be quite clear that both backsight and foresight ought to be bounded by horizontal and vertical lines. And here we might close the subject but for the stupendous power developed by the military mind, of pooh-poohing suggestions which have only science and reason to recommend them. "Extremely ingenious and scientific, no doubt," our soldier critic may say, "but shows a woeful ignorance of the practical conditions of the problem. You see our recruits are not philosophers, but rough fellows, and we must give them something easy to understand and handle, and capable of bearing rough usage. Experience has shown that there is nothing like the old V backsight, and knife-edge foresight, with which the British soldier can go anywhere and do anything, &c. &c. You scientific people always forget to look at the practical side of things."

Now that is just what scientific men don't forget. No man can be really practical unless he is also in his measure scientific. But in this matter of sights it may be worth while to follow the advice of our imaginary soldier, and look more minutely at the practical side of the question. What is the test of a good sight? Obviously the best sight is that which enables the rifleman most easily to judge whether two successive aims are alike, or if they differ, to say in what direction and to what extent they do so. For this reason the soldier is taught to discriminate between fine sights, full sights, and half sights, according to the quantity of foresight which he allows to be visible above the bottom of the V. What he sees is an irregular lozenge, and the accuracy of his shooting, so far as elevation is concerned, depends upon his judging with perfect exactness the length of the vertical diagonal of this lozenge. Again, if wind has to be allowed for, he must fix in his mind the apparent distance at which the tip of the foresight should be seen on the right or left of the object. The difficulty of these eye-adjustments is enormously increased by giving him nothing but inclined datum lines from which to estimate. The language in which it is customary at Hythe to inculcate the method of making allowance for wind, curiously illustrates the absurdity of the received sight-pattern. The recruit is told that he must "aim at nothing to hit something," a phrase really very happy as a description of the difficulty of estimating at the same time elevation and lateral allowance with a Government rifle. The most practised shots find it by no means easy to vary their horizontal allowance for wind without in some degree altering their elevation, and all this difficulty is quite gratuitous.

The true test of the value of a system of sighting is of course to be applied rifle in hand, but our pictures will show tolerably well what the result of such a test would be. Figure 1 is what a man sees when he takes an aim with the present military sights. It is meant for a half-sight, the lozenge in the middle being the portion of foresight visible, and the shading being as near a representation as we can give of the haze which always more or less blurs the outline of the backsight. We have supposed a little wind, and the object is therefore seen on one side of the sight, and is as nearly level with the tip of it as it can be brought without the guidance of any horizontal line. Now suppose the rifleman finds that sight correct, and wishes to shoot down a second enemy at the same range and under the same conditions. He will try to take just the same sort of aim again, and Fig. 3 is a pretty good approach to it. Probably the reader will have to look backwards and forwards rather carefully before he can judge whether there is any, and how much, discrepancy between Figs. 1 and 3, and if (as would be the case in actual practice) Fig. 1 had gone out of existence say only a few minutes before Fig. 3 was looked at, we doubt very much whether the error would be large enough to be discerned at all, and we are sure that its amount would not be correctly estimated.

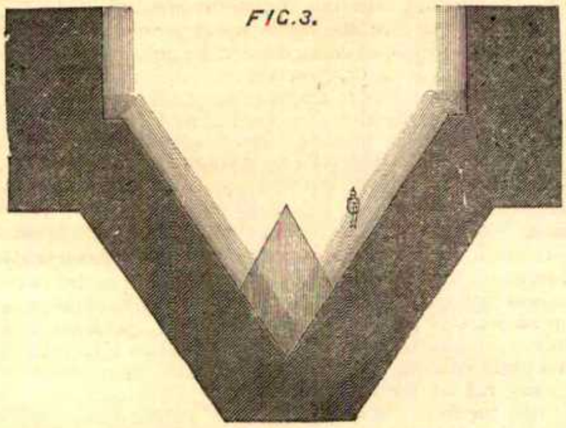
FIG. 3.

Now, take the case of rectangular sights such as are drawn in Fig. 2. In this, as in Fig. 1, the elevation is supposed to be normal—that is, with the horizontal top of the foresight level with the top of the backsight below the haze. The allowance for wind is exactly the same as in Fig. 1, and its amount may be fixed in the memory by noting that the object is about twice as far from the outer edge of the foresight as it is from the little rectangular notch which marks the middle of it. Having looked at this, it would be quite possible to go away for a day, and then return and look at Fig. 4, and see at once that this latter differs from the former by being a considerably higher sight with a much larger allowance for wind. No error anything like as gross as this could be made even by the poorest shot in two successive aims. And yet the real differences in elevation and horizontal allowance between Figs. 2 and 4 are not larger than those between Figs. 1 and 3, which cannot be detected or remembered without the greatest difficulty. And this is the result simply of substituting rectangular for oblique sights.

"All very pretty," says our soldier, again, "but how are you to get such a sight as you have drawn, on a rifle meant to carry a bayonet and to be used in war?"

We are fortunately able to answer this question. Figs. 2 and 4 are taken from the actual sights of a rifle prepared as follows—A London armoury rifle of Government pattern was taken; the knife-edge of the foresight was filed clean off, leaving the block, and then a small rectangular groove was filed longitudinally in the middle of the block, thus leaving the foresight less liable to injury than before. Then the sliding-bar was turned upside down, and a square notch filed instead of the triangular V.

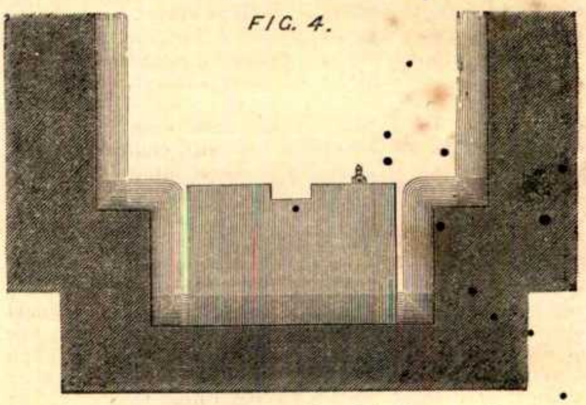
FIG. 4.

The rifle so altered was rather fitter for rough work than before, and it was possible to aim with it and to make exact allowance for wind into the bargain. The use of it, too, was incomparably easier for a recruit to learn. We may add that the result of actual shooting with it has been quite conclusive in its favour. And yet the beautiful and expensive Henry-Martini is now being turned out with sights which neutralise one half of the accuracy of the weapon by making accuracy of aim impossible.

It will cost at least 1,000,000*l.* to arm our troops with Henry-Martinis in lieu of Sniders, and on a moderate computation one-third of the gain in accuracy (which may be represented by one-third of the cost) is utterly thrown away by the defective sighting.

If these things were not ascertained facts, it would be hard to believe them possible. Is it even yet too late to stop the mischief? G. W. H.

NOTES

Mr. George Biddell Airy, Astronomer Royal, has been selected by the Council of the Royal Society as a fit and proper person to be nominated as President of that Society on the occurrence of a vacancy, and Mr. Airy has declared his willingness to accept the proffered honour. As General Sir E. Sabine, the present president, has intimated his intention of resigning office at the next annual meeting, the election, which rests with the fellows, will come on at that time. We may congratulate the Society and English science generally on the wise choice which the Council have made. In scientific distinction, administrative ability, and honesty of purpose, the president-elect will prove no unworthy successor of the distinguished man of science who has now for so many years filled the office.

The Royal Irish Academy has approved of the following allocations from the fund for promoting scientific researches:—G. J. Stoney, F.R.S., 50*l.*, "For Researches on the interrupted Spectra of Gases;" Prof. R. S. Ball, 6*l.* (additional), "For Experiments on Vortex Rings;" Prof. Henry Hennessy, F.R.S., 20*l.* (additional), "For Experiments on the friction of Fluids in contact with Solids;" Prof. Thiselton Dyer, 29*l.*, "For Researches on Vegetable Physiology;" Prof. Traquair, M.D., 25*l.*, "For Researches on the Crania of Osseous Fishes."

It is stated that the appointment of Professor of Chemistry and Experimental Physics at the Indian Engineering College will be filled up by the transfer of Mr. Herbert M'Leod from the College of Chemistry. We consider this arrangement one which reflects great credit on those who are responsible for it. Mr. M'Leod has long been known not only as an accomplished chemist, but as one who has the rare gift of imparting his knowledge to others, and he has filled the position he held at the College of Chemistry for many years.

We understand that the names of Dr. Cunningham and Mr. E. Ray Lankester have been selected from the candidates for the vacant Professorship of Natural History in the Queen's College, Belfast, and that it is probable that His Excellency the Lord-Lieutenant will appoint the first named gentleman as successor to Prof. Wyville Thomson.

We are glad to learn that the "Zoological Research Association," concerning which we some weeks ago inserted a note, is now fully established.

We regret to have to record the death on the 7th inst., at the age of sixty-eight, of Mr. Henry Denny, the curator of the Leeds Philosophical and Literary Society, an office he had held since 1825. It is to Mr. Denny's untiring zeal and assiduity that the Leeds Museum owes its high position among provincial institutions of a similar character. Unwearied energy, a great knowledge of science, and unfailing courtesy, were combined in him in a manner which will render it very difficult for the society to supply his place. In addition to the title which he held as Associate of the Linnean Society, Mr. Denny was a corresponding member of the Academy of Natural Sciences of Philadelphia and of the Syro-Egyptian Society of London; honorary member of the Philosophical Society of Dickinson College, Carlisle, Pennsylvania; of the Yorkshire Philosophical Society; the Blackmore Museum, Salisbury; and of the Norfolk and Norwich Museum.

There are few more zealous cultivators of Astronomy than an Indian gentleman, Mr. Nursing Row, a friend of the late Admiral Manners, who has built an observatory at his own expense at Vezagapatam. Although he has recently suffered a heavy loss of property from a cyclone sweeping over his estate, Mr. Nursing Row sent the munificent donations of 100*l.* to the Mansion House Fund for the relief of the distress in Paris, and 100*l.* to the fund for supplying seed and other aid to the French peasantry. He is also a most generous benefactor to the poor in his own neighbourhood.

We learn from Paris that the report which reached the Academy of the death of M. Becquerel, the electrician, and which we copied last week, is incorrect. It was his son, M. Dumeril Becquerel, who died during the investment of Paris.

Dr. Page, of Edinburgh, has commenced a course of thirteen lectures on Geology, descriptive and industrial, in the Mechanics' Institution, Glasgow. The lectures are largely attended by ladies and gentlemen. The study of the science in this large commercial city has received new impetus from the attractive and eloquent manner in which the lecturer presents it. Few, indeed, have the gift which Dr. Page possesses of making scientific study popular. The directors of the Institution are indebted to Dr. Page for enabling them to offer such valuable instruction at so small a charge.

The Council of the Pharmaceutical Society offers a silver medal for the best herbarium collected in any part of the United Kingdom between May 1, 1871, and June 1, 1872. The collections are to consist of flowering plants and ferns, arranged according to the natural system of De Candolle, or any other natural method in common use. No candidate will be allowed to compete unless he be an associate, registered apprentice, or a student of the Society, or if his age exceeds twenty-one years.

We have great pleasure in calling attention to the letters which will be found in our columns of correspondence respecting the International College at Isleworth, and the Crawfurd College at Maidenhead, where instruction in science appears to be given with highly creditable results.

At the last meeting of the American Academy, a new form of solar eye-piece was exhibited by Prof. Pickering. A cube is made by cementing together two right-angled prisms of glass, which is substituted for the reflector in a diagonal eye-piece. By using a cementing liquid whose index of refraction is very nearly the same as that of the glass, almost all the light and heat pass directly through the cube, enfeebling the image so that it can be borne by the eye with impunity. We can thus cut off as much of the light as we please, and yet avoid all danger of cracking the glass by the heat, as frequently happens when a coloured glass is used to absorb it. Since the relative index of refraction of the inclined surface is very nearly unity, the angle of total polarisation is 45°, or the actual angle of incidence. Accordingly, the image will be perfectly polarised, and may be varied in intensity at will by a Nicol's prism. If we make the angle of incidence equal to 45° in Fresnel's formula, it takes the simple form that the reflected light $= \frac{\sin^2 v}{\cos^4 v}$ in which v is the deviation of the ray in passing from one medium to the other. In the present case if $n = 1{\cdot}01$, we find that the reflected beam equals ·00005, or only one twenty-thousandth part of the intensity of the incident ray, if $n = 1{\cdot}001$, the light is diminished to one two-millionth. On trying the experiment, it was found that the image formed was coloured, the tint changing with the angle of incidence. This curious effect is probably due to the fact that the dispersion of the balsam used for cementing the prisms is greater than that of the glass, and hence the relative index and quantity of light reflected differ for different colours. Apart from its practical application, this instrument possesses a scientific interest as furnishing a means of making large plane surfaces whose index is nearly unity, and thus enabling us to verify laws for whose proof we have heretofore been dependent on observations with gases only.

A slight shock of earthquake was felt on Friday night in the North of England. In and near Manchester the shock was felt soon after eleven o'clock. At Singleton Brook, near Manchester, the first shock was felt at precisely six minutes to eleven, A resident says that the windows of his house were violently shaken, as though a heavy vehicle was passing along the road. About five minutes past eleven the noise was again heard, accompanied, as before, by a tremulous motion. This time the effect was much more marked and continuous. At first the impression produced was merely that of trembling, which lasted for perhaps two seconds. This was succeeded by a slight pause of about half a second, and then the beds were distinctly felt to roll from side to side, exactly like the heaving of a ship at anchor, and with the same sharp and sudden check to the motion. The time occupied by the second shock was about four seconds. Immediately before the first shock a heaviness in the atmosphere had been noticed, as if there were a sudden change in the temperature. A similar effect was produced, according to concurrent testimony, in the neighbourhood of Bowdon. A decided shock was experienced at Leeds, and from accounts received from York, Wakefield, Doncaster, and other places, it seems to have been pretty generally felt over the southern part of Yorkshire. The accounts obtained from a variety of sources in Leeds show that it was felt in nearly every part of the borough. The statements vary slightly as to the exact time of the occurrence, but it must have taken place from eleven to a quarter-past. There were two motions, the first being very slight—so slight that no notice would probably have been taken, but for a more decided one which followed, and the result of which was that windows were violently shaken in their frames, and in some houses articles of crockery displaced. At Ulverston and Lancaster the shock was felt. Furniture in houses was thrown over, and people shaken and considerably alarmed. Two distinct shocks were experienced in the neighbourhood of Kendal; the first at about 6.20, the second at about 11.15. The first was not violent, and it does not appear to have been generally noticed, but the second was everywhere felt, and created great alarm. It lasted about ten or twelve seconds, and was felt at a distance of sixteen miles from Kendal, on the borders of Yorkshire. It appears to have taken a south-westerly direction. Several witnesses state that at the time of the occurrence they observed an unusual swell in the waters of Windermere lake. During the evening the atmosphere was very calm and foggy, like that preceding a thunderstorm. At Preston the shock was very keenly felt, and it created very much confusion and alarm. In our Correspondence columns will be found several other letters descriptive of the unusual phenomenon.

M. DECAISNE, director of the *Jardin des Plantes* at Paris, writing to the *Gardener's Chronicle*, thanks the English horticulturists for their offers of assistance, and asks especially for contributions of Pandanaceæ, Nepenthes, Cyclantheæ, Orchids, and Ferns. "But," he asks, "who will restore to us the Malpighiaceæ which our illustrious predecessor, Adrien de Jussieu, got together with so much pains? Who can give us back the old plants which were deposited here by such men as Aublet, Commerson, or Du Petit Thouars? For many years our stoves must bear the traces of these cruel losses. The effect," he adds, "of the shells on the monocotyledonous plants was very singular, and different in different cases. Thus the Pandanaceæ, Cyclantheæ, and Dracænas had their leaves and young stems completely cut off by the explosions of the shells, while the Bromeliaceæ were uninjured alike by the concussion and by the cold, which destroyed contiguous plants of other families."

WE learn from New Zealand that a fine display of Aurora was seen in that colony on the 24th and 25th of October last. It is not a little remarkable that while on the 24th Lyttleton was almost totally burnt down and the colonial papers attributed the blood-red appearance of the sky to the reflection of the fire, many persons in this country actually supposed the appearance of our display to arise from the burning of Paris.

ONE of the smartest shocks of earthquake known for some time in New Zealand was felt at Wellington at a few minutes past twelve on the night of the 1st of January last.

THE Marlborough College Natural History Society has issued its report for the half-year ending Christmas 1870, showing that it continues to prosper and to do good work. The most interesting papers printed at length are one by Mr. F. E. Hulme, "On Mosaic, Ancient, Mediæval, and Modern," containing a history of the art, with a very good illustration; and a prize essay by Mr. J. B. Fuller, "On the Identification of Birds' Eggs;" in which an empirical mode of classification and recognition is given, which will be useful to collectors. As an appendix is printed the commencement of a very careful second edition of a "Flora of Marlborough," by the Rev. T. A. Preston, which is carried on as far as the completion of Thalamifloræ.

THE Winchester and Hampshire Scientific and Literary Society has issued its first annual report, a promise, we hope, of good things to come. Although the first meeting was only held in November 1869, under the presidentship of the Rev. C. A. Johns, the society now numbers over a hundred members, and seems to have set itself to do useful work in local and general natural history and physical science. Thirteen papers were read before the society during its first session; and a list of plants collected during the year in the immediate neighbourhood of Winchester is promised with the next report. We would remind those who have to compile this list that grasses are flowering plants, although a sentence in the report before us would seem to imply the contrary. The rapid spread of these local natural history societies is a very healthy sign of the increase of the study of science in the country.

THE late numbers of the *Journal of the Scottish Meteorological Society*, of which two, published together, have just appeared, contain, as usual, valuable contributions from Dr. Buchan. In No. 27 he gives his second notice on the Rainfall of Scotland, treating of the central districts from the Firths of Forth and Clyde to the Grampians. The former notice referred to the southern counties. The paper shows that the precipitation in the upper valleys of the Forth and Tay is about 90 inches, or 20 inches more than at the wettest stations in the South. The maximum, 91·90 inches, was observed at Glengyle. It also appears that the amount does not depend on the level of the gauge nearly as much as on local conditions. At Leng, at the height of 325 feet, the amount is 66·37 inches, whereas on Ben Ledi, at the height of 1,800 feet, it is only 58·43 inches. The last number contains a most important paper on the Mean Temperature of these islands, based on the Registrar General's Reports, and on observations from a few foreign stations. The means are taken from the maximum and minimum readings, and in some cases are for thirteen years, in others for less; but all have been reduced to the probable thirteen-year means. Daily range has been disregarded. The paper is illustrated by twelve monthly and a general chart. The results have been carefully discussed by Mr. Buchan, and, whenever possible, with reference to sea temperature. The results for Ireland are of less value than those for Great Britain, owing to the paucity of observations. The paper is specially to be welcomed, as Dove's Tables give but little information for the United Kingdom. The numbers contain some other short papers and the usual tables.

WITH reference to the flowering of the hazel, the *Gardener's Chronicle* calls attention to the singular fact that male catkins often appear in January, which have completely withered away before the female flowers and a second crop of male catkins appears some weeks later, and pertinently asks, what can be the object of this first crop of catkins?

REPORT ON DEEP-SEA RESEARCHES

Carried on during the months of July, August, and September, 1870, in H.M. Surveying Ship "Porcupine." *

BY W. B. CARPENTER, M.D., F.R.S., AND J. GWYN JEFFREYS, F.R.S.

(*Continued from p. 334.*)

DIRECTING our course again towards the Algerine coast, we kept nearly parallel to it during the greater part of the next day, occasionally sweeping the bottom with the "tangles," which gave us abundance of Polyzoa, Echinoderms, &c., of well-known types, without any specimens of novel or peculiar interest. We reached Algiers on the afternoon of the 26th; and as it was necessary to take in coal, we remained in harbour until the 29th, when we resumed our easterly course, still keeping near the coast. The weather now began to be oppressively hot; the surface-temperature of the sea rising to 76° or 78°, and that of the air being often several degrees higher. Wishing to see what would be the point at which the effect of this extreme superheating would cease to manifest itself, we took a set of serial soundings at Station 53, with the following result, which we incline to consider typical of the condition of the proper surface-water of the Mediterranean in the summer season:—

	° Fahr.
Surface	77
5 Fathoms	76
10 "	71
20 "	61.5
30 "	60
40 "	57·3
50 "	56·7
100 "	55·5

Thus the amount of heat lost in the first 20 fathoms is no less than 15°·5; and as much as 9°·5 of this loss shows itself between 10 and 20 fathoms.

Again proceeding into deep water, we perseveringly explored the bottom with the dredge; and from a bottom of 1,508 fathoms we brought up some hundredweights of the same barren mud as had previously given so much trouble to so little profit. The sieve and the washing-tub again returned for answer "barren all." Disappointing as this negative result was to us as zoologists, there are aspects under which it may be viewed that may give it no small value to geologists. On these, however, we can more fittingly enlarge hereafter. Once more, shifting our ground a few miles, we put down our dredge in 1,456 fathoms, and brought it up loaded with a similar profitless freight.

We now determined to keep closer to the shore, and worked for several days along the African coast, for the most part using the "tangles," the ground being too rocky for the dredge. Here we came upon a small fleet of coral fishers, and were not a little interested in finding that they employed "tangles" similar to our own as their most effective method of collecting. We swept the shore with these very assiduously, usually between 50 and 100 fathoms; and although we obtained Polyzoa, Echinoderms, and corals in considerable abundance, there were not many of special interest. We may note, however, that several of the Polyzoa which occurred in the region in which the red coral is found had, when fresh, a red colour nearly as brilliant as that by which it is characterised; but this colour, in the Polyzoa, was quite evanescent.

The extreme heat of the weather having produced an exhausting effect upon our crew, especially on the engineers and stokers, Capt. Calver considered it desirable to give them rest; and we accordingly made for the Bay of Tunis, which we reached at mid-day on Saturday, Sept. 3rd. The town itself is situated at the head of a shallow lagoon, or salt-lake, that communicates with the sea by a narrow channel, and at this entrance there is a small sea-port named the Goletta, having a basin for vessels of moderate size. The lake, although about six miles long, has only from six to seven feet of water at its deepest part; and when the water is unusually low, a small steamer, which plies between the Goletta and Tunis, is not always able to run, as happened at the time of our visit. Owing to the great evaporation, and the absence of any stream of fresh water, the water of this lake is usually very salt; but when heavy rains fall the level is considerably raised, and the saltness is diminished. Thus the condition of this lake in regard to that of the sea outside is sometimes that of the Mediterranean in regard to that of the Atlantic, and sometimes that of the Baltic towards the German Ocean; and we would suggest whether it might not be possible, through our Consulate (which has an office at the Goletta), to have a regular series of observations made upon the relative densities of the water of the lake and that of the sea, and upon the direction of the upper and under current in the channel of communication between them, that might furnish valuable data for the complete elucidation of the subject of currents occasioned by excess of evaporation. We availed ourselves of this short rest to visit the town of Tunis, which, for the most part, retains its genuine Moorish character; and the ruins of Carthage, a few miles off, the most remarkable part of which consists of a series of immense reservoirs for water, supplied by an aqueduct that brought it from a range of mountains at no great distance, from which also the modern town of Tunis is supplied.

This part of our work having brought us to the neighbourhood of the Island of Pantellaria, we landed on it with the view of visiting, if possible, a cavern which had the reputation of being "of icy coldness." As we found, however, that a whole day's delay would be involved, we gave up the idea; and we afterwards obtained elsewhere the information we desired. The continuance of the very hot weather having brought a large part of our crew to a state of such exhaustion as to render a continuation of our operations undesirable, Captain Calver considered it expedient to proceed to Malta without further delay; and we anchored in the Harbour of Valetta on the morning of Saturday, September 10. Here we found it necessary to remain for ten days, the illness of our chief engineer, which we at first hoped might be only temporary, proving sufficiently serious to require that a substitute should be found for him. Our time was passed very pleasantly in visits to the various objects of interest in which the island abounds, and in the enjoyment of the kind hospitality of His Excellency the Governor, Vice-Admiral Key, and other officers. The time was too short for any careful examination of the geology of the island; but one point which struck me as of special interest in relation to the deposit at present forming on the Mediterranean bottom will be specially noticed hereafter.

Quitting Valetta Harbour at midday on September 20, we steered in a N.E. direction towards a point about sixty miles distant, at which a depth of 1,700 fathoms was marked on the chart. This we reached early the next morning (60); and a sounding being taken, 1,743 fathoms of line ran out. As this was the greatest depth we had anywhere met with in the Mediterranean, and as the basin in which the sounding was taken is cut off by the shallows between Sicily and Tunis from all but superficial communication with the western basin, we watched the heaving-in of the sounding apparatus and its accompaniments with no little interest. The thermometers recorded a temperature of 56°, which was one degree *higher* than that which we had met with in our two deepest soundings (1,456 and 1,508 fathoms) in the western basin. The sample of the bottom brought up in the tube of the sounding apparatus indicated the prevalence of a yellowish clayey deposit so similar to that which had elsewhere proved so disappointing, that we could not feel justified in pressing Capt. Calver for the sacrifice of nearly a whole day, which would have been required for a single cast of the dredge at this depth. The specimen of bottom-water brought up by our water-bottle surprised us by its very small excess of density above the surface-water; the specific gravity of the former being only 1·0283, whilst that of the latter was 1·0281; and the proportion of chlorine per 1,000 being 21·08 in the former, whilst that of the latter was 20·77. The surface-water being here *more* dense than the average, the bottom-water was *less* dense; a result which a good deal surprised us at the time, but which subsequent comparison with the densities of specimens taken from the greatest depths we had sounded in the western basin showed to be by no means exceptional. And when we came to reason out the mode in which surface-evaporation may be presumed to operate in augmenting the density of the water beneath, we found it to be quite in accordance with *a priori* probability, that the *deepest* water should show the *least* excess of density above the water at its surface.

Having thus satisfied ourselves, so far as we could do by a single set of observations, that the physical conditions which we had found to prevail in the western basin of the Mediterranean present themselves also in the eastern, we steered for the coast of Sicily; and in a few hours came in sight of Syracuse, with the lofty mass of Etna as a magnificent background in the remote distance. The clouds which lay upon its summit during the earlier part of the day gradually dispersed as we approached it, so that we could distinctly trace the outline of its cone, save where this was obscured by a constantly shifting semi-transparent cloud. Whether this was a light smoke given off from the cone, or a film of vapour condensed by the contact of a current of warm moist air with the colder surface of the mountain-summit, we were unable to distinguish, though we watched it with great interest during the whole afternoon. We steamed quietly along the Sicilian coast during the night, so that sunrise the next morning found us in the narrowest part of the Strait of Messina, between Messina and Reggio; and we shall not easily forget the beauty of the spectacle we then beheld on either shore. Passing through the once-dreaded Charybdis, the dangers of which are rather poetical than real, and leaving on our right the picturesque castle-crowned rock of Scylla, we passed out of the "Faro," which narrows at its northernmost extremity to about three and half miles, into the open sea to the north of Sicily, studded by the Lipari Isles; and steered direct for Stromboli, stopping at 10 A.M. to take a sounding (station 61). This gave us a depth of 392 fathoms, and a bottom temperature of 55°·7, which afforded no indication of unusual elevation. Here again we found the density of the bottom-water scarcely in excess of that of the surface-water; and it was even lower than the surface-water in another sounding taken somewhat further on, (station 62), and at a depth of 730 fathoms, which gave a bottom temperature of 55°·3.

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* Extracted from the Proceedings of the Royal Society.

	Sp. Gr.	Chlorine.
Surface	1·0281	21·32
Bottom, 392 fathoms . . .	1·0282	21.36
Bottom, 730 fathoms . . .	1·0280	21·22

This result, again, surprised us much at the time; but we are now inclined to attribute it to the decrease of surface-evaporation, consequent upon the marked decrease in the heating-power of the sun, which showed itself in the change of the relative temperatures of the sea and air. For whilst for some days before we put into Malta, the surface-temperature of the sea had ranged between 76° and 78°, and the temperature of the air had been usually about 1° *higher*, we now found that while the surface-temperature of the sea ranged between 73°·6 and 76°·6, the temperature of the air was between 2° and 4° *lower*. This difference continued to show itself nearly all the way to Gibraltar; the daily averages of the surface-temperature of the sea ranging between 73°·1 and 75°·6, whilst those of the temperature of the air ranged between 68°·5 and 72°·0. We now approached the rugged cone of Stromboli, from the summit of which there was constantly issuing—as has been the case since the time when the neighbouring island of Hiera was fabled to be the workshop of Vulcan—a cloud of smoke, indicative of active changes in the molten depths beneath. Of this activity, however, we had found no special indication in the temperature-soundings taken nearest to the island. Whether the *general prevalence* in the neighbourhood of Sicily of a bottom-temperature averaging about a degree above that of the western part of the Mediterranean, is due to subterranean heat, is a question which can only be determined by a larger number of observations than we had the opportunity of making. As we neared Stromboli, we were much struck with the height to which the energetic industry of its inhabitants had carried the vine-cultivation all round the cone, save on two slopes looking N.W. and S.E., over one or other of which there is a continual discharge of volcanic dust and ashes. Although no flames were visible during daylight, we could distinctly perceive occasional flashes as night came on. Our course was now laid straight for Cape de Gat, which we passed on the 27th of September, arriving at Gibraltar on the evening of the 28th. The only scientific observations which we had the opportunity of making during this part of our voyage were confirmatory of those which we had made at the commencement of our Mediterranean cruise as to the lower temperature and inferior density of the surface-water, both which we attribute to the inflow from the Atlantic.

Having taken in at Gibraltar as much coal as we could carry, we left the harbour at 9 A.M. on the 30th September, and proceeded at once towards the scene of our previous observations. We thought it worth while, however, to take a sounding in our way towards this, near the 100-fathom line (station 63), for the sake of ascertaining the temperature and specific gravity of the bottom-water. The depth was found to be 181 fathoms, showing that the slope from the shallow to the deep portion of the channel is here very rapid. The bottom-temperature was 54°·7, that of the surface being 68°; and the specific gravity of the bottom-water was 1·0280, that of the surface being 1·0271. This bottom-water thus agreed closely in both particulars with that of the *deep* mid-channel, as ascertained in our first set of observations, and confirmed by our second.

We then steamed out to a point (station 64) nearly identical with that from which our previous investigations had been carried on; and commenced our work with a temperature-sounding. The surface-temperature (65°·6) proved to be here *less* by 2°·4 than it had been found to be at station 63; and this although it was taken an hour later in the forenoon, when an increase might have been expected. It thus corresponded closely with what had been previously found to be the average temperature of the Strait in mid-channel, both during the first approach at Gibraltar from westwards, and during our own experiments at the commencement of the Mediterranean Cruise; and the continuation of the like observations during the remainder of the day and ensuing night gave the same remarkable result, the *rationale* of which will be considered hereafter. The depth was somewhat less than at the neighbouring station 39, being 460 fathoms instead of 517; but the bottom temperature was a little lower, being 54°·7 instead of 55°·5. The respective specific gravities of the surface and bottom waters, and of that of the intermediate stratum of 250 fathoms, were found to coincide almost exactly with those previously determined, as the following comparative statement shows:—

	Sp. Gr. Station 39	Sp. Gr. Station 64
Surface	1027·1	1027.1
250 fathoms	1029·3	1029·2
Bottom	1028·1	1028·3

Now the density of the *bottom*-water here corresponds so exactly with that which prevails over the *deeper* bottom of the western basin of the Mediterranean, whilst it so considerably exceeds that of the *bottom* as well as of the *surface* water of the Atlantic, that we cannot fail to recognise it as belonging to the Mediterranean basin; so that, if it has any motion at all, we should expect that motion to be from east to west. Still more certainly may this be affirmed of the *intermediate* stratum, the density of which corresponds with that of the bottom waters of the *shallower* part of the Mediterranean basin; the greatest depth (586 fathoms) at which such water was obtained, being at station 40, the nearest point to the Strait from which a specimen of bottom water was obtained. And it may be further predicated that a stratum of water of a density of 1029·3 could not overlie water of the density of 1028·1, unless it *moved over* the stratum below, that is, unless (1) the two strata were moving in opposite directions, or (2) were moving at different rates in the same direction, or (3) the upper stratum were in motion in either direction, and the lower stratum were stationary. It will presently appear that the second of these conditions is the one which obtains in the present case.

We now proceeded to repeat our experiments with the "current-drag," with the view of obtaining, if possible, unequivocal evidence of the existence of that westerly under-current, which so many considerations combined to render probable. The direction of the wind during this set of experiments was from the east, or opposite to that of the surface-current; and its force (3 to 4) was sufficient, by its *meeting* the current, to produce a considerable swell, which necessitated the employment of a larger boat, and rendered it unsafe to allow her to drift without men. The sectional area of the boat was therefore greater than on the former occasion, giving the in-current a stronger hold upon her; but, on the other hand, the surface she presented to the wind was also greater; and as this acted in the opposite direction, the latter increase might be considered to neutralise the former, or even rather to exceed it, so as to render the boat more capable of being carried westwards by the "current drag," if this should be acted on by an outward under-current. The rate of surface-current was tested as before, and proved to be 1·823 mile per hour, its direction being N.E. by E. ½ E. This was a retardation of more than a mile per hour as compared with the former observation; and that it was not attributable to the mere surface-action of the easterly wind, was clear from the result of the next observation, which showed that the retardation extended to a depth far below the influence of surface-action.—The "current-drag" having been lowered to 100 fathoms' depth, the drift of the boat was reduced to 0·857 mile per hour, or *less than half* its surface-drift; its direction was nearly the same as that of the surface-current, viz., E. by N. ½ N. The "current-drag" was then lowered to a depth of 250 fathoms; and in a short time the boat was seen to be carried along by it in a direction (W.N.W.) *almost exactly opposite* to that of the middle *in*-current of the Strait. The rate of outward movement of the boat was 0·480 mile per hour; but from the considerations formerly stated, it is clear that the actual rate of the under-current must have exceeded that of the boat on the surface. The "current-drag" was then lowered down to a depth of 400 fathoms; and again the boat was carried along in nearly the same direction as in the previous experiments, namely N.W. ½ N.; but more slowly, its rate of movement being 0·300 mile per hour.

Thus, then, our previous deductions were now justified by a *conclusive proof* that there was at this time a return-current in the mid-channel of this *narrowest* part of the Strait, from the Mediterranean towards the Atlantic, flowing beneath the constant surface-stream from the Atlantic into the Mediterranean; and it will be shown hereafter, by a comparison of all the results of our observations, that a strong presumption may be fairly raised for the *constant* existence of such a return-current, though its force and amount are liable to variation.

As the determination of the boundaries of this return-current, and of the amount and conditions of its variation, could only be effected by multiplied simultaneous observations a different points, with ample license as to time, neither of which fell within the scope of the present expedition, we were obliged to content ourselves, as regards this locality, with what we had found ourselves able to accomplish; and at the conclusion of this day's work we proceeded westwards under easy steam, so as to be able to resume our experiments the next morning in the *shallowest* part of the Strait.

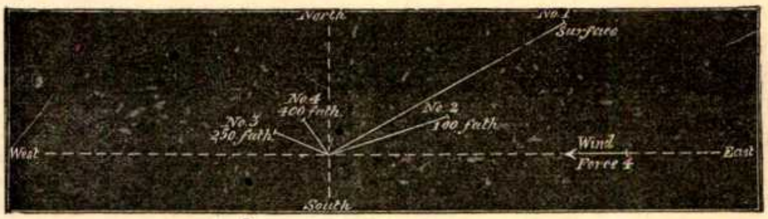

FIG. 1—Rate (per hour) and Direction of Movement of Surface-Float, and of Current-Drag at different Depths; with Force and Direction of Wind.

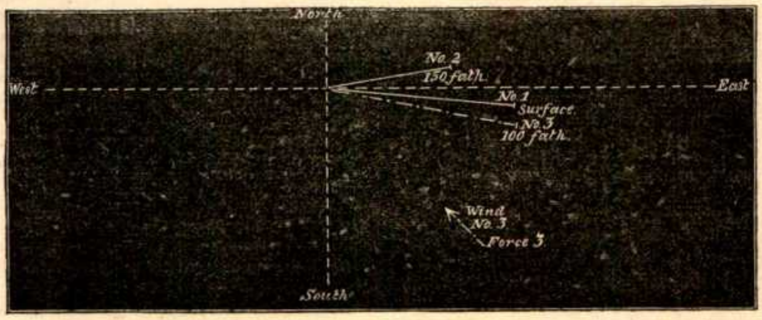

FIG. 2—Rate (per hour) and Direction of Movement of Surface-Float, and of Current-Drag at different Depths; with Force and Direction of Wind in No. 3. (No Wind in Nos. 1, 2.)

The average surface-temperature of the mid-stream during our outward passage through the Strait proved to be 66°; thus corresponding exactly with what we had found it to be on our inward passage seven weeks previously. This depression, as compared with the surface-temperature of the Strait itself nearer the shore, both north and south, and with the temperature of the Mediterranean to the eastward and that of the Atlantic to the westward, is extremely remarkable. We shall hereafter inquire how it is to be explained.

The breadth of the Channel between Capes Spartel and Trafalgar is about twenty-three nautical or twenty-six and a half statute miles. Its northern half is much shallower than the southern, the 100-fathom line off the Spanish coast running at about twelve miles' distance from Cape Trafalgar; whilst along the African coast it keeps much nearer the hore, being at only two miles' distance from Cape Spartel. Between these two lines, the greatest depth marked in the chart is 194 fathoms; and this occurs off Cape Spartel, at less than a mile from the 100-fathom line. Between this and the opposite border of the deeper channel, the depths vary from 130 to 180 fathoms; the abruptness of the differences at neighbouring points indicating a rocky bottom, of which we soon had unpleasant experience.

SCIENTIFIC SERIALS

IN the *Journal of Botany* for March we find a continuation of the useful catalogue of new species of Phanerogamous plants published in Great Britain during the year 1870, and of Mr. Hiern's paper on the forms and distribution over the world of the *Batrachian* section of *Ranunculus*. Dr. Dickie contributes a paper on the distribution of Algæ, and Mr. A. G. More the commencement of a Supplement to Bromfield's "Flora Vectensis." Short notes, reviews, and reports of Societies fill up the number.

THE *American Naturalist* for February contains several interesting papers. Among them is one on the ant-lion (*Myrmeleo immaculatus*), a Neuropterous insect, by Mr. J. H. Emerton, with drawings of its matamorphoses; one on the resources and climate of California, by Rev. A. P. Peabody; notes on some birds in the Museum of Vassar College, by Prof. Jas. Orton; a short account of the spores of Lichens, by Mr. H. Willey; the Sperm Whales, giant and pigmy, by Dr. Theodore Gill, illustrated with numerous drawings, including the skull of *Callignathus simus* and *Physeter macrocephalus*. The Natural History Miscellany comprises also several shorter papers of much interest, including one on the morphology and ancestry of the King Crab, by the editor, Dr. A. S. Packard, jun.

THE March number of the *Geological Magazine* (No. 81) commences with a long article by Mr. James Croll "On the Determination of the Mean Thickness of the Sedimentary Rocks of the Globe." The author disusses the different methods which have been adopted in order to obtain an approximate estimate of the time occupied in the formation of the sedimentary rocks, and remarks that in all these researches it must be borne in mind that, from the continual action of denudation, the existing sedimentary rocks only represent a fraction of the whole thickness of sediments that have been deposited. Taking the denudation of the area of the Mississippi as a guide, he estimates the wearing down of the land at one foot in 6,000 years, and the matter thus removed spread over the bottom of the ocean would produce a deposit one foot thick in 14,400 years. Taking the maximum thickness of British sedimentary strata as calculated by Prof. Ramsay, namely, 72,000 feet, to represent the mean thickness of all the sedimentary rocks which ever have been formed, the author thus gets 1,036,800,000 years as the age of the stratified rocks. Mr. Croll also notices the conditions of the deposition of the sediment carried from the land, and his remarks upon this subject are all worthy of consideration. The editor, Mr. H. Woodward, describes and figures a new Myriopod from the Scotch coal-measures, under the name of *Euphoberia Brownii*, and also some new palæozoic Phyllopod Crustacea, namely *Ceratiocaris ludensis*, a gigantic species from the Lower Ludlow of Leintwardine, *C. oretonensis* and *C. truncatus*, from the yellow carboniferous limestone of Oreton and Farlow in Worcestershire, and *Dithyrocaris Belli*, from the Middle Devonian of Gaspé. He also figures a specimen of *D. tenuistriatus*, McCoy. Mr. De Rance communicates a paper on the occurrence of two distinct glaciations in the Lake District; Mr. John Aitken notices some curious faults occurring in drift at Stockport in Cheshire; and Mr. S. C. Perceval describes the occurrence of Websterite at Brighton.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 16.—"Description of *Ceratodus*, a genus of Ganoid Fishes recently discovered in rivers of Queensland, Australia." By Dr. Albert Günther, F.R.S. We shall return to this communication.—"On the Formation of some of the Subaxial Arches in Man." By George W. Callender.

Geological Society, March 8.—Mr. Joseph Prestwich, F.R.S., president, in the chair. Lieut. Lewis de Teissier Prevost and Mr. John Haines were elected Fellows of the Society; and Dr. C. Nilsson, was elected a foreign member of the Society. The following communication was read:—(1) "On the Red Rocks of England of older date than the Trias," by Prof. A. C. Ramsay, LL.D., F.R.S., V.P.G.S. The author stated that the red colour of the Triassic beds is due to peroxide of iron, which encrusts the sedimentary grains as a thin pellicle. This could not have been deposited in an open sea, but rather in an inland salt lake or lakes. The peroxide of iron, which stains the Permian, Old Red Sandstone and Cambrian rocks, is believed by the author to have been deposited in the same manner, in inland waters, salt or fresh. Agreeing with Mr. Godwin-Austen, the Old Red Sandstone was of Lacustrine origin. The absence of marine shells helps to this conclusion. The fish do not contradict it, for some of their nearest living congeners live in African and American rivers. The life of the Upper Silurian deposits of Wales and the adjoining districts continued in full force up to the passage-beds, which mark the change from Silurian to Old Red Sandstone. In these transition strata, genera, species, and individuals are often few, and dwarfed in form. Near Ludlow and May Hill the uppermost Silurian strata contain seeds and fragments of land plants, indicating the neighbourhood of land, and the poverty of numbers and the small size of the shells a change in the condition of the waters. The fish of the Old Red Sandstone also indicate a change of condition of a geographical kind. The circumstances which mark the passage of Silurian into Old Red Sandstone were as follows:—First, shallowing of the sea, so that the area changed into fresh and brackish lagoons, afterwards converted into great freshwater lakes. At the present day marine species are occasionally found living in fresh water, as for example in the Swedish lakes. The same may have been the case in the Old Red Sandstone period. The Old Red Sandstone waters at their beginning are comparable to the Black Sea, now steadily freshening; or the Caspian, once united to the North Sea, if by a change of amount of rainfall and evaporation it freshened by degrees, and finally became a freshwater lake. The Permian strata, to a great extent, consist of red sandstones and marls in the greater part of England; and the Magnesian Limestone of the north of England is also in less degree associated with red marls. These do not occur in the same districts of England, excepting in Lancashire, where a few beds of Magnesian Limestone are interstratified with the marls. The sandstones and marls being red, the colouring matter is considered to be due to peroxide of iron, possibly precipitated from carbonate of iron, introduced in solution into the waters. Land plants are found in some of the Permian beds, showing the neighbourhood of land. No mollusca are found in most of the red beds, except a brachiopod in Warwickshire, and a few other genera in Lancashire, in marls associated with thin bands of Magnesian Limestone. The traces of amphibians are like those found in the Keuper Sandstone, viz., *Dasyceps Bucklandi* and labyrinthodont footprints in the Vale of Eden and at Corncockle Moor, printed on damp surfaces, dried in the sun, and afterwards flooded in a way common in salt lakes. Pseudomorphus crystals of salt and gypsum help to this conclusion. The molluscous fauna of Lancashire, small in number, in this respect resembles the fauna of the Caspian Sea. The fauna of the Magnesian Limestone of the east of England is more numerous, comprising thirty-five genera and seventy-six species, but wonderfully restricted when compared with the Carboniferous fauna. The specimens are generally dwarfed in aspect, and in their poverty may be compared to the Caspian fauna of the present day. Some of the fish of the Marl-Slate have strong affinities to carboniferous genera, which may be supposed to have lived in shallow lagoons, bordered by peaty flats; and the reptiles lately described by Messrs. Howse and Hancock have terrestrial affinities. Besides the poorness of the Mollusca, the Magnesian Limestone seems to afford other hints that it was deposited in an inland salt lake subject to evaporation. Gypsum is common in the interstratified marls. In the open sea limestone is only formed by organic agency, for lime, in solution, only exists in small quantities in such a bulk of water; but in the inland salt lakes carbonates of lime and magnesia might have been deposited simultaneously by concentration of solutions due to evaporation. Some of the Magnesian Limestone strata have almost a tufaceous or stalagmitic aspect, as if deposited from solution. The Cambrian strata also show some evidence of not being true marine deposits. They are purple and red, like the other strata previously spoken of; and the surfaces of the beds sometimes exhibit sun-cracks and rain-pittings. The trilobite *Palæopyge Ramsayi* is considered by the author to be an accidental marking, simulating the form of a trilobite; and the fossils of St. David's are found in grey beds, which may mark occasional influxes of the sea, due to oscillations of level. The foregoing reasonings, in the author's opinion, lead to the conclusion that a continental area existed more or less in the northern hemisphere from the close of the Silurian to the end of the Triassic epoch, and that this geographical continuity of land implies probable continuity of continental genera. There is therefore no palæontological reason why the *Hyperodapedon*, *Telerpeton*, and *Stagonolepis* of the Elgin country should be considered of Triassic age, especially as the beds in which they occur are stratigraphically inseparable from the Old Red Sandstone. Finally, terrestrial and marine European epochs were rapidly reviewed. 1. The Cambrian epoch was probably fresh water. 2. The Old Red Sandstone, Carboniferous, Permian, and Trias were formed during one long continental epoch. This was brought to an end by partial submergence during the Jurassic epoch; and by degrees a new continental area arose, drained by the great continental rivers of the Purbeck and Wealden series, as shown in various parts of Europe. 3. This continent was almost entirely swallowed up in the Upper Cretaceous seas. 4. By subsequent elevation the Eocene lands were formed, and with this continent there came in a new terrestrial fauna. Most of the northern half of Europe since then has been continental, and its terrestrial fauna essentially of modern type. If according to ordinary methods we were to classify the old terrestrial faunas of North America, Europe, Asia, and probably of Africa, a Palæozoic epoch would extend from Old Red Sandstone to Wealden times, and a Neozoic epoch at least from the Eocene period to the present day. The Upper Cretaceous strata would at present remain unclassified. The marine epoch would also temporarily be divided into two, Palæozoic from Laurentian to the close of the Permian times, and all besides down to the present day, would form a Neozoic series. The generic gaps between the two begin already to be filled up. The terrestrial and the marine series at their edges at present overlap each other. The great life-gaps between the two terrestrial periods may some day be filled up by the discovery of the traces of old continents containing intermediate developments of structure as yet undiscovered. Prof. Huxley was pleased to find that the author, on physical grounds, extended some views which he himself had, from other reasons, brought before the Society. He mentioned that there had lately been found in the fresh-waters of Australia a remarkable fish, which had been described, he thought erroneously, as a *Ceratodus*, but which, in many essential characters, was a *Dipterus*, though allied in some respects to *Phaneropleuron*. In other respects it was connected with *Lepidosiren*. It was about to be described by Dr. Günther. The dentition of this fish is curiously similar to that of the Devonian *Dipterus;* and its existence, he thought, corroborated Prof. Ramsay's argument. He agreed with the author as to his views respecting the terrestrial fauna of ancient times, and was quite prepared for the discovery of mammalian remains in earlier formations than those in which they are at present known. He did not so cordially agree with his views as to the marine fauna. He would carry back the forms from which those of the present day are immediately derived to Cretaceous rather than Eocene times. Between the Cretaceous and the Liassic strata there was what appeared to be a middle group, succeeding the Palæozoic. Mr. Etheridge commented on the dwarfed condition of our Permian fauna, which corresponds in the main with that of the Continent, though with fewer genera and species. Prof. Rupert Jones protested against some of the reasons adduced for regarding some of the areas cited as having been inland lakes, though no doubt such lakes must have existed. He thought that mere colour could not be taken as a criterion. If it were, he inquired why the bottoms of the present lakes were not red? Many of the red rocks were, moreover, full of marine fossils. He contended for the true trilobite character of *Palæopyge Ramsayi*, and mentioned its occurrence and that of *Lingula ferruginea* in red Cambrian rocks as proving the marine character of the beds. The Magnesian Limestone he also insisted upon as a purely marine and open sea deposit. Prof. Morris thought the subject required further consideration before the whole of Prof. Ramsay's views were accepted. The Cambrian beds, for instance, containing great beds of conglomerate, seemed such as could only be due to marine action, and would derive their red colour from the decomposition of the old hornblendic gneiss from which they were derived. With regard to the Red Sandstone, he would inquire whether the colour might not be derived from the decomposition of rocks composed of hornblendic materials. The Old Red Sandstone beds, though in this country containing fishes which might be of freshwater genera, had in Russia the same fishes associated with marine shells; and much the same was the case in the Trias. Dr. Carpenter had been led to the conclusion that wherever there was an inland sea connected with the ocean by a strait even of moderate depth, there was a double current tending to preserve some degree of similarity between the waters of the two, the difference of specific gravity in the Mediterranean as compared with the Atlantic being about as 1·026 to 1·029. In the Red Sea, where so little fresh water came in, and there was an evaporation of nearly eight feet per annum, the water was but little salter than that of the ocean with which it was connected. In the Baltic there is an undercurrent inwards, which still keeps it brackish; for otherwise the influx of fresh water was so enormously in excess of the evaporation, that it would long ago have become perfectly fresh. Such facts bore materially on the speculations of the author. Capt. Spratt maintained that in the Dardanelles there was not a trace of such an undercurrent as mentioned by Dr. Carpenter. In the winter months, when the flow of the rivers into the Black Sea was for the most part arrested by ice, the salt water of the Mediterranean was carried into the inland seas, and these being much deeper than the channel of the Dardanelles, the salt water, by its greater specific gravity, remained in the bottom of the sea of Marmora, so that while the upper portion of the water and that on the shores were fresh, marine conditions existed in the deep centre of the sea. Dr. Duncan mentioned that in certain coral reefs intersected by freshwater currents, the corals still continued to be formed; so that the existence of dwarfed forms of corals in ancient times was quite consistent with modern facts. Mr. Forbes commented on the chemical features of Prof. Ramsay's views, and could see no reason why the beds containing iron should not have been deposited in the open sea. Many beds, for instance the Gault, contain more iron than those which are now red, though they may be grey or blue. In sands the grains are often coloured only superficially with iron, probably derived from sulphates. In other cases the sands consist of fragments of rocks already red. There was, in fact, no reason why the beds deposited in the open sea might not subsequently, by oxidation, become perfectly red. Prof. Ramsay replied to the remarks of the various speakers, and summed up by contrasting the usual colour of marine fossiliferous beds with that of the thick, almost non-fossiliferous rocks of which he had been treating

Anthropological Institute, March 20.—Sir John Lubbock, Bart., M.P., president, in the chair. Mr. William Sloan and Mr. John Edward Brearey, of Madras, were elected members. After the adjourned discussion of Mr. Jackson's paper, "The Racial Aspect of the Franco-Prussian War," Mr. Hyde Clarke read a paper "On the Migrations of the Georgians, Circassians, and Amazons, and their connection with the Tibeto-Caucasian race," of which the following is an abstract:—By means of the application of the Georgian, Circassian, and other existing languages *in situ*, the existence of a previous Georgian or Caucasian population was shown, and that the extent of its area was much greater than could have been suspected. This Palæogeorgian language had a much nearer relation to the existing languages than the Hieroglyphic to the Coptic, or the Cuneiform to the Syriac and Persian, but it was in a different and earlier stage of comparative grammar than the Hebrew or Sanskrit, and to which the Caffre group presents some resemblances of structure. The connection of the language with the comparative mythology of the worship of fire and water, gives further evidence as to the diffusion of a population which had held empire over India and thence to the Atlantic shores and these islands. Accepting as a doctrine the conquest of Palestine from the Canaanites and other races identified with the Caucaso-Tibetans, the period of empire would range from 3,500 to 4,500 years ago, during which the germs of the existing civilisation were developed. This population belonged to the family which includes the Tibetan and Chinese stocks. Many portions of the Mosaic record, considered to have been interpolated during the Babylonian captivity, now appeared to be of the greatest antiquity. Many subjects, corollary to the main discoveries, were touched upon, including the connection of the Etruscan, the Phrygian, the languages of Asia Minor, the Akkaa with the Palæogeorgian, also the Lydo-Assyrian rock-cut monuments, the Cyclopean buildings, the so-called Druidic structures, the discovery of metals, &c.

Royal Geographical Society, March 13.—Major-General Sir Henry C. Rawlinson, K.C.B., vice-president, in the chair. The following new Fellows were elected:—Sir James Anderson; W. Blackmore; R. B. Jackson, Sir Donald F. McLeod, K.C.S.I., C.B.; Capt. James Nicol; G. Wm. Petter. The paper read was, "On Mr. Baines's Explorations of the Gold-Fields of South Africa," by Dr. R. J. Mann, and was founded on the voluminous journals, itineraries, astronomical observations, &c., sent home by Mr. Thomas Baines, who had been employed, since the end of 1868, in making a general survey of the gold-yielding country lying between the Limpopo and Zambesi rivers. Leaving the Limpopo at its north-western bend, near the Makloutse and Shapsa rivers, he traversed, with his companions, the range of highlands separating the basins of the Zambesi and Limpopo, in a north-easterly direction, for 300 miles, negotiating with the powerful Matabele chiefs, fixing geographical positions, investigating the mineralogy, and sketching, with his well-known artistic skill, the scenery and people. His farthest point to the north was 17° 30′ S. lat., and in one part of his route he was within 120 miles of the Zambesi. On the route, the heads of a great number of streams were struck, flowing on the one side into the Zambesi, and on the other towards the Limpopo or Indian Ocean, the high land (averaging about 3000 feet) forming the watershed in this part of Africa. The country was healthy, but rather barren and arid, especially on the western slope of the watershed. The chief of the Matabele came to an amicable agreement regarding the working of the gold, which was found very widely distributed over the region, but only in quartz reefs, not in alluvial washing. Many additional particulars regarding the country were given, after the reading of the paper, by Sir John Swinburne, who travelled over most of the same ground, and partly in company with Baines. He said the dry uplands were totally unfit for European settlement, but the well-watered northern and eastern slopes were fertile, and adapted for all kinds of tropical produce. The rich, well-wooded country on the eastern side, rugged with precipitous hills and deep valleys, was inhabited by a superior negro tribe, called *Mashonas*, totally distinct from the invading Matabele of the opposite (western) side of the uplands. Whilst the Metabele—a section of Caffres—follow no arts but those of war, and go nearly naked, the Mashonas are well clothed, and practise the art of smelting and working iron in great perfection. He exhibited a specimen of gold, weighing 27 ounces, extracted by his men from the quartz reefs. Mr. Galton spoke of the great additions made by Mr. Baines, in this journey, to our topographical knowledge of Africa; and Mr. Dunlop stated that quartz had now been found in the country yielding eight and ten ounces of gold to the ton, and that the country was a suitable field for British enterprise.

Linnean Society, March 16.—Mr. G. Bentham, president, in the chair. Col. Grant was elected a fellow.—Prof. Oliver exhibited specimens of *Cupania cinerea*, Poepp. belonging to the order Sapindaceæ, from the Kew Herbarium, in which the seed, partially surrounded by an arillus, splits open, and the exalbuminous embryo falls out, leaving the testa and arillus on the tree, the only instance known of such dehiscence of the seed itself.—An extract was read from a letter from General Munro to Dr. Hooker, describing the vegetation of a little known part of the island of St. Vincent, in the West Indies.—Mr. Henry Reeks exhibited a series of forms of *Aspidium* from Woodhay in Hampshire, which he considered showed a regular gradation between *A. aculeatum* and *A. angulare* of authors.—Notes on *Capparis galeata* and *C. Murrayii*, by Mr. N. A. Dalzell, who believes that these two perfectly distinct species have generally been confounded with one another.—Dr. B. Seemann exhibited a lamellicorn beetle from Nicaragua, one of the largest Coleoptera yet found in America.

PARIS

Academy of Sciences, March 13.—A sharp discussion arose on reading the *procès verbal* of the last sitting. General Morin complained that it was stated by M. Sainte Claire Deville that science had not received proper application in warfare. He was obliged to confess that the French artillery was not up to the times, since they had no steel guns. Steel guns had been condemned as useless by the committee because His Majesty was a great artillerist.—The report of the death of M. Becquerel, sen., during the investment of Paris was stated to be incorrect. It was really M. Dumeril, the son of the celebrated electrician, who had died; M. Becquerel, sen., was not present at the sitting.—M. Leverrier was present at the sitting. M. Dumas read for the learned astronomer a long memoir on the Defence of the Rhone Valley, to which M. Leverrier was attached during the investment of Paris. He resided at Nîsmes and not at Marseilles, as had been said. The principal feature of this work is the construction of an apparatus for optical signalling. This apparatus can be used during day-time, and signals can be seen at a distance of eight miles by day with the naked eye.—M. Serret, President of the Scientific Delegated Commission at Tours and then at Bordeaux, read over a reclamation on behalf of M. Bouccarut, who claims a right to the invention of the instrument manufactured by M. Janssen for guiding aëronauts. M. Serret gave a certificate testifying that M. Bouccarut in the month of September communicated an instrument similar to M. Janssen's compass. If so why did the Delegated Scientific Commission keep the communication without warning the Government of National Defence at Paris, where the instrument was much wanted, as not less than ten balloons were lost, five of them in the sea, because aëronauts were unable to see their way? M. Delaunay read a declaration stating that he acknowledged that Mr. Hennessy had used the same arguments as himself against Mr. Hopkins' theory relative to the fluidity of the interior parts of the earth. But the adhesion given by Sir W. Thomson and other learned men to Mr. Hopkins' views is the reason why he did not regret having again raised this much controverted question.—M. de Fonvielle presented a paper explaining why the gas inside an aerostat very often suddenly increases in density. The phenomenon is common in warm weather when the gas is saturated with vapour from the water of the gasometer, and also when the balloon is rising at a quick rate. The increased density is owing to a quick refrigeration corresponding to the dilatation of the gas when the balloon is ascending to a higher level. It is an illustration of the law of equivalence of *force and heat*. It is the same experiment as is noted in Tyndall's special treatise on that subject, when damp air is placed under an air-pump worked at a certain rate. The movements of the balloon being able to be controlled, it is possible, through an aëronautical ascent, to come to a numerical conclusion.—M. Bouley delivered an interesting lecture on the cattle plague, which is one of the most important topics of the moment. He gave conclusive evidence that it was imported by the Prussian armies. The plague has had really terrific effects in the provinces. On a sea coast the carcasses of infected animals were so numerous that it was impossible to bury them. The authorities were obliged to fill with the putrid cargo old hulks, which were sunk by cannon balls from a distance. He said that infected animals were not unwholesome in their flesh. A secret committee was opened on the question, proposed by M. Sainte Claire Deville.

DIARY

THURSDAY, MARCH 23.

ROYAL SOCIETY, at 8.30.—Experiments on the Successive Polarisation of Light, with the Description of a New Polarising Apparatus: Sir Charles Wheatstone, F.R.S.—On an Approximately Decennial Variation of the Temperature at the Observatory Cape of Good Hope, viewed in connection with the Variation of the Solar Spots: E. J. Stone, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—On Flint Implements and other Antiquities from Kent: J. Brent, F.S.A.—On Miscellaneous Antiquities from Leicestershire: Rev. Assheton Pownall, F.S.A.

ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

LONDON INSTITUTION, at 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers, M.A.

FRIDAY, MARCH 24.

QUEKETT MICROSCOPICAL CLUB, at 8.

ROYAL INSTITUTION, at 9.—Colour: Prof. Clerk Maxwell.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, MARCH 25.

ROYAL INSTITUTION, at 3.—Spirit of the Age: Mr. O'Neil.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.

MONDAY, MARCH 27.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

INSTITUTE OF ACTUARIES, at 7.—On the Equitable Appointment of a Fund between the Life-tenant and the Reversioner: Andrew Baden.

LONDON INSTITUTION, at 4.—On Astronomy: R. A. Proctor. (Educational Course.)

ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

TUESDAY, MARCH 28.

ROYAL INSTITUTION, at 3.—Nutrition of Animals: Dr. M. Foster.

WEDNESDAY, MARCH 29.

SOCIETY OF ARTS, at 8.—On Woman's Work, with Special Reference to Industrial Employments: Miss Emily Faithfull.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

THURSDAY, MARCH 30

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

LONDON INSTITUTION, 7.30.—On Economic Botany: Prof. Bentley.

CHEMICAL SOCIETY, at 8.—Anniversary Meeting.

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THURSDAY, MARCH 30, 1871

FIRST REPORT OF THE ROYAL COMMISSION ON SCIENTIFIC INSTRUCTION AND THE ADVANCEMENT OF SCIENCE.

TO THE QUEEN'S MOST EXCELLENT MAJESTY.

May it please your Majesty—

WE, the Commissioners appointed by Your Majesty to make Inquiry with regard to Scientific Instruction and the Advancement of Science, humbly beg leave to present to Your Majesty the following First Report:—

1. We have heard the evidence of witnesses in reference to the following subjects, forming part of our inquiry, viz., the Royal School of Mines, the Geological Survey of Great Britain and Ireland, the Mining Record Office, and the Museum of Practical Geology, at present located in Jermyn Street; and also concerning the Royal College of Chemistry, at present lodged in a building in Oxford Street; which institutions are under one head, entitled Director-General of the Geological Survey of Great Britain and Ireland and Director of the Royal School of Mines.

2. There is no necessary connection between the direction of the Geological Survey of Great Britain and Ireland and the government of the Royal School of Mines.

3. The Royal School of Mines and the Royal College of Chemistry, which practically constitute one School of Pure and Applied Science, are not organised in such a manner as to enable them to perform efficiently the work for which they were originally, or are, at present, intended. We base this conclusion upon three grounds, (*a*) The absence of a chair of Mathematics, (*b*) The absence of Physical or Biological Laboratories in which students can receive practical instruction, (*c*) the insufficiency of accommodation in the Royal College of Chemistry.

4. The officers of the Geological Survey are greatly hindered in their work by want of accommodation; for although their number has been quintupled during the last twenty years, the space originally allotted to them has not been increased.

5. The space allotted to the Mining Record Office is already insufficient for the proper reception and arrangement of the valuable series of documents accumulated there; and for the accommodation of the public who desire to consult them.

6. The collections in the Museum of Practical Geology require greater space for their proper display than is at present afforded.

7. In order to provide a remedy for the inconveniences which have been enumerated, we recommend: (*a*) That the building in Jermyn Street be given up to the Survey and to the Museum, with the reservation that the Lectures to Working Men be delivered as heretofore in the Theatre; (*b*) That the building in Oxford Street be vacated by the Royal College of Chemistry; and (*c*) That the Mining Record Office be lodged with the Statistical Department of the Board of Trade; or, failing accommodation there, in the building now occupied by the Royal College of Chemistry.

8. Without expressing any opinion, at present, as to the policy of Government Schools of Science, your Commissioners, having to deal with the Royal School of Mines and the Royal College of Chemistry as Institutions which have existed for 20 years, and which, during that period, have turned out a large number of well-instructed Students, consider that such steps should be taken as may be necessary to render their Teaching thoroughly efficient.

9. With this object we recommend that the two Institutions be consolidated; that Mathematics be added to the Courses of Instruction now given; and that sufficient Laboratories and Assistance for giving Practical Instruction in Physics, Chemistry, and Biology, be provided.

10. The Institution thus formed (hereinafter called the "Science School") may be conveniently and efficiently governed by a Council of Professors, one of that body acting as Dean.

11. We have further heard evidence concerning the Buildings at South Kensington, now nearly completed, and intended for the reception of a projected School of Naval Architecture and Science; and we recommend that the Science School should be accommodated in these buildings. We have given careful attention to the considerations in favour of the retention in Jermyn Street of the Technical Instruction in certain branches, but we are of opinion that these considerations are outweighed by the great advantages to be derived from concentration.

12. We have further heard evidence concerning the Royal School of Naval Architecture and Marine Engineering, now conducted at South Kensington; and we recommend that the theoretical instruction of that school should in future be given in the Science School, the general instruction in Mathematics, Physical Science, and Mechanical Drawing thus becoming common to both schools. We also recommend that no additional buildings, and no reconstruction of the temporary buildings at present occupied by the Royal School of Naval Architecture and Marine Engineering, should be undertaken, until a further Report has been received from this Commission.

13. We have further heard evidence concerning the system of teaching Elemementary Science under the Science and Art Department; and we are of opinion that the quality of the Instruction given under this Department would be greatly improved if the teachers received Practical Instruction in Elementary Science. Such instruction has, indeed, already been given with marked advantage, although only to a limited extent. The Science School will be available for the instruction of many Science Teachers throughout the country; but we reserve for a Further Report any expression of opinion as to the precise character of such Instruction, and as to the conditions under which it shall be accessible.

14. The organisation of, and accommodation required by, the Science School (including its Technical Branches) and the Royal School of Naval Architecture, will be dealt with in detail in a further Report.

All which we humbly submit for Your Majesty's gracious consideration.

(*Signed*) DEVONSHIRE

LANSDOWNE, JOHN LUBBOCK, J. P. KAY SHUTTLEWORTH, B. SAMUELSON, W. SHARPEY, THOMAS H. HUXLEY, G. G. STOKES, HENRY J. S. SMITH.

J. NORMAN LOCKYER, Secretary
March 9, 1871

THE IRON AND STEEL INSTITUTE

NOTWITHSTANDING the pre-eminent scale on which the mineral and metallic industries of Great Britain are conducted in practice, it must nevertheless be admitted that, as a rule, we have hitherto been long and far behind our continental neighbours in respect to possessing institutions calculated to aid in developing or advancing the scientific or practical bearings of such subjects, or to afford the means of intercommunication between those occupied or interested in such pursuits. To this rule, however, we now have, at least, one honourable exception in the case of the Iron and Steel Institute, now holding its second annual meeting in London, and the establishment of which, in 1869, must be looked upon by all interested in the application of science to the arts, not only as a decided step forward in the right direction, but may even be regarded as inaugurating a new era in the history of the so important iron and steel manufactures of Great Britain.

It has been often the fashion, possibly also with some show of justice, to represent the British manufacturer as a narrow-minded individual surrounded by and, as it were, isolated from even the rest of his own class by a sort of atmosphere heavily loaded with trade jealousy and manufacturing secrets. The experiment of the last two years, however, has amply proved, at least in the iron trade, that it only required the establishment of such an association as the Iron and Steel Institute, to present him in a very different and more favourable light; for the mere fact of bringing together from different parts of the country men all deeply interested in similar pursuits, has at once dissipated the petty jealousy inseparable from a previous state of isolation, and has, besides indicating how much can be effected by combined action, convinced the majority at least, that the interests of the individual manufacturers are intimately bound up with the advancement of the country at large.

The Iron and Steel Institute now numbers some four hundred or more members, including the principal ironmasters and others practically engaged in the production or working of iron and steel, or connected more or less directly with those manufactures by reason of their scientific attainments in metallurgy or the allied sciences; so that, taking into consideration that the Institute has as yet been barely two years in existence, this rapid progress must be regarded as the most convincing proof that a real want for such an association had been very generally felt.

The consideration of what may be termed commercial in contradistinction to technical, questions, such as, for example, those connected with wages, trade regulations, &c., do not come within the sphere of the Institute; the objects of which, besides affording a means of communication between its members, are restricted to the acquisition and dissemination of information, and the discussion of all scientific and practical subjects bearing upon the production and manufacture of iron and steel.

The methods by which these objects are sought to be attained are threefold—viz., by the publications issued by the Institute; the formation of committees to examine into and report upon subjects of special interest; and by general meetings of the members, two at least in each year, one of which is held in London in the spring, whilst the other or autumn meeting is located in the country, in some one of the manufacturing districts, as may be determined by the Council.

The excellent attendance at both the London and the country meetings at Middlesborough and Merthyr Tydvil, under the able presidency of the Duke of Devonshire, as well as in the sustained interest which has been kept up in the proceedings of the Institute, have already proved it to be a success, besides showing how much may be effected by bringing from all parts, into personal contact, those interested in the same occupations, whereby a mutual interchange of ideas and a spirit of generous rivalry is established, which cannot fail to do good to the individual, as well as tend to the progress of the industry of the country at large.

An examination of the publications issued by the Institute up to the present date, will amply justify the assertion that they fully maintain the high position which it aspires to, whether they be judged from a strictly scientific or technical point of view, and that they are entitled to rank alongside any which emanate from even the best institutions of like character on the Continent. For the years 1869 and 1870 they appeared in the form of Transactions; in all seven numbers, which contained the proceedings of the Institute, prefaced by an able inaugural address delivered by its first president, the Duke of Devonshire, who himself is largely interested in iron mines and smelting works, along with a series of well-illustrated papers on various subjects relating to iron and steel-making communicated by members of the Institute.

At the commencement of the present year, however, the Council decided that these Transactions should give way to the more convenient form of a Quarterly Journal, and the first number made its appearance on the 1st of February as a volume of 276 pages, copiously illustrated by well got-up plates, and containing numerous valuable communications to the Institute; as, for example, by Mr. J. L. Bell, on the chemical phenomena of iron smelting; Mr. Siemens on pyrometers; Mr. Kohn on alloys of iron and manganese, &c., whilst in addition to the usual proceedings of the Institute a new and important feature was introduced in the shape of quarterly reports from the two secretaries; the general secretary giving a summary of what has been done in Great Britain in connection with these subjects outside the Institute, whilst the foreign secretary communicates a report on the progress of the iron and steel industries abroad, being an attempt to supply a long-acknowledged desideratum by keeping the public here informed as to what is being done in connection with the production and manufacture of iron and steel in foreign countries.

The present meeting opened on Tuesday with an address from its new President, Mr. Henry Bessemer, so well known in connection with the process which now has effected a world-wide revolution in the manufacture of steel; whilst on the two following days various communications were to be brought forward by Messrs. Bell, Ferrie, Kohn, Tate, Walker, and others, besides a lecture by Prof. Roscoe, F.R.S., on Spectrum Analysis, with special reference to the manufacture of iron and steel.

DAVID FORBES

THE ZOOLOGICAL RECORD ASSOCIATION

A FEW weeks since we announced the establishment of an association for the purpose of continuing the publication of the *Record of Zoological Literature*. Within the last fortnight this new body has been definitely constituted, its rules settled, and a council chosen to manage its affairs. A few words concerning the Association and its objects may, therefore, not be out of place.

The *Record of Zoological Literature* was commenced in 1865 as the private undertaking of its publisher, Mr. Van Voorst, under the editorship of Dr. Albert Gunther, F.R.S., who in addition to the superintendence of the whole, himself contributed no small part of the contents of the volume, namely, the portions relating to Mammals, Reptiles, and Fishes. The remaining groups of animals were placed in the hands of Prof. Newton, Dr. Eduard von Martens, Mr. Spence Bate, Mr. W. S. Dallas, Prof. Reay Greene, and Dr. Cobbold. The extreme value of the work to all zoologists was at once recognised by those who became acquainted with it; but, owing to the notorious difficulty of making purely scientific publications known to the persons most desiring them, its sale was limited even in this country, while the high price of the volume kept it almost entirely out of the foreign market. Still publisher, editor, and contributors struggled on, some of the latter even foregoing the scanty pittance they were entitled to receive for their labour. With the third annual volume the undertaking would probably have ceased entirely, but for the generous intervention of the Biological Section of the British Association, which evinced so strong an interest in its continuance that the Committee of Recommendations was prevailed upon to sanction a grant of 100*l.* in its aid—a grant which has since been annually renewed. At the same time, chiefly in consequence of representations made by influential members of the Association, the experiment was made of selling the volume in three separate parts. But this "division of the records" brought no more "comfort" to its supporters than the same process did to the hero of "Locksley Hall;" for the result was that, while no doubt a larger number of zoologists altogether availed themselves of the opportunity of purchasing at a comparatively low price the parts in which they were especially interested, a smaller number of entire copies were sold, and the publisher's loss became so considerable that he positively refused to continue the work.

At this juncture the Zoological Record Committee of the British Association appointed at Liverpool last year, after making several attempts to place the work on a new footing, arrived at the conclusion that the only way of continuing it was by means of an Association with that especial object, and set to work to develop itself accordingly. The process, we believe, was not effected without the normal concomitant symptoms of yelk-cleavage, but these have been got over, and the embryo is now before the world, a promising entity, which, we trust, will, after a little more nursing, maintain an independent existence—for that, of course, is the intention of its producers.

Besides the grant from the British Association before mentioned, aid has been rendered for the forthcoming volume by the Zoological Society of London, in the shape of the interest of a legacy lately bequeathed to the Council of that Society for the furtherance of zoological science, and a very respectable amount of guarantors to the amount of 5*l.* each stand godfathers to the newly-born Association. These consist of nearly all the leading zoologists or well-wishers to zoology of the day, and, glancing over the list, we find upon it such names as assure us that the utmost divergence of views has not hindered their bearers from combining in support of the cause.*

And now a few words as to the objects of the *Zoological Record*, and on the manner in which it has been conducted. The enormous range which Biological or even Zoological Science has of late years taken, has produced a correspondingly enormous mass of literature, a compendious knowledge of which is absolutely necessary to every one engaged in the study. Many have been the cases when, after a long and tedious investigation of some abstruse point has been fully or nearly accomplished, the toiling zoologist has found his labours forestalled by some one else, who has been working also in the same direction, and has published the results in some little-known journal mayhap only a short time before. We do not say that such labour is thrown away; but it will be admitted that such anticipation by another is a sore discouragement, and may be anybody's fate. Now, it is one of the main designs of the *Zoological Record* to prevent, so far as it is preventible, this loss of time, and there can be little if any doubt that the publication of an annual volume showing what has been done must most effectually point out what remains to do. An explorer starting off to make geographical discoveries in Africa or the Polar Regions would have little chance of success without knowing what tracts have been already visited. Just then, as the publication of maps or charts is absolutely necessary to produce original geographical researches, so the publication of an annual like the *Zoological Record* is necessary to produce original researches in those regions of Biology to which it refers. The only question is how far the volumes already published have fulfilled their purpose. The recorders, we are sure, are content to have the value of their labours judged by their fellows, and of what nature that judgment is, the unanimous approval of Section D of the British Association is a sufficient test. It must be observed that the ideal "record" of its authors is something more than a mere "report," while it is certainly not a "review." The distinction is somewhat difficult to define, and not easy to maintain. It may be that the recorders, being human, have not always succeeded in maintaining it; but it seems to us remarkable that they have so nearly succeeded. In the first volume this is especially to be observed, and since there was no exemplar before the eyes of the contributors while working, we must attribute this singleness of purpose to the good drilling of Dr. Gunther, who originally planned the campaign, wherein he was no less the active man-at-arms than the skilful commander-in-chief. Various changes have been made in the corps of recorders, but of the seven contributors to the first volume four took part in the last, while several recruits have from time to time been enlisted, and the last muster-roll includes Messrs. Rye, Kirby, and Marshall, together with Prof. Percival Wright,—Mr. Spence Bate, Prof.

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* To take only a few by way of example, there are Profs. Owen and Huxley, Drs Gray and Sclater, Canons Kingsley and Tristram, Messrs. Darwin and Jenyns.

Reay Greene, and Dr. Cobbold having retired from the ranks. As regards the future, Dr. Gunther, we may state, has resigned the editorship to Prof. Newton, who cannot do better than follow generally in the footsteps of his predecessor, but we are glad to say that the founder will still continue his admirable contributions, for which no one in this country is more fitted. The rest of the administration is not, we believe, definitely settled, but there is little fear that it will not be as efficient as ever, and we wish prosperity to its useful labours.

But before we dismiss the subject, we should like to add a few suggestions, both to the recorders in particular and to the scientific public at large. To the former we would say that as brevity is the soul of wit, so it is of recording. There have been occasions, it must be owned, when this virtue has been disregarded. Let there be no indulgence in "paste-and-scissors-work." It is no doubt a hard and perhaps a dangerous matter to attempt an abstract of what some authors are pleased to term specific or generic "characters"—extending, it may be, over a whole page In such cases by far the best plan would be simply to state that such so-called "characters" are too diffuse for reproduction and too complex for condensation. In this way all chance of misrepresenting an author's meaning—the greatest danger that a recorder has to run—would be avoided, while a gentle hint would be conveyed to a wordy writer that, as it was said of yore, "*verbositas auctoris artis calamitas*." Of course this treatment should not be practised but on proper provocation, or the recorder will justly be held to be angular rather than angelic, and this last is the character he ought to bear, whether he may ever deserve or not the title of the "Recording Angel." To the scientific public we may declare that the present is an occasion when they ought to come forward and freely encourage the new Association. The chemists, we know, are stirring, and very properly, for a similar annual, and we believe our herbaceous brethren would gladly hail a *Botanical Record*. What chance of getting one so good as by showing that the *Zoological Record* can be made to preserve an existence independent of all grants from scientific bodies, though in its time of nonage such extraneous help was still needed?

OUR BOOK SHELF

The Land of Charity. A Descriptive Account of Travancore and its People. By the Rev. Samuel Mateer, F.L.S. (London: John Snow and Co. 1871.)

THIS book is the result of material collected and observations made during nine years' residence of a missionary in our great Indian empire. Its pages are, of course, chiefly devoted to an exposition of the different forms of worship and belief, and the progress of missionary work amongst the native population; but the author acknowledges the claims of Science by giving three chapters to the Natural History of Travancore and the economic uses of the plants of the country. With regard to the Animal Kingdom, we are told "a curious story of a crocodile which attempted to seize a cow that was grazing near its haunts, fastened to a stake by a long rope. The monster had nearly reached the wooden post before it was perceived by its intended victim. On discovering its danger the terrified cow rushed round and round, and the rope caught the crocodile in such a manner as to wind around its body and the post, so that it was held firmly until seen and dispatched by the owner of the cow." Snakes are, as we all know, very abundant in India, but it is curious to know that in 1862, in Bengal alone, 2,394 persons met their deaths from the bites of these reptiles, and it is further estimated that, throughout the whole of India, not less than 10,000 persons annually die from this cause. Though it is a fact that heaps of Indian plants, having no truly recognised economic or medicinal value, are reputed by the natives to cure snake-bites, our author tells us that no certain specific is known, ammonia being probably the most useful medicine in these cases. The cultivation of the Tapioca plant, which is a native of S. America, seems to be spreading in India. The best Tapioca comes into this country from Rio de Janeiro, but large quantities are also shipped from Singapore, the plant being cultivated in the Straits on account of its commercial value. It appears also to be grown in Travancore, and yields the natives an abundance of wholesome food. In districts where water is scarce, or in times of drought, they almost exist on the roots. It would appear, from the remark made by the author of the presence of a poisonous juice in the roots, that only one species of the Tapioca or Cassava plant is grown, and that the *Manihot utilissima*, Pohl, or bitter Cassava, the poisonous properties of which, however, are thoroughly dispelled by heat.

J. R. J.

The Food, Use, and Beauty of British Birds, &c. By C. O. Groom-Napier, F.G.S., &c., author of "The Book of Nature and the Book of Man," "Tommy Try," &c. New Edition, 18mo, pp. 88. (London, Groombridge, 1870.)

THE author says that his book was, by an eminent living anatomist, termed a "delightful concentrated essence of British Ornithology;" but it appears that the British public were so wilfully blind to its merits that they refused to buy it, for the so-called "new edition" is simply a re-issue of the original unsold sheets with a new title-page, a little padding in the shape of "opinions of the Press," and a new preface, to eke out which the Sea Birds' Preservation Act has been introduced. The table of contents, and, still more, the list of errata which were to be found in the old issue, have disappeared from the new one, as also has the photograph representing the author in Mr. Miles's starling-haunted shrubbery. The British public, we think, will ratify their former estimate of this book. A good work might certainly be written on the food and use of birds, and, as we know, many volumes are devoted to the illustration of their beauty, but with all deference to the author of "Tommy Try," we doubt if he is the man for the task.

The Forces of the Universe. By George Berwick, M.D. Pp. 127. (London: Longmans and Co., 1870.)

WHAT could possibly have induced the author to "offer this work to the general public" (preface) we cannot tell, unless we are to attribute it to a "sudden seizure of enthusiasm." These sudden seizures, together with "revolutionary movements," "religious revivals," "insane wars" and "sunstroke," arise, as we find on p. 124, from similar causes, and these, it seems, are more than probably due to "an aberration of the normal molecular action of the nerve-centres, or an increased electrical tension or polarisation of the nervous element in the brain itself."

The aim of the work seems to be an attempt to prove the identity of electrical attraction and the attraction of gravitation. The author confesses that the idea is not original, but we may add that his arrangement, to say the least of it, of the English language certainly is, *e.g.* p. 43—"It has been ascertained that the deeper that we descend into the bowels of the earth, the temperature increases at the rate of," &c. On p. 41, "moreover, the diversified configuration of the earth's surface, formed, as it is, of seas and continents of lands, &c., all which physical conditions having different powers of radiating and absorbing heat, also have the power of causing great local disturbances in this aërial ocean."

It is probably due to a mistake on the part of the printer that the pressure of the air in the latitude of Paris at the level of the sea has risen to 147,304 lbs. per square inch, but we cannot quite account for the statement on p. 39, that the earth revolves daily on its axis "from east to west," or again on p. 12, where common salt is made to consist of sodium and chlorine in "*equal* proportions."

The lines of force in a magnet have often been likened in shape to a double egg-cup, but we doubt if any of our readers have ever seen anything so charmingly *àpropos* as the following similitude. "They (the lines of force) also unite and cross over the equator of a magnet in the same manner that the peripheral nerves of animals decussate over the pons Varolii, and again separate to the opposite hemispheres of the brain." The whole book is written in an absurdly inflated style, such as, for instance, p. 88—"The intensified electricity in these regions will rather pour its fitful beams over the serrated edge of the circular icy continent," &c.; or p. 59—"The blood and the nerves and the muscles that composed his (man's) fabrication (!) moulder into dust, which, in the crucible of time, yields up protoplasm for vegetation." On the last page electricity "carries faithfully the thoughts of men far along the profoundly silent abysses of the deep blue sea." Space, however, compels us to take leave of Dr. Berwick's "bright plateau of cultivated intellectual existence."

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

Botanical Museums

IN your excellent article on Botanical Museums, one point appears to have been overlooked, and to which, with your permission, I should like to call attention. I allude specially to the want of an extensive series of carefully prepared specimens and dissections illustrative of the principal modifications of form and structure to be met with in plants. To the ordinary student of botany, especially to the beginner, a series of herbarium specimens conveys about as much information as a similar collection of postage stamps would do. It is not until the pupil has made some considerable progress that he is in a position to make use of herbarium specimens with advantage to himself for anything more than superficial examination. The supply of fresh specimens in a large town like London is necessarily limited, if not in point of numbers, at least in variety. Would it not therefore be advisable in any future re-organisation of our botanical museums, to meet this want so far as it is possible to do so? A collection, such as I am alluding to, should comprise specimens selected and displayed in such a manner as to show the principal variations in the structure and form of the several organs of plants from the lowest to the highest. It should illustrate, so far as circumstances will allow, the comparative anatomy and physiology of plants much in the same way as the Hunterian Museum of the Royal College of Surgeons illustrates the peculiarities of animal life. In such a museum the system should be subordinated to the plants, not the plants to the system. I am quite aware that in all three establishments to which you call attention some specimens of the kind I refer to are to be found. In the Edinburgh Botanical Museum also are to be seen models and preparations made under the superintendence of Professor Balfour by several of his pupils. It is such specimens as these that for educational purposes it is so desirable to multiply and collect together in a separate department. Where, from the nature of things, such as the delicacy or minute size of the organ or what not, it is not possible to prepare a satisfactory specimen for reference, large models in wax or papier maché might be substituted with advantage. What teacher who has had to initiate the tyro into the complexities of the sphenoid bone or the disposition of the ovules, the structure of the anther, the development of the flower, the arrangements of the flowers in grasses, &c., but has longed for Brobdignagian models whereon to demonstrate the peculiarities of their formation. The organs themselves are often so small, and require so much practice with the use of the dissecting needle before they can be seen by the student, that it is very desirable to aid his preliminary labours as much as possible; to give him, at least, a general idea of what he is to look for in the living specimen. For want of this preliminary help specimens are often wasted by the inexpert pupil, who becomes disgusted because he is unable to see for himself what his books or his teachers tell him he ought to see. A good collection of microscopical preparations should also be provided to illustrate such points as require the use of the compound microscope. Probably the British Museum would be the most fitting place wherein to exhibit such specimens side by side or in conjunction with those illustrative of fossil plants. If some such plan as that hinted at in your article were adopted, we should have the general collections at Kew in conjunction with the gardens and Economic museum, the historical and structural collections at the British Museum, and the Trade Museum at South Kensington. I cannot conclude this letter without adverting to the facilities which exist at Kew for the determination of unknown plants, and particularly of plants cultivated in gardens. Thanks to the admirable arrangements made by former and by the present curator of the herbarium and their assistants, the determination of an unknown plant becomes, comparatively, an easy matter.

MAXWELL T. MASTERS

Gardener's Chronicle Office, March 27

Occurrence of Glutton near St. Asaph

A CAVE has long been known to exist close to Plas Heaton, the property of Mr. J. R. Heaton, but as it was filled with brown earth nearly to the roof, and the entrance obstructed by large blocks of limestone, it could not be explored without some labour. Mr. Heaton has recently commenced opening it, and, among a large number of bones, has been rewarded by finding part of a jaw, which has been determined by Mr. Boyd Dawkins to be that of a glutton. This is a discovery of very great interest, as occurring in the district where we have already found the remains of reindeer, elk, &c., upon which the glutton principally feeds. The cave is situated on very much higher ground than any of the other bone-bearing caves of the district, and runs down into the hill with the bedding of the rock. Where the other end may be there is as yet no evidence to show, but it promises to be a cave of great extent, and, judging by the festoons of stalactite already arrived at, of great beauty also. Its chief interest, however, lies in the strong probability, from its size and position, that it will contain a very full record of the early natural history of the district, and the first results certainly encourage further exploration.

T. McK. HUGHES

Splendid Meteors

ON Saturday evening last, March 25, at about half-past nine local time, I happened to be observing some stars in the eastern quarter of the heavens, when I was astonished by the sudden appearance of a brilliant meteor with a long tail, or streamer, of a reddish hue. The colour of the ball itself was a vivid bluish white. It seemed to start from near ε *Virginis*, or a little to the right of that position, and to take a leisurely course in a straight line towards the north under *Arcturus*, α *Coronæ Borealis*, and ε *Herculis*, till I lost it behind some houses not far from the northern point of the horizon, if anything, a little to the east of that point. I was most struck with the leisurely pace at which it moved, so different from an ordinary falling star, the velocity appearing to slacken as it proceeded, like that of a railway train after it has passed a spectator. Just when passing under *Arcturus*, the globular head broke up, not unlike one of the fireballs of a rocket, into a string of five or six luminous beads, getting smaller and smaller towards the tail. The entire length of the meteor seemed to be fore-shortened as it receded towards the horizon. Judging without a watch, I estimated the interval between its appearance and disappearance to be about nine seconds. At the same time a second meteor of inferior dimensions and briefer duration took a somewhat parallel course between *Bootes* and *Ursa Major*.

EDWIN SMITH

Forest Road, Nottingham, March 26

A few minutes after sunset on Thursday last (23rd) I saw a very fine meteor in the direction E.N.E.

Its course at first appeared southerly, then, bending downwards, it seemed slowly to drop in a direction perpendicular to the horizon, and I lost sight of it behind a clump of trees. Its light was exceedingly brilliant.

J. F. Duthie

Leyton, Essex, March 27

Books Wanted

The only way in which your correspondent of last week, Mr. H. J. Watson, is likely to obtain the volume of the "Annales de Chimie," containing the paper of Braconnot, (not Bracconot, the misprint is Sir John Herschel's), and that of the "Philosophical Transactions" with Dr. Prout's paper, would be to request some second-hand scientific bookseller to place them on his list of "desiderata," so that they may be brought under the notice of those who are likely to have duplicates.

Perhaps Mr. Watson is not aware that there is a good abstract of Braconnot's paper, occupying just over six pages in the *Quarterly Journal of Science*, vol. viii. 1820, and also short abstracts in the *Edinburgh Philosophical Journal*, No. iv. 1820, and Tulloch's *Philosophical Magazine*, vol. lv. 1820. It has also been reprinted into some foreign scientific periodicals, a list of which may be seen on reference to the "Royal Society's Catalogue of Scientific Papers," vol. i.

Jas. B. Bailey

Oxford, March 24

Measurement of Mass and Force

Professor Everett, towards the close of his much-needed and exhaustive letter in Nature (March 2) on the Measurement of Mass and Force, proposes to supply an undeniable want in Dynamics by coining the word *kinit* to denote that force which, acting on an avoirdupois pound of matter for a second, generates a velocity of a foot per second. He then adds, "If we substitute gramme for pound and metre for foot, we obtain a different unit which must be called by a different name."

Now one, and perhaps the only, objection to this is that in the face of the rapidly spreading metrical system it seems injudicious and somewhat savouring of retrogression to appropriate the most suitable root at our disposal to designate a force-unit depending upon the *pound* and *foot* (the abandonment of which is now only a question of time), while that based on the gramme and metre is sent a begging. I would therefore suggest to Prof. Everett that a *kinit* (or simply the monosyllabic *kin*) be defined as *that force which, acting on a gramme of matter for a second, generates a velocity of a metre per second;* and then there would follow as a matter of course *kilokin*, &c., suggestively and conveniently denoting either "the amount of force which, acting on a *kilogramme*, &c., of matter for a second, generates a velocity of a *metre* per second," or, "the amount of force which, acting on a *gramme* of matter for a second, generates a velocity of a *kilometre*, &c., per second."

Besides Kinetics and Heat (see Nature, vol. i. p. 606) there is another department of science where a similar want exists—less pressing, perhaps, as yet, but felt, nevertheless. This is Electricity. Taking as basis Sir W. Thomson's general definition of unit quantity of frictional electricity (*Camb. and Dubl. Mathematical Journal*, March, 1848) a particular unit (*i.e.* a unit dependent upon previously fixed particular units of force and distance) might be chosen, formally defined, and named.

This question of units of measurement and their names appears to me to be anything but trivial. A science, which, by the choice of *one good system of units*, and the adoption of *a suggestive definite and uniform nomenclature*, has put its house in order for the proper reception of the powerful chief Mathematicus, has laid the foundations of true and rapid progress.

College Hall, St. Andrews

Thomas Muir

The Earthquake

A rather severe shock of earthquake was experienced in this neighbourhood on Friday, March 17. The day in question had been remarkably calm, and a heavy suffocating feeling in the atmosphere noticed. About 11.15 P.M. a somewhat loud rumbling noise was heard as if a heavy waggon was passing over pavement; windows, chandeliers, furniture were violently shaken. Cups and saucers made themselves heard, and beds in some cases were distinctly felt to oscillate and heave like a ship at anchor. Breathing in some cases became difficult, but whether from fright or the oppressive state of the atmosphere does not appear. The vibrations were apparently horizontal, and probably in a direction from N. to S., lasting about three or four seconds. Poultry and cage birds showed particular distress by the noise and fluttering which they made. The temperature, which on the previous Tuesday night had fallen as low as 17°, suddenly changed, and the minimum of Friday night was 41°. Saturday morning was remarkably warm; the black-bulb thermometer in vacuo reading 92°. The barometer showed no unsteadiness, but had been gradually rising for some days previously. The shocks seems to have been felt from the south of Scotland as far as the north of Derbyshire, much the same account having been received from each locality.

Thomas Fawcett

Blencowe, Penrith, March 20

The earthquake recorded for the night of Friday (17th) last was felt here about 11 P.M., distinct vibration being observed by two members of my household.

In support of the theory that shocks are mainly noted along lines of fault, there is a considerable one within a short distance extending northwards for some miles.

Charles Henry Middleton

Lingen Vicarage, Presteigne, Herefordshire, March 23

A slight shock of earthquake was felt and heard in this neighbourhood at about 9.55 on Monday night last, the 20th instant.

The duration of the shock was not longer than three seconds; it was accompanied as if by a muffled explosion, followed by a slight rumbling of the earth, and a gradual dying away of the sound, which seemed to be in an easterly direction. The last sensation is obviously not very reliable, as much will depend upon the position in which the hearer was sitting.

Henry Cooper Key

Stretton Rectory, Hereford, March 23

The Reality of Species

Among the many misconceptions that have arisen in connection with the doctrines of evolution appears to be one that species have no real existence. In a recent review (appearing in one of the best London papers) of Mr. Mivart's work, this mistake is strongly expressed, the writer appearing to entertain a profound contempt for anyone who still retains the foolish notion that there is any such thing as a species in nature. Every working naturalist knows well that most assuredly species do exist, and that in the most positive manner, not being conventional merely, but separated from one another in nature by distinct and real characters.

Fortunately for the doctrine of natural selection, it does not in the least question this fact, for did it so, it would be disposed of at once by pointing to a red admiral and tortoiseshell butterfly flitting side by side. It cannot be too distinctly insisted on that natural selection opposes no barrier whatever to the reception of the idea of distinct and separate species. That which it has destroyed is the notion of the constancy of species if the idea of time be set on one side. To argue that species have at the present day no separate existence because they had formerly a common origin, is a foolish confusion. The separate existence of a full-grown and mature animal is not questioned, because at one period it was a bud closely connected with its parent. In point of fact the question of species is really very similar to that of individuality, viewed as a question of origin, the individual and the species are both untenable ideas; but viewed at any one moment, both individual and species are among the most prominent and undoubted facts of our experience. Equally futile is it to argue that species have no existence, because we cannot exactly define what we mean by a species. It is well known that all the efforts of biologists have hitherto failed to produce a satisfactory definition of life. Are we, then, to conclude there is no such thing as a living animal?

The evolutionist contemplates throughout the universe a power underlying all things, indestructible and infinite, most various in its manifestations, always changing and always shifting, but steadily in a given direction, not revealed to man as a separate existence, but known only by its changes and movements, and veiled under the form of matter. Side by side with this universal and unknowable force he sees an opposing power, a tendency in things and matter to be always as they have been, a tendency which the restless force has ever to overcome; but as soon as this has gained its victory, again is it subject to the grasp of its ignoble foe; the struggle, though becoming ever more and more one of detail, is no spasmodic one, though more revealed to us in some phenomena than in others, and more evident at some moments than at others. The questions of the origin, the existence, and the value of species in such a system are easily appreciated.

D. SHARP

Thornhill, Feb. 23

The Preponderance of West Winds

IN NATURE of the 16th inst. you appear to contest Mr. Laughton's statement that west winds preponderate over east on the entire globe. I believe that Mr. Laughton is right as to the fact. We have no reason to think that the earth's atmosphere is acted on from without by any force except the sun's heat; if this is the case, the winds can have no effect whatever in either accelerating or retarding the earth's rotation; and, conseqently, the east and west winds must exactly balance each other's effect, for if either were unbalanced it would have an effect, however small, on the earth's rotation. But "an east wind near the equator has more effect in retarding the rotation of the earth, than a west wind of equal extent and force at a higher latitude in accelerating it, just as a weight at the end of the long arm of a lever outweighs an equal weight at the end of the short arm. It is for this reason that the west winds, which are mostly in the higher latitudes, are of greater force, and probably cover a greater area than the east winds, which, under the name of trade winds, predominate near the equator."

This quotation is from a letter of mine published in NATURE of 16th Feb.

In the same number of NATURE there is a letter from Mr. Laughton, maintaining that rain may be caused by fires or explosions. This is confirmed by a fact mentioned by Humboldt in his *Cosmos*, that he once saw an eruption of the volcano of Cotapaxi in the Andes, during which the cone became red hot, and rain fell at an unusual time of the year.

In the same number is a most interesting account of the Winter Meteorological Observatory on Mount Washington in New England. Mount Washington must be much more isolated than mountains 10,000 feet high generally are; and I hope the opportunity may not be lost of making comparative barometric observations, extending over a considerable time, at the summit and the base. In NATURE of 19th January, you published a letter of mine on the importance of such a comparative series.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, March 18

Morell's Geometry

IT was with no small surprise that I found myself accused by Mr. J. R. Morell, more than once in last week's NATURE, of having overlooked the fact "that all the proofs in the work ('Essentials of Geometry') are taken from French and German sources." Nothing to my mind could be more obvious [than that throughout my review in NATURE for Feb. 23, I was criticising the performance of a compiler. In one instance, indeed—Mr. Morell has surely not already forgotten it—I took the trouble to show *how* Amiot's demonstration of the fundamental properties of parallels had been mutilated by him. I must protest, too, against his claim to freedom from censure on the ground that he has merely copied passages from the works of our highest authorities; for it is about as reasonable as a claim to sanctity would be on the part of one who habitually, it is said, quotes Scripture for his own purposes.

Utterly ignoring the italics which I introduced, to save comment, into his definition of a plane angle, Mr. Morell quotes the Greek of Euclid and the English of Thomson, in justification, apparently, of the aptness of the introduction of the notion of revolution, which no one contested. He compels me, therefore, to draw his serious attention to the fact that neither of these geometers, nor any other to my knowledge, ever confused mankind as he has done by speaking of the "inclination of two straight lines *to a common point.*"

Mr. Morell has, lastly, the audacity to defend his pretended demonstration, on p. 44, of the theorem that two triangles are equal in every respect when the sides of one are respectively equal to the sides of the other, by stating, *first*, that it "is based on the previous pages (42 and 43), overlooked by the Reviewer;" and, *secondly*, that it "agrees almost word for word with Legendre." The first of these statements is absolutely incorrect; on pages 42 and 43, there is not a word upon which the demonstration in question *could* be based. With respect to the second statement, I admit that Mr. Morell's demonstration substantially differs from Legendre's, as given by Blanchet, in Prop. xi. Bk. 1, only by the omission of four words; and to show, by a striking example, what mischief scissors can do in Mr. Morell's hands, I will supply the four missing words, between brackets, in the following reproduction of his demonstration:—

"Let ABC, DEF, be two triangles, having AB = DE, AC = DF, BC = EF, then angle A = D; for if they were unequal, sides BC and EF would be unequal [by the previous proposition]. Therefore A = D."

The fact that Legrendre's most essential previous proposition is *nowhere to be found in* Morell's "Essentials of Geometry," sufficiently accounts for the omission of the four words above inserted. Mr. Morell, however, has yet to realise the fact that these omissions of his have converted a genuine demonstration into a mere assertion, or rather into a flagrant "violation of the most obvious of all logical rules."

Mr. Morell threatens, if space be given him, to show, on his "own authority," that he has "good arguments for what has been advanced." Before he does so let me remind him that logical demonstration, like the multiplication table, is more a subject for direct apprehension than for argument.

THE REVIEWER

A Meteorological Question

WHILE glancing the other day over the article "Meteorology," in the Supplement to the 4th, 5th, and 6th Editions of the *Encyclopædia Britannica*, I was surprised to find under the marginal heading, "Hauhber's Experiment," the following:—"It is conceived that a current of air in sweeping over the surface of the earth, must cease to exert any vertical pressure. But this assumption can hardly be reconciled with any strict principles in science, for the particles of air will not for a moment cease to gravitate, nor will any horizontal motion of them produce the slightest derangement in a perpendicular direction." Is not this a great mistake?

QUÆRE

A SUGGESTED NEW DIVISION OF THE EARTH INTO ZOOLOGICAL REGIONS

IT seems now to be generally agreed among zoologists who are specially conversant with the fauna of India, that "the Indian Region" of Dr. Sclater and others can no longer be regarded as a genuine or natural zoological division of the globe, and that India properly so called (from the Himálya to the sea), is rather a border territory where different zoological regions meet and are variously interposed, at the same time blending, as a matter of course, to some extent.*

This is a subject which has long occupied my thoughts, and I am gradually arriving at the opinion that the present dry land upon our planet may be most naturally divided into seven zoological regions, which again are divisible into sub-regions, and these into provinces and sub-provinces.

I. *The Boreal Region*, which is divisable into: 1. Arctic Sub-region, within the confines of the Arctic Circle, but also inclusive of the whole of Greenland, and of Foxland (west of Davis Strait and north of Hudson Strait). 2. Neo-septentrional Sub-region—North America. 3. Neo-meridional Sub-region—Central America with the Antilles. 4. Andisian Sub-region—the chain of the Andes with Chili, Patagonia, and the Fuegian and Falkland Archipelagos. 5. Palæo-septentrional Sub-region—Europe and Asia south of the Arctic Circle, and north of the Pyrenees, Alps, Taurus, Elburz (south of the Caspian Sea), Hindu Kosh, and Western Himálya, extending from the British Islands to Northern Japan? 6. Palæo-meridional Sub-region—the countries adjacent to the Mediterranean, as Africa north of the Atlas (with Madeira, the Canaries, and the Azores), Spain, Italy, Dalmatia and Illyria, Greece, the islands of the Mediterranean and the Levant, Turkey, Asia Minor,

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* Vide Mr. W. T. Blanford in the "Proceedings of the Asiatic Society" for September 1867, p. 145 "The fauna of India at the present day is a remarkable mixture of African and Malay forms. The idea, so commonly expressed in European books, of India belonging to the same geological [qu. zoological?] province as the Malay Peninsula and Southern China, is quite erroneous." Vide also the same "Proceedings" for January 1868 p. 18, January 1869, p. 40, and July 1870, p. 238.

Syria, Egypt, Palestine, Mesopotamia, Persia, N. Afghánstân, Pánjâb, Middle China, Southern Japan? 7. Mongolian Sub-region—Mongolia, Tibet, and Chinese Tartary.*

II. *The Columbian Region.*—South America, minus the chain of the Andes and the extreme south. Divided into —1. Brazilian Sub-region; the forest countries east of the Andes. 2. Pampian Sub-region; the Pampas territory. 3. Peruvian Sub-region; Bolivia, Peru, Chili, and the Gallápagos Archipelago.

III. *The Ethiopian Region.*—Africa, south of the Atlas and of Egypt. Divided into—1. Lybian Sub-region; extending from Senegal to Nubia and Arabia, the country bordering on the head of the Persian Gulf (Mekran), S. Afghánstân, Beluchistân, and the desert country of N.W. Hindustân. An outlying strip of this sub-region extends along the cleft, or wady, continuous with the Gulf of Ormuz, in which is situate the depression of the Dead Sea and beyond it the Valley of the Jordan. 2. Nigritián Sub-region; Negroland. 3. Caffrarian Sub-region; Southern Africa. 4. Indian Sub-region; Hindustân proper, or the plains of Upper India E. and S. of the N.W. desert; Dukhun, or table-land of the peninsula of India; and the intervening territory, inclusive of the Vindhaian ghâts; Coromandel coast; low northern half of Ceylon.

IV. *The Lemurian Region.*—Madagascar, the Mascarine Islands, Seychelles, &c. Probably an extensive region of dry land formerly, but now for the most part submerged, where the coral formations occupy so extensive an area.

V. *The Australasian Region.*† The Indo-Chinese peninsula, together with the Indo-Malayan region of Wallace; the southern watershed of the Himályas, at least up to the zone of oaks and rhododendrons, and jungle-clad hill country of Southern India and of Ceylon, if not also certain fertile hill territory in Southern Arabia; Lower and Eastern Bengal; Philippine Islands; Chinese islands of Hainan and Formosa (minus its mountain spine), and probably so much of the south of China as is inhabited by the *Manis* or pangolin, the *Palæornis cyanocephalus*, the genus *Centropus*, and other Australasian and Ethiopian forms. Divided into: 1. Indo-Chinese Sub-region—extending southward over one-half of the Malayan peninsula, as far as Pinang and Province Wellesley; Hainan and lowlands of Formosa, and more or less of the southern part of China. 2. Malayan Sub-region—southern half of the Malayan peninsula, Sumatra, Banka, Borneo, Java, and Bali. 3. Philippian Sub-region (which has Melanesian affinities, as indicated by the presence of a peculiar species of cockatoo—mammiferous animals few in number), Philippine Islands. 4. Himályan Sub-region—the southern watershed of the Himályas, with the *tárai* region at its base, Asám, and Eastern and Lower Bengal (*i.e.* the Sundarbáns). 5. Cinghalese Sub-region—the hilly parts of Ceylon (occupying the southern half of the island), those of southernmost India, and the Malabar ghâts; perhaps also the little-known fertile mountain territory of Southern Arabia, from which Mekka is supplied with grapes and other fruit.

VI. *The Melanesian Region.*—Divided into: 1. Australian Sub-region—Australia (minus York peninsula and part of Queensland), Tasmania. 2. Papuan Sub-region—Papua, New Britain and New Ireland, Jilolo or Halmahira, Ceram, Buru, Moluccas, Aru Islands, and Timor-Lât; Louisiade Archipelago; York Peninsula and eastern half of Queensland (as far as the dividing range) on the mainland of Australia. 3. Celebesian Sub-region—the very remarkable island of Celebes, which has Australasian affinities, but subordinates to the present region: Islands of Lombok, Sumbáwa, Flores, Wetter, Timor, and Sandalwood Island. (Austro-Malayan region of Wallace.) 4 Antarctic Sub-region, inclusive of Kerguelin's Land.

VII. *The Polynesian Region.*—Divided into: 1. Moarian* Sub-region—New Zealand, with the islets appertaining to it, inclusive of Macquairie Island (upon which far southern land a peculiar species of ground-parrakeet inhabits, of a Polynesian genus—*Cyanorhamphus*). 2. Polynesian Sub-region—comprehending the Archipelagos of the Pacific, excepting those which appertain to the Columbian region.

All of these Zoological Divisions of the dry land upon the surface of our planet might be amply illustrated by an enumeration of the species and genera, or even higher groups, which are respectively peculiar to them. Thus, the presence of the true raven (*Corvus corax*) exactly coincides with the limits which are here assigned to the vast Boreal region, even to the Indian Pánjâb, and assuming that the so-called American species (*C. carnivorus* and *C. mexicanus*) do not really differ, which I believe to be the case, as likewise the so-called *C. tibetanus*; the sole exception being that of the Andisian sub-region, inasmuch as there is no *Corvus* in all South America. Be it always remembered that the major and the minor continents approximate in the Northern Pacific, to say nothing of the connection between them afforded by the chain of the Aleutian islands, and that a remarkable wild mountain sheep (*Ovis canadensis*) inhabits both sides of the Pacific, and not only the peninsula of Kamchatka, but the Stannavoi mountains which lie west of the sea of Ochotsk.

The southern watershed of the Himálya (below at least the zone of oaks and rhododendrons) consists, decidedly, of an extension westward of the Australasian region, and in the bird-class its connection with India is maintained chiefly by migratory species; and very many of the permanently resident species, which have been thought to be specially characteristic of the Himálya, are equally found more or less throughout the Indo-Chinese sub-region, and not a few of them even in the Malayan sub-region, although nowhere met with in India, properly so called (the extensive *Liothrix* series of birds, for instance), while a few of them reappear as the same or as closely proximate (little altered) species in the hilly parts of Southern India and of Ceylon. On the other hand, as may be generally remarked of bordering sub-regions of different regions, there are some cases of mutual representation in the Himályan sub-region of the Australasian region, and the Indian sub-region of the Ethiopian region. The lángur monkey (*Presbytes schistaceus*) of the Himálya; is thus a specialised form of the hunumân group of India, exemplified by the Bengal hunumân (*P. entellus*) and others, this being a characteristic Indian division of the genus which has no representative eastward of the Ganges.

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* It may seem a bold idea to put forward, but I have an exceedingly strong impression that the Stannavoi mountains, and the country eastward of them, the Valley of the Amur and all Mantchuria, with the Korean (or Koriak?) Peninsula, the country of the Tchuktchi who, though pastoral, are veritable Eskimo, (I adopt the spelling of Sir J. Richardson), the peninsula of Kamchatka (as Mr. F. Whymper spells it), the Kurile Islands, the Japanese Archipelago in part, and the Island of Saghalian or Sankhalin, which separates the Gulf of Tahtary from the Sea of Okhotsk, together with the Alaska territory, *eastward* of Bering's Straits (constituting part of the mainland of America) appertain alike to my Neo-septentrional sub-region, however the area may be subdivisible into provinces and sub-provinces; the Palæo-septentrional, the Neo-septentrional, and Arctic sub-regions being there variously interposed, with a certain amount of blending as in all such cases. Be it remarked that the *Ovis canadensis* is identical on the Stannavoi Mountains, those of Kamchatka, and the Rocky Mountain chain in North America, also that the insectivorous genus *Urotalpa* (even the species apparently) is identical in Western North America and Japan, and there are very remarkable ethnological affinities pervading the entire area, to which it is sufficient here thus cursorily to allude. Indeed, the Japanese themselves are more nearly akin to the Columbians (or redskins, the so called "American Indians") than to the Asiatic nations of pronounced Mongolian type, to which latter the Eimo of Yeso and kindred Kamchatkadales strictly appertain. The *Ovibos moschatus* (or musk-sheep) is asserted to exist on the Island of Saghalian! May not the species prove, however, to be *Ovibos Pallantis* of De Blainville, supposed hitherto to be extinct, rather than *O. moschatus* of the Arctic-American Barren-grounds? If I mistake not (writing from memory) the *O. Pallantis* is identical with the North American *Bootherium bombifrons* of Trans-Atlantic palæontologists. *O. moschatus* would appear at one time to have been distributed over my Palæo-septentrional sub-region even to the area now constituting the British islands. The present Arctic sub-region must at that time have been co-extensive with the existent Palæo-septentrional if not also the existent Neo-septentrional sub-regions. Together with *Ovibos moschatus* on the Arctic American Barren-grounds, the European and Asiatic *Ursus arctos* there exists, which is significant of a former connection with the major continent! The Barren-grounds physically resemble the "Tundras" of Siberia and likewise mountain Lapland. The more that I reflect upon what is known of the fauna and flora of the debatable land in the extreme N.E. of Asia, the more thoroughly do I feel convinced that the Stannavoi mountains constitute the true and real boundary between Asia and America. Southward of the Stannavoi mountains, in Mantchuria, the two continents blend. The Japanese archipelago belongs to America, and not to Asia; at least in great part, as indicated by the presence of the *Urotalpa*; but there is also a true *Talpa* indicative of both Palæo-septentrional and a Palæo-meridional relations, and *Phasianus versicolor* and *P. Soemmeringii* indicate the latter, while many of the insessorial birds of Japan indicate the former, and the bulbul of the genus *Microcelis* indicates an Australasian element, so that the Japanese Archipelago is, after all, a debatable land where different zoological regions and sub-regions meet and blend more or less. *Cervus pseudaxis* of the mountain spine of Formosa, so nearly akin to the much smaller *C. sika* of Japan and the much larger *C. mantchuricus* of Mantchuria, again indicating the propriety of recognising a Japanese or Korean province of the Neo-septentrional sub-region of the grand Boreal region. All qualified botanists will surely bear me out in this opinion.

† Austral-Asia as distinguished from Australia: approximately the same as "the Indian region" of Dr. Sclater, but shorn of the greater part of *India* properly so denominated.

* I propose Moaria rather than Maoria, following Dr. Sclater's example of Lemuria; naming the land from the indigenous "moa" genus (*Dinornis*) rather than from the present Maori inhabitants. Of course, it is well known that in the Polynesian languages "moa" merely signifies a fowl, but it has become specialised since the discovery of the extinct *Dinornis* genus by Europeans.

India, properly so called, is a land where sundry sub-regions appertaining to different regions meet, and are variously interposed. The Palæo-septentrional sub-region of the Boreal region extends into the Pánjâb, while the Mongolian sub-region borders upon the S.E. Himálya; the southern flank of the Himálya constitutes the Himályan sub-region of the Australasian region, to which, perhaps, should be referred (as distinct provinces) the mountains of Southern India and of Ceylon; then the Lybian sub-region of the Ethiopian region extends as far as the desert country N.W of Delhi; and the rest of India, with the low northern half of Ceylon, constitutes the main part of the Indian sub-region of the Ethiopian region. The N.W. Himálya again passes northward into the Palæo-septentrional and southward (in the alpine Pánjâb) into the Palæo-meridional sub-regions. In a S.W. direction the Máldive and the Láccadive coral-islands belong strictly to the Lemurian region of Dr. Sclater, and I am not sure that the latter does not reach the mainland of India, to comprehend the Concan or low maritime country constituting the Malabar coast, and lying along the foot of the ghâts. India, therefore, instead of being the nucleus of a distinct zoological region, is a land of extraordinarily complex zoological affinities.

As regards North America, it may be observed that the migratory insessorial birds are of Columbian types, whereas the permanently resident species are of types cognate with those of corresponding latitudes in the major continent: as we likewise find, in Europe, that our feathered summer visitants are of tropical or juxta-tropical forms, as exemplified by the roller, bee-eater, cuckoo, oriole, and the mass of small insectivorous genera; and the same has already been remarked with reference to the Himálya, viz. that as concerns the bird class, the connection of the southern flanks of the Himálya with the plains of India is chiefly maintained by the species which migrate to and fro. Among the gallinaceous birds of North America, the turkeys (*Meleagris*) are the only fowls which have spurred tarsi, indicating their affinity to so many of the major continent genera of poultry-birds, while the partridges of the same sub-region (*Ortyx* and *Lophortyx*) are not more different from major continent forms than are many of the latter from each other. The turkeys are assuredly not more peculiar in any respect than are the peafowl and the tragopans (*Ceriornis*) of Asia, and the same holds true of the North American colins or partridges, even admitting the affinity of the latter for the *Odontophori* of South America. The northern continent, however, has nothing corresponding to the curassows and guans (*Cracidæ*), or to the tinamou (*Tinamidæ*) of the Columbian region, to its great family of toucans (*Rhamphastidæ*), motmots (*Momotidæ*), jacamars (*Galbulidæ*), puff-birds (*Bucconidæ*), cariamas (*Cariamidæ*), trumpeter-birds (*Psophiidæ*), or its nandous (*Rhæidæ*); and of the enormous family of humming-birds (*Trochilidæ*) it has only some four or five species as seasonal immigrants! Again, in the class of Mammalia it has no living representative of the *Edentata*, so characteristic of the Columbian region (though it did formerly possess the *Megalonyx*), nor of the marsupial true opossums (*Didelphidæ*), save one species only in the southernmost Atlantic States of the Union; but it has a fair proportion of *Insectivora*, which in South America the *Didelphidæ* completely replace. The rodent families *Chinchillidæ* and *Caviidæ*, I look upon as Andisian forms, even though the viscacha (*Lagostomus tetradactylus*) represents the former in the Pampian sub-region of the Columbian region. The mammalia of the Neo-septentrional sub-region of the Boreal region are surely not more different from those of the Eur-Asian sub-regions of the same region, than are those of the latter from one another, say the Mongolian sub-region from either of the rest. With regard to the Andisian sub-region, I am mainly induced from a consideration of its extinct mammalia (so different from those of the Pampas) to consider it as a southern extension of the American portion of the grand Boreal region, and especially from the occurrence not only of such an animal as the *Mastodon andium*, but also of the living llámas and alpácas (*Auchenia*), which have no other known kindred, existent or extinct, than the camels of the major continent. Nevertheless, as usual in bordering sub-regions of different regions, there is an interposition of forms to a certain extent, as illustrated, on the one hand, by the existence of the viscácha on the Pampian sub-region of the Columbian region, and by that of the edentate *Chlamydophorus truncatus* in the Andes.

The Neo-meridional sub-region of the Boreal region has much stronger affinities for the Columbian region upon which it borders than has the Palæo-meridional sub-region of the Boreal region for the Ethiopian region and for the Australasian region upon both of which it borders, which of course is attributable to its nearer proximity to the Equator bringing it within the influence of the tropical and periodical rainfall. The several regions which are subject to that rainfall in different meridians hold relations of analogy with each other, *i.e.* the Nigritian sub-region of the Ethiopian region, the Lemurian region, the whole Australasian region, the Celebesian and the Papuan sub-regions of the Melanesian region, and the Peruvian and Brazilian sub-regions of the Columbian region. Then, southward, in corresponding latitudes beyond the influence of the tropical rainfall, the same analogies hold between the Andisian sub-region of the Boreal region, the Pampian sub-region of the Columbian region, the Caffrarian sub-region of the Ethiopian region, and the Australian sub-region of the Melanesian region; while, again, south of the parallel of 40° S. lat., the analogies (though still considerable) between the Patagonian sub-region and those parts of the Australian sub-region and the Moarian sub-region which fall within the boundary indicated are, to some extent, less prominently marked. *Ptilopachus* and kindred strong-footed passerine birds in Patagonia, nevertheless, most readily call to mind *Menura* and *Orthonyx* in Australia, and *Mohoua* in Moaria or New Zealand.

Having submitted these views I reserve for another occasion the consideration of other classes of the animal kingdom; premising, however, that I am well aware of such facts as the utter absence of the *Cyprinidæ* (or carp family) in all America eastward of Bering's Straits, a group of fishes which is so immensely developed in S.E. Asia. So far as the classes of mammalia and birds are concerned, I think that I have about hit upon the true classification of zoological regions, and I wish, before returning to the subject, to avail myself of the critical remarks of competent naturalists having reference to all classes.

E. BLYTH

THE PLANET JUPITER

SINCE the earliest days of telescopic investigation, there has never been a period in which this magnificent planet has been subjected to such an extended scrutiny as the present. Telescopes of all sizes and powers, and eyes of all degrees of sensitiveness and accuracy, are being directed night after night—or rather would be so, but for the proverbial uncertainty of our English climate—towards the splendid gem of the south-eastern heaven. Such is the natural result of that unexampled diffusion of a taste for astronomy, and of the equally unprecedented multiplication of telescopes of considerable pretensions, which is characteristic of the present time, and which is so gratifying to the lovers of physical science.

This state of things, however, it need not be said, does not date from the present apparition of the planet. It is merely a renewal of the attention which was directed to the same object during the preceding winter, and it is the character of the results obtained during that former period, which has led to the ensuing remarks.

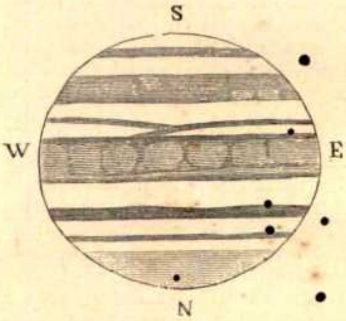
It may be reasonably thought that a comparison of such drawings as are generally attainable, professing to represent the planet during its late appearance, is not very satisfactory; and that greater agreement might fairly have been expected. Every observer must be presumed to have done his best: yet deviations exist of no inconsiderable magnitude. There must be some reason for these discrepancies; and it will be worth our while to inquire into their nature, while, at the same time, we would carefully avoid anything like an implied depreciation of any individual result.

In the first place, as regards colour, there is little difficulty in seeing that a considerable margin must be left for uncertainty. We meet with unpleasant statements from time to time as to the prevalence of colour-blindness, or at least what might be called colour-perversion, or colour-partiality, in some modification or other, to a greater extent than might have been supposed. And independently of this, there may be some dissimilarity of tint in images formed respectively by the achromatic and the reflector, though perhaps less than some persons might expect; since the blue rays which usually go astray in the former instrument are also absorbed by the silver film of the latter, so that there will be a prevailing complementary orange, however feeble, in the light that forms the image in either case. There will, however, be this advantage—and a considerable one—on the side of the reflector, that the blue light, in place of forming an unpleasant fringe, entirely disappears, leaving a delightfully colourless field in the use of the higher powers. Refractors may also differ somewhat in their mode or degree of correction, and consequently in the uncompensated tint; and in either construction eye-pieces may be chargeable with deficient achromaticity. So that on the whole we can scarcely be surprised at some differences in the results as far as colour is concerned.

But there would seem to be other and more influential reasons for the want of correspondence, as affecting form and position. One may be that all observers do not *see* equally well: not referring by this expression to clearness of eye, though in this there may be many degrees, but to the habit of attention. Where one man, for example, merely notes concerning three or four objects that they are not all of the same size; another will almost instinctively estimate their proportionate difference in magnitude. One observer again may be impressed with the proportions of the objects, but may miss those of their mutual distances: nor, again, are the estimates of different observers equally entitled to confidence. There are eyes, too, very good ones for other purposes, which are less successful than would have been expected in the discrimination of minute planetary markings, a fact not easy of explanation.

But besides this source of discrepancy, another exists in the fact that all observers do not draw equally well; or rather it may be feared that but few draw well at all. It is much to be regretted that a certain amount of artistic skill is not considered absolutely necessary in a liberal education; the advantage and pleasure derivable from it in after life are so obvious that it may well be questioned whether some of the time that is spent in mastering classical and mathematical niceties of an extremely unserviceable and unpractical nature, might not be better expended in the acquirement of the most useful art of design. It ought to be remembered, also, that not only a general facility in observation and delineation is requisite, but that something depends upon that special training which results from familiarity with the individual object. Even a careful observer, whose attention had been chiefly turned to objects of another kind, might not recognise as much of planetary markings at first, as after he had studied and learned their character; and on the other hand, a competent artist might produce inaccurate work during his early acquaintance with the telescope, simply from the unfamiliar aspect of what he has to represent, as compared with anything which he has been accustomed to delineate.

When, therefore, all these sources of discrepancy are taken into account, the differences in instruments, in eyes, in hands, and in experience, it is no longer matter of surprise that the results already obtained with regard to this planet are less satisfactory in points of mutual agreement than would have been desirable. It may be hoped, and to a certain degree expected, that we shall do better in future. Inexperience is a fault that will disappear of itself; and it would be well if the unpractised observer would be content to expend a little time and trouble in making tentative drawings before he considers them worthy of taking rank as representations of the planet. Imperfect attempts are, nevertheless, often deserving of preservation, as recording, as far as they go, something that may not be seen again, and thus acquiring a relative value. The remarks which follow may be thought to require some apology, especially after what has been said as to some of the causes of discrepancy; but it is hoped that their purport will not be misapprehended. It is well known that success in observation is much more readily obtained by those who have some previous idea as to what they may fairly expect to see; and on this ground the ensuing suggestions may be permitted, as the result of considerable familiarity with the object. They claim no higher character than suggestions—open as freely to contradiction as confirmation; and their object will be accomplished if they are found to give some aid to the unpractised amateur; the skilled observer will forgive them if he finds them needless, or correct them where they may be in error. They are deduced from the observations of forty-three nights, from 15th October, 1869, to 4th April, 1870, in the use of a With silvered glass speculum of 9⅓ inches



This sketch is copied from a drawing bearing the date of 1870, December 14d 11h 10m. It may serve as an illustration of some of these remarks, though it makes but little pretension to accuracy.

aperture and power most frequently of 212.* For the sake of convenience we may call the dark streaks *belts* and the luminous ones *zones*; and we may divide the disc into three portions—the equatorial, the northern, and the southern region.

The equatorial region may be described as a coloured girdle surrounding the planet, and consisting of two belts with a lighter space included between them. The relative proportion of these belts—the edges of the girdle—seems not invariable, and should be noted. Especial attention should be paid also to the tint of this region, as a diminution of intensity is suspected. The centre of the girdle is one of the most interesting portions of the disc, containing a number of dark markings projecting into it from the S. (upper as inverted) border, but not reaching the opposite N. edge, and taking the form of the piers of a bridge, or, when more fully developed or perfectly seen, of festoons, in which lighter yellow elliptical areas are included. For atmospheric phenomena, as these appear to be, they maintain a singular degree of uniformity and persistency, although their dimensions seem to vary, on different sides of the globe. An examination of earlier drawings, some of them in private hands, which I have been permitted to inspect, shows that, although at present conspicuous chiefly in the equatorial region, they are by no means restricted to that portion of the planet's surface. And here it may be noted that for the due comprehension of these details, especial attention should be paid to the laws of perspective. The *foreshortening*, as it is technically termed, of the regions approximating to the planet's limb, has a great influence on the apparent form of every marking lying in a meridional direction, while the ordinary belts are entirely unaffected by it. In all probability our familiarity from early youth with the common maps of the globe in two hemispheres may tend to preoccupy us with erroneous impressions in this respect, the exterior portions of those hemispheres being projected on an entirely artificial system, which exhibits them as little foreshortened as may be, and consequently extremely unlike their natural appearance on the globe. For any one unacquainted with perspective, it would be worth while to compare the edges of such a map with the corresponding portions of a terrestrial globe, as regarded from one point of view; or a globe being surrounded with a strip of paper bearing a number of equidistant markings, it will be instructive to notice the wide change in their aspect as the globe is made to rotate on its axis. To this foreshortening, it may be noted, rather than to atmospheric obscuration, as has often been alleged, we may ascribe the invisibility of many features of the planet in the neighbourhood of the limb; the existence of an external hazy envelope may be probable, but the fading of the belts at their extremities, which has often been referred to in this light, cannot be considered conclusive; for it is remarkable that as telescopes have been improved, this alleged fading has been less spoken of. It is not at all shown in the finest drawing hitherto published (that by De La Rue), in the use of an instrument of the most critical defining power; and with my nine-inch speculum it is so doubtful under favourable circumstances that I should not have independently noted it. Attention might well be given to this point, as if a difference should be found with the same eye and telescope in different seasons, it would plainly suggest atmospheric variation.

The northern region commences with a very luminous zone, to my eye the brightest and whitest part of the disc, and usually free from interruptions. Next to this comes a belt of remarkable intensity and permanency, from the edges of which dark but short markings have been, though rarely, seen to project. Its colour formed an uniform contrast with the equatorial belts during the last season; but I have thought this less striking recently. This great belt is commonly divided on the north side by a narrow light zone from a small thin companion of uncertain aspect, and during the present season not invariably parallel to its overpowering neighbour; sometimes, it is a mere edging to a streaky cap reaching to the pole, at others, a light stripe intervenes.

Passing now to the other side of the central girdle, we find next to its south border a region of some breadth, less luminous and much less tranquil than the corresponding north zone. It is interrupted by a belt of variable position and aspect, sometimes very feeble and narrow, at others a rival or more to the neighbouring edge of the central girdle. Its origin lies so close to this latter belt that it almost seems to branch out from it, and last season it followed an independent, and, in part, slightly spiral track round the greater part of the globe before its disappearance; at present I have not been able to trace its whole course, but I see that some remarkable changes have taken place. In this zone lay last winter the very curious elliptical marking described by Gledhill and Mayer, but not, I believe, by others. I never saw it, though it must have been repeatedly before my eyes—a valuable caution to myself and other students—for I had supposed myself too familiar with every portion of the disc to have overlooked so singular a feature. It is possible that my failure may have arisen from my having omitted to stop off the extreme margin of my speculum, which lies quite open in its cell without any rim. I had been repeatedly requested to do so by its accomplished maker, but from want of leisure and some difficulty in the arrangement had neglected it till near the close of those observations, when the evident improvement of definition made me regret that I had recourse to the expedient so late. This precaution, which is requisite with all open specula, is in the ordinary mode of mounting rendered needless by enclosure in a cell.

South of this changeable region lies a grey belt of uncertain form and extent, sometimes divided in two, at others partially interrupted by white spots. Between this and the south pole another belt is occasionally seen, and I have thought the colouring of this and the corresponding north area, not identical. The extreme foreshortening of these regions, and the nearly vertical position of the planet's axis, must ever prevent us from obtaining much knowledge of a wide tract surrounding either pole. The north pole is at present the one more exposed, not only to our view but to solar influences; and it may be worthy of notice that there is some existing dissimilarity in the aspect of the two hemispheres. An axial inclination of little more than 3° would indeed cause but little variety of season on the earth; but with the very dissimilar constitution and condition of Jupiter we know not how much may be effected by its influence.

These suggestions need not be carried into further detail. We shall however note, for the benefit of those who are little accustomed to the object, that the optical changes produced by rotation alone are such that we must be on our guard against being misled by their extent and rapidity.

T. W. Webb

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* The results have already been given in detail in the *Popular Science Review*, July 1870.

NOTES

We have received the first Report of the Royal Commission on Scientific Instruction and the Advancement of Science just as we were going to press. We therefore let the Report, which we print elsewhere, speak for itself. We observe that it is signed by all the Commissioners, and we trust that it is the earnest of other, equally if not more important, ones to follow.

We are informed that the future of the Kew Observatory is now being considered by the Kew Committee. We would ask whether the occasion is not a fitting one to bring before the Royal Commission on the Advancement of Science, which is now sitting, the disgraceful state of this country in the matter of such institutions as Kew. All true and disinterested lovers of science would certainly second anything the Kew Committee might urge as to the importance not only of the retention of the Observatory in its present functions, but also of a considerable expansion of them. We fearlessly express the opinion, that in England the attempt to carry on researches "of long breath" by private effort has signally failed, and the sooner this is acknowledged by scientific men the better. Our remark does not apply only to such researches even, for on all sides, in all branches of knowledge, we are conspicuous from our prominent position in the rear. It is not right that in England we should simply teach what Germans and others have discovered.

WE greatly regret to have to record the death, at Vienna, of Prof. Haidinger, the eminent mineralogist. Next week we hope to give a sketch of his life.

WE have already announced the proposed formation of a Society of Biblical Archæology. The Society has now been organised, and the following papers will shortly be read, or are in progress:—On the relations of the Assyrian and Scriptural Chronology; by Sir Henry Rawlinson, K.C.B., &c. On an Eclipse mentioned on the Assyrian Tablets, hitherto unidentified; by Mr. H. Fox Talbot. On the Medical Science of the Ancient Egyptians; by Dr. S. Birch. On the Invasion of Egypt by adjacent Nations in the time of Menephthah I.; by Dr. S. Birch. On the Ancient Connections of Egypt and Assyria; by Mr. G. Smith. On Ancient Chaldean Inscriptions and their History; by Mr. G. Smith. On the Caverns under Jerusalem; by Mr. W. Simpson. On the Flora of Palestine; by Mr. B. T. Lowne. On the Sarcophagus of Oimenephthah I. (Sethos I.); by Mr. Joseph Bonomi. We give in another column a report of the inaugural meeting held on the 21st inst.

WE learn from our Paris correspondent that the leaders of the insurrection have suspended the publication of the *Comptes Rendus*, and have dismissed M. de Parville, the scientific editor of the *Journal Officiel*. Public libraries and scientific institutions are mostly closed, but the museums and galleries have not yet been pillaged. It will be seen, however, from our report in another column that the *Académie des Sciences* continues its sittings with uninterrupted regularity.

WE have received a parcel of numbers of the *Révue des Cours Scientifiques*, the publication of which was also maintained during the Siege of Paris, though not with the same regularity as the *Moniteur Scientifique*. The numbers to hand contain many interesting articles on the supply of food, hygiene, surgical science, and other subjects incident to the state of siege.

A SCIENTIFIC institution has, we learn from *Harper's Weekly*, lately been organised in the city of Washington under the name of the Washington Philosophical Society, the object of which is to furnish to the many persons interested in science, and resident in Washington, the means of convenient communication with each other, in order to exchange discoveries and observations in different branches of learning. Many organisations with the same end in view have been started at various times in Washington, but have not been very successful in accomplishing their purpose, partly on account of the ambition of the founders in endeavouring to give to them too much of a national basis, and partly because the time was not entirely ripe for such an enterprise. It is believed that the prospects of the success of this new society are very good, as Washington possesses, at the present time, a larger number of specialists in the different departments of science than is to be found in any other city in the United States. This is easily accounted for when we bear in mind that the Coast Survey Office, the Patent Office, the Agricultural Department, the United States Medical Department, the Smithsonian Institution, the Library of Congress, the National Observatory, the Scientific Bureaus of the War, Navy, Treasury, and Interior Departments, and other public bodies, all established at Washington, necessarily attract around them men of eminence in the various branches involved in the proper prosecution of their duties. Professor Henry was elected president of the new society, and the usual officers were also chosen.

THE library of the Smithsonian Institution at Washington now numbers about 70,000 volumes, including pamphlets. In this collection are comprised complete series of the Transactions of many of the older societies of England and France; and many works which it would be almost impossible to obtain except by the system of exchange which the Society has organised with the scientific publications of societies and individuals throughout the civilised world.

THE rapid decrease of food fishes on certain parts of the sea-coast and in the lakes of the United States has for a long time been a subject of much solicitude to thoughtful persons: and various causes have been suggested for this state of affairs, and remedies proposed for its correction. Laws have been passed by most of the New England States, and by the British provinces, regulating and protecting the fish and fisheries in the inland waters; but as the jurisdiction of the States does not extend over the high seas, no special effort has been made on their part to protect the marine species by legal enactments. In view of the difficulty referred to, a bill was introduced at the last session of Congress, providing for the appointment of a Commissioner of Fish and Fisheries on the part of the United States, to make inquiries as to the alleged facts, and to report upon the same to Congress, together with any suggestions for legislative action in the premises, and the President has just appointed Professor Baird, of the Smithsonian Institution, as the Commissioner in question, with instructions to enter immediately upon the discharge of his duties.

WE regret to learn from a letter from Cairo that Dr. Gedge, the medical man and naturalist of the Viceroy's expedition under Sir S. Baker, fell sick some considerable distance above Khartoum. He was sent back, but did not get farther than Khartoum, where he died of acute mania. Mr. Wood, Sir S. Baker's secretary, was also, on account of sickness, obliged to return, and is now under medical care in Cairo. The last news from Sir S. Baker is to the effect that he is still endeavouring to make a passage through a part of the river that is filled up with dead trees and mud for two or three miles. This is situated about 6° N. lat.

EXAMINATIONS in connection with the Exeter Science Classes will take place at the Albert Memorial Museum on the following dates:—Inorganic Chemistry, May 4th; Animal Physiology, 5th; Machine Construction and Drawing, also Building Construction, 6th; Mathematics, Stage 1, 2, 3, May 8th; Physical Geography, 9th; Acoustics, Light, and Heat, 10th; Navigation and Geology, 11th; Magnetism and Electricity, 12th; Theoretical Mechanics, 16th; Applied Mechanics, 20th; Vegetable Physiology, 18th; Systematic Botany, 12th; Mathematics, Stage 4, 5, May 20th.

AT the recent annual meeting of the American Geographical and Statistical Society, Prof. D. C. Gilman, of Yale College, devoted his annual address to a sketch of the last ten years of geographical work in America. He referred especially to the publication by the Maine Historical Society of an elaborate treatise by Dr. J. G. Kohl on the early voyages to America; the topographical work of the United States' Coast Survey; the new geological maps of New Jersey, by Prof. Cook; the work of Humphreys and Abbot on the Mississippi river; the geological surveys of Ohio, Indiana, and Nebraska, by Profs. Haydn and Newberry; the topographical survey by the North-west Boundary Commission, from the Rocky Mountains to the Pacific Ocean; and the explorations of Colorado with its wonderful canons.

A PROSPECTUS is being sent out of a new work on Fungology:—Mycological Illustrations, being Figures and Descriptions of new and rare Hymenomycetous Fungi; edited by Mr. W. W. Saunders, assisted in the text by Mr. A. W. Bennett, and in the drawings by Mr. W. G. Smith. The first number will be published by Mr. Van Voorst on May 1st.

THE *Meteorological Magazine* for February contains a paper on certain Variations of Temperature during the Solar Eclipse of December 22, 1870, by Mr. Townsend M. Hall, the conclusions of which are as follows:—"The total amount of depression at the time of the greatest obscuration was 21°·75, and I would submit that these figures represent more exactly the influence of an eclipse, than if the observations had been taken at any other period of the year. During the summer months both the atmosphere and the earth are so charged with heat, that a partial darkening over of the sun for so short a time loses, to a certain degree, its effect—the diminution of warmth being partially neutralised, before it can reach the earth, by reason of its passage through the intervening atmosphere. It is to be hoped that similar observations will have been made at some point along the line of total obscuration, whilst, on a future occasion, it will remain an interesting question to be determined by meteorologists, how far the thermal depression varies with the season of the year and the climate of the locality."

WE have received a paper on a Wind-direction Rain-Gauge, by Mr. J. R. Napier, F.R.S. The principle of this gauge consists essentially in supporting a vessel, like the first receiver of an ordinary gauge, on a pivot, so that it may be turned with the least wind, and having a spout attached to it, leading the rain into vessels in fixed directions surrounding the receiver, so that if it rains, for example, when the wind is between N.N.W. and N.N.E., the north vessel receives it, or when between N.N.E. and E.N.E., the north-east vessel receives it, &c.; for there are eight vessels which show the amount of rain and direction of the wind at the time.

THE Geologists' Association has paid visits during the past month to the British Museum and the Museum of Practical Geology; and have arranged for excursions on April 10 to Cambridge to visit the Woodwardian Museum and the exposures of cretaceous strata in the neighbourhood of Barnwell, under the guidance of Mr. Wiltshire and Mr. Bonney; to the Zoological and Hunterian collections of the Royal College of Surgeons April 18; and on April 29 to Belvedere and Crayford, led by Prof. Morris, the chief object of interest being the Mammalian remains of the newer Pliocene deposits.

OF late years many discoveries have been made in regard to the habits and characteristics of the aborigines inhabiting the coasts of North America prior to the time of Columbus by careful examination of the artificial heaps of refuse shells, bones, &c., accumulated in the vicinity of their villages. The published researches of Professor Wyman and others have proved full of interest; and as the subject continues to excite the attention of American archæologists, we doubt not that much now hidden will yet be brought to light. As these deposits are usually on or very near the sea, they are much exposed to the wearing of the waves; indeed, their discovery is usually due to exposure of a section by this influence. For this reason it is of importance that the examinations in question should be prosecuted before the heaps have entirely disappeared, as a large proportion will probably not outlive the next half century. We learn from *Harper's New Monthly* that a careful search on the shores of Kent and Northumberland counties, on the eastern coast of New Brunswick, has shown that, in consequence of the wearing away of the soft sandstone shale of the coast for many rods, all traces of the shell deposits, believed to have once existed in abundance, have now entirely vanished.

ACCORDING to a recent report by Dr. Stimpson upon the Crustaceans dredged in the Gulf Stream by Count Pourtales, of the Coast Survey, eighty-one species, of forty-seven genera, were obtained, of which fifty-two of the species and nineteen of the genera are to be considered as new. Only a small proportion of the species were from great depths, fifteen alone being recorded as coming from below 100 fathoms. The greatest depth at which any of the species were found was 150 fathoms, these belonging to the family of the *Portunidæ*. The portion of Dr. Stimpson's report on the brachyurous crabs of this collection has just been published in the Bulletin of the Museum of Comparative Zoology at Cambridge, already so well known for the merit of its zoological memoirs, and the remainder will follow at no distant interval.

DR. KESSLER claims to have discovered lately in Cassel the oldest herbarium known, some of the plants having been prepared in 1556. It contains 614 plants, properly fastened down and labelled, and was formed by Caspar Katzenberger.

A PLAN has been introduced at the Society of Arts, which has found favour so far, for instituting a kind of British Association for Indian topics under the name of the Oriental Congress of Great Britain and Ireland. It will embrace geology, natural history, biology, and other branches of science, and their economical applications. The proposal to hold the first meeting at Manchester met with the approval of the representatives of the Cotton Supply Association.

THE Colaba Observatory at Bombay recorded in three nights only in January 2·28 inches of rain. This is said to be unprecedented there, and generally throughout the Presidency there has been a fall of rain threatening the cotton crops.

UNDER the title of "Comision de la Flora Forestal Espanola," a useful account is published of the trees most suitable for forest cultivation in Spain, with remarks on the importance of keeping up the forests to maintain the equilibrium of the climate.

AT a time when the small-pox is so prevalent, and vaccination has become, so to speak, fashionable, it is surprising that we do not hear something of the many reputed remedies of foreign countries, either as a cure for this disease or as an eradicator of its effects. The *Sarracenia purpurea* is well known for its supposed efficacy, and it was even introduced into this country some few years back. But a plant not so well known in Europe is the *Melia Azadir-achta* L. of India, the leaves of which are used by the natives to cover the bodies of patients recovering from small-pox, as they are supposed to prevent the mark becoming permanent. Dr. Wright says of this tree that "the leaves beaten into a pulp, and externally applied, act like a charm in removing the most intractable form of Psora and other pustular eruptions."

IN a recent article, *Harper's Weekly* referred to the fact that torpedoes were being used for killing fish for manure on the coast of Florida. This business is being carried on about six miles below New Smyrna, at Mosquito Lagoon; and the method adopted is said to consist in exploding the torpedoes in the water, under the schools, as they pass by. In addition to the many that are killed outright and float on the surface, large numbers are wounded, and go off elsewhere to die without being caught. This practice, we are assured, has already resulted in a very marked diminution of the schools of fish in that vicinity, and has been greatly resented by the people of the State, who are endeavouring to drive the operator from its waters.

An earthquake at Arequipa in Peru, on the 21st December, was strong, and is said to have lasted fifty or sixty seconds. It was attended with much subterranean noise.

An earthquake shock was felt at Bombay and as far north as Baroda on the night of January 31. It was distinctly noticed over a large area, but did little or no damage.

Recent telegraphic advices from Havana announce that Captain Selfridge, who is engaged in prosecuting the Darien ship-canal exploration, thinks he has discovered a practicable oute over a line where the elevation of the divide is not more than three hundred feet above the level of the sea. The despatch is dated Paya, on the Tuira River, a stream which empties into the Gulf of San Miguel, on the Pacific.

The well-known Pinang or Betel nuts, the seeds of *Areca catechu*, a handsome palm cultivated in all the warmer parts of Asia, and used by the natives to chew with lime for the purpose of producing a gentle kind of intoxication, form an important article in the interior trade of the Malayan Archipelago, being exported from Sumatra to other islands in large quantities. The exports from Padang alone in one year amounted to 5,057 piculs.

From the twenty-sixth Report of the Proceedings of the Calcutta School-Book Society, we learn that during the two years 1868-1869, more than 179,300 rupees was spent in purchases of books for distribution among the natives, the object of the society being "to supply and distribute, at the lowest possible price, a healthy household literature in the vernacular tongues," in which it is assisted by a small monthly grant from Government. The titles of some of the works circulated sound to us peculiar, as "A Treatise on Spiritualism and its Manifestations," "A Drama against Upstarts," "The Grief of Females on the Departure of their Husbands by Rail on Monday," &c.; while the modicum of science given is very small; and, judging from the titles, we should suppose that the instruction in natural and physical science, thought good enough for the natives of Hindostan, is about on a par with that which prevailed in this country in the time of Oliver Goldsmith.

The Berwickshire Naturalists' Field Club, the oldest society of the kind in Britain, has issued its "Proceedings" for 1870. Besides one or two archæological papers, and an address from the President, the Rev. G. S. Thomson, we have the following contributions to Natural History and Ethnology, chiefly connected with these branches as represented in the district:—"The History of the Wolf in Scotland," "Turnip Insects during 1870," "Botanical Notices," and "Contributions to the Entomology of the Cheviots," chiefly in coleoptera; all these from Mr. James Hardy. The Secretary, Mr. George Tate, contributes a carefully worked-out paper on "The Stature, Bulk, and Colour of the Eyes and Hair of Native Northumbrians;" and Mr. Ralph Carr treats of "The Northumbrians between Tyne and Tweed." There are several other short papers of local interest, and a statement of the rainfall concludes the number.

In a discussion at the Indian Conferences at the Society of Arts, as to a proposition of Col. Wragge to employ Neilghery peat on railways, it was mentioned that the two largest peat bogs near Ootacamund have been swept away in rains. This was alleged to be through cutting them at the wrong end.

We learn from Nicaragua that the river and port of San Juan del Norte are shoaling in many places. Where there was deep water in the latter ten years ago, there is now a bank above the surface.

ON THE CONNECTION BETWEEN TERRESTRIAL TEMPERATURE AND SUN-SPOT PHENOMENA

Mr. Stone, the newly-appointed Astronomer Royal at the Cape of Good Hope, has recently communicated to the Royal Society an important paper and curve, in which the thermometric observations taken there since 1841 are discussed. This curve he has compared with another constructed on Wolf's observations of sun-spots, and with the following result, which we give in his own words:—

"The agreement between the curves appears to me so close that I cannot but believe that the same cause which leads to an excess of mean annual temperature leads equally to a dissipation of solar spots. There is on the whole a curious appearance of logging of the inverse curve of solar spots over that of temperature. At the maximum about 1856, this, however, does not appear to be the case; but when the uncertainties of the data, both of the solar spots near the minimum, and of the mean temperature also, are taken into account, such discrepancies might perhaps fairly be expected, even if there be a physical connection between the two phenomena as results of some common cause. If there be a sensible inequality in the mean temperature with a period of about ten years, then the mean temperature resulting from the observations in the temporary observatory, which were made near a maximum, will be too high. The corresponding ordinates, therefore, will be depressed too much relatively to those corresponding to observations made in the other two observatories. In the curve 2 I have imperfectly corrected the mean of the results for the temporary observatory on the supposition of such an inequality existing. The only result of such a correction is to modify the curve at the points of junction of the observations made in different positions. The general form is unaltered. It should be mentioned that the point about which the curves appear to differ most is near or at the change of exposure from the original observatory to the temporary shed about 1852.

"I may mention that I had not the slightest expectation, on first laying down the curves, of any sensible agreement resulting, but that I now consider the agreement too close to be a matter of chance. I should, however, rather lean to the opinion that the connection between the variation of mean temperature and the appearance of solar spots is indirect rather than direct, that each results from some general change of solar energy. . . . The problems of meteorology appear to be presented here in a simpler form than in England, and probably systematic photographic self-registering observations extended over a few years might lead to important results."

EXPERIMENTS ON CERTAIN VIBRATORY PHENOMENA

The apparatus made use of consists simply of a cardboard disc furnished with radial slits, and which can be rotated with any desired velocity. To examine a coal-gas flame singing in a glass tube, the disc is placed in front of the flame, and the eye placed where the slits pass in a vertical position. When the dish rotates with such a velocity that the interval between two slits passing the eye is just equal to the period of a complete vibration of the flame, the flame appears to be motionless; but if the velocity of the disc be slightly reduced, the flame is seen slowly to go through its changes of form, appearing to consist of a series of puffs, resembling those from the funnel of a luggage locomotive. When the interval between the passing of the slits is equal to, or is one-half, one-third, &c., of the period of vibration of the flame, a singular appearance of a phantom disc is seen, having as many as twice or three times the number of slits really in the disc; this phantom wheel appears motionless if the periods exactly coincide, but if they do not, it slowly revolves in one direction or the other. It is obvious that this affords an easy method of counting the vibrations of the flame. With a sixteen inch tube I thus found the number of complete vibrations per second to be about 453.

When the disc is rotated in front of a vertical vibrating wire, the eye being placed where the slits pass in a horizontal position, the interval of the slits passing being equal to a complete vibration of the wire, the wire appears thrown into undulations and motionless. If the periods do not exactly coincide, the undulations travel up or down the wire. If the velocity of the disc be doubled, or trebled, the apparent number of wires is increased in like proportion; and if it be regarded by the two eyes placed where the slits do not pass in a horizontal position, they assume the form of spirals, which appear to revolve around each other in an extremely beautiful and illusive manner. In the above cases I have supposed the wire to be twanged in the centre, in which case the undulations are beautifully symmetrical curves, and represent a pure note. If, however, the wire be twanged near to one end, the change in the *quality* of the note is manifest, the irregularity of the curves showing the presence of minor undulations superimposed upon the primary one. The best wire for this purpose is a fine spiral one, as it gives vibrations of great amplitude, and of long continuance. A vibrating steel rod also appears thrown into the same undulations.

CHARLES J. WATSON

THE ACTION OF FLUORSPAR ON DIFFERENT QUALITIES OF CAST IRON

IN my articles contributed to NATURE (No. 57, p. 94, and No. 64, p. 233), I have given descriptions of my process of applying fluorspar combined with oxides, and fluorspar combined with oxides containing titanium, to ordinary cast iron.

It is reported that attempts have been made to apply fluorspar alone to ordinary cast iron by eminent chemists in the laboratory, but the results have been of a negative character. These reports have been corroborated by my own experiments. I have discovered,* however, that although fluorspar has no effect when used alone in treating ordinary cast iron, it will act energetically upon cast iron containing titanium. The titaniferous cast iron was made at Glassdale Furnaces, near Whitby, by melting Cleveland white pig iron in a cupola in admixture with Norwegian titanic iron ore, and blast furnace cinder as a flux. This metal was treated here in the laboratory by being melted upon powdered fluorspar. The resulting metal was found to be wrought iron.

These results may be obtained in any suitable vessel, furnace, apparatus, or process; the only conditions necessary to be observed are, that the metal be maintained in the fluid state, and the fluorspar placed so as to act upon the metal from the under side upwards, or placed in admixture with it, and that when apparatus is used having silicious linings, the silicious linings be protected with sheet or cast iron placed upon the silicious lining before the fluorspar and iron to be treated are charged into the vessels. No labour is necessary, except that of "balling" the metal, and removing it from the apparatus.

The results given by this experiment go to prove that by this process superior qualities of wrought iron, which will be purer than the highest standard brands of wrought iron, may be produced from the English Cleveland pig iron, which contains, according to the best metallurgical authorities, from 1·25 to 1·38 per cent. of phosphorus.

One ton of Cleveland pig iron was melted in a cupola with 7 cwts. of Norwegian titanic iron ore, containing, by analysis, about 40 per cent. of titanic acid. The resulting metal was titaniferous cast iron, analysing:—

Titanium	1·2551
Silicon	1·8139
Phosphorus	0·4604
Sulphur	0·3620
Carbon	1·2982

Hence it appears that the iron, by being re-melted with titaniferous iron ore, took up 1·25 per cent. of titanium, and lost 0·90 per cent. of phosphorus, and 1·75 per cent. of carbon. It is obvious that the metal in this condition is not available for any purpose without subsequent treatment, as it contains about as great an amount of impurities as it did before treatment.

The advantages gained by re-melting the pig iron with titaniferous iron ore are, a reduction of the amount of phosphorus and carbon, and the alloying of the metal with titanium, which facilitates the removal of the impurities in the subsequent treatment.

The above described titaniferous cast iron was remelted upon fluorspar, and about 30 minutes after the iron melted, or in about an hour after they were both charged, the iron was found to be malleable iron; the button analysing as follows:—

Titanium	0·0215
Silicon	None
Phosphorus	0·1399
Sulphur	0·0620
Carbon	Traces

When worked on a larger scale, so as to produce blooms that can be worked into merchantable shapes, the finished results will show less phosphorus and sulphur than the above analysis, as it is well known to metallurgists, by the experiments of Messrs. Calvert and Johnson, published in full in Kerl's "Metallurgy," vol. ii., "Copper and Iron," 1869, that 0·022 per cent. of phosphorus, and 0·040 per cent. of sulphur, are removed in working blooms into finished iron.

It will be seen that the action of the fluorspar removed 4·9662 per cent. of the impurities contained; and that the resulting metal contains less impurities in amount than the highest standard qualities of wrought iron. The explanation of these effects I leave to chemical investigators, without hazarding an opinion which might be erroneous, and therefore disadvantageous to me.

JAMES HENDERSON

MR. WALLACE'S ANNIVERSARY ADDRESS*

A CONSIDERABLE portion of this Address is devoted to a discussion of the facts of distribution of beetles, as presented by Mr. Wollaston in his great work, the "Insecta Maderensia," with special reference to the views advocated by Mr. Andrew Murray, in his paper on the Geographical Distribution of Beetles. After touching on the various methods by which insects are known to be distributed, and mentioning several of the instances in which they have been captured some hundreds of miles from land, it is concluded that, in opposition to the view held by Mr. Murray, there is no reason to believe that the Atlantic islands owe their Coleoptera to a former land connection with the continent, more especially as there is such strong evidence against that view in the total absence of mammals and reptiles. Mr. Wallace then applies Mr. Wollaston's facts to a detailed test of these views; and, as this portion of the paper is of general interest to naturalists, we give it at length:—

The most novel and striking facts brought out by Mr. Wollaston's researches in Madeira are, as is well known (1) the affinity with the Mediterranean fauna; (2) the total absence of certain large divisions of Coleoptera abundant in that fauna; (3) the number of new and peculiar species and of new and anomalous genera; and (4) the unexampled preponderance of apterous species. Now accepting, as Mr. Murray does, the theory of slow change of forms by natural causes, we may take the first and third of these facts as proving that the origin of the Madeiran fauna is of very ancient date. Let us see, therefore, how the second and the fourth set of facts bear upon the mode of its origin, whether by a land-connection with Europe or by transmission across the sea. It will be convenient to take first the facts presented by the apterous or winged condition of the species.

This striking peculiarity consists, either in species being apterous in Madeira which are winged elsewhere, or in genera which are usually winged consisting of only apterous species in Madeira, or lastly, in the presence of endemic apterous genera, some of which have winged allies, while others belong to groups which are wholly apterous. Such phenomena undoubtedly show that there is something in Madeira which tends to abort wings; and Mr. Wollaston was himself the first to suggest that it was connected with exposure to a stormy atmosphere. His further observation, that many of the winged species had wings more developed than usual, enabled Mr. Darwin to hit upon that beautiful explanation of the facts which commends itself to all who believe in the theory of Natural Selection, while Mr. Wollaston himself admits it as fully accounting, teleologically, for the phenomena. That explanation briefly is, that the act of flying exposes insects to be blown out to sea and destroyed; those which flew least therefore lived longest, and by this process the race became apterous. With species to whom flight was a necessity, on the other hand, the strongest-winged lived longest, and thus their wings became more and more developed in each succeeding generation.

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* Patent No. 318, Feb. 3, 1870

* An Address read at the Anniversary Meeting of the Entomological Society of London, on the 23rd of January, 1871, by Alfred R. Wallace, F.Z.S., F.R.G.S., President.

Now this view of the case enables us at once to explain some of the most striking gaps in the Madeiran coleopterous fauna. The Cicindelidæ, for instance, are entirely absent; and almost all the European species are winged insects of somewhat feeble flight, yet to whom flight is necessary. We can readily understand that such insects would be easily exterminated if they arrived singly or in small numbers; though it is not so easy to understand why, in a forest-clad island, some of the sylvan species should not have found a home had the land ever been connected with a continent where they abound. Their total absence is, therefore, decidedly unfavourable to the theory of a land-connection with Europe. To the Melolonthidæ and Cetoniidæ, as well as the Eumolpidæ and Galerucidæ, which are all wanting, the same argument will apply; and also to the Elateridæ and Buprestidæ, which are represented each by one minute species. But if Madeira is the remains of a continent once continuous with the south of Europe and deriving its fauna from such continuity, how are we to explain the absence of extensive genera very abundant in South Europe, and, from their being apterous, specially adapted to the peculiarities of Madeira? Such are *Carabus*, *Lampyris*, *Pimelia*, *Akis*, and many others. But these facts are all consistent with the theory of introduction across the sea. Apterous groups, however abundant on the continent, should, as a rule, be absent; and I find that almost all the European apterous genera are wanting, and among the few exceptions there are some whose presence is easily explained and really prove the rule. We must remember, however, that the apterous condition, except in those cases where it is characteristic of an extensive group, is one of little stability or importance. There are species which are sometimes apterous and sometimes winged, and we may therefore be sure, that if any advantage was to be derived by either condition over the other, Natural Selection would very rapidly render it constant by the repeated survival of the favoured individuals. This is illustrated by the fact that we have winged and apterous species in the same genus, as well as winged and apterous genera in the same family. The coleopterous order being essentially winged, and the vast majority of its members being capable of flight, it is a presumption, if not almost a certainty, that all apterous varieties, species, or groups, have been derived from winged ancestors—comparatively recently in the case of the former, and at a more remote epoch as the character becomes more constant and attached to groups of higher classificational value.

Taking these principles as our guide, let us examine more closely the facts presented by the Madeiran Coleoptera, and their bearing on the rival theories as to their mode of introduction.

There are a large number of European beetles belonging to the very varied genera and families which are apterous, and a large proportion of these inhabit the south of Europe and North Africa. Now, on the theory of land connection, there should be no marked absence of these groups; on the contrary, apterous forms being especially adapted to Madeira, we should expect them to predominate. But, on the opposing theory of transmission across the sea, we should expect them to be wholly absent, or, if there are any exceptions, we should expect to be able to detect some special circumstances which might favour their transmission. A careful examination of Lacordaire's "Genera," and of some works on European Coleoptera, has furnished me with the following list of genera which are wholly apterous, and which abound in South Europe and North Africa.

Carabus possesses about eighty species in these regions; but is wholly absent from Madeira.

Thorictus has ten South European species, and one representative in Madeira, which is an ants'-nest species.

Rhizotrogus (Melolonthidæ), twenty-seven species in Sicily and Algeria, the very country to which the Madeiran fauna is traced, yet it is wholly absent.

Lampyris, *Drilus*, and *Troglops* (Malacoderms), of which the females are apterous, possess twenty-seven South European and North African species; none in Madeira.

Otiorhynchus, *Brachycerus*, and twenty other genera of Curculionidæ, comprising more than 300 South European and North African species, are absent from Madeira, with two exceptions. One is the *Trachyphlœus scaber*, a widely-spread European insect often found in ants' nests; and this, with the case of the *Thorictus*, renders it probable that ants'-nest species have some unusual means of distribution, which are by no means difficult to conceive. The other exception is that of the genus *Acalles*, which has a number of Madeiran species, all peculiar, and is very abundant in all the Atlantic islands. Now we have first to remark that *Acalles* is an isolated form, but is allied to *Cryptorhynchus*, which is often amply winged; so that we may easily suppose that its introduction to Madeira took place before it became completely apterous in Europe. In the second place we have the fact, that many of the species are confined to peculiar herbaceous and shrubby plants, in the stems of which they undergo their transformations, and which habit would afford facilities for their occasional transmission in the egg or pupa state across a considerable width of ocean, while a fragment of dry stem containing egg or larva might possibly be carried some hundred miles or more by a hurricane. Such suppositions would not be admissible to account for numerous cases of transmission, but, as will be seen, this is almost the only example of a genus of large-sized apterous European beetles occurring in Madeira.

Pimelia, *Tentyria*, *Blaps*, and eighteen other genera of Heteromera, comprising about 550 species of South Europe and North Africa, are totally absent from Madeira, with the following interesting exceptions:—two common species of *Blaps*, which are admitted to have been introduced by human agency, and three species of *Meloe*, two of which are European and one peculiar. The means by which the apterous, sluggish, and bulky Meloes were introduced is sufficiently clear, when we remember that the minute active larvæ attach themselves to bee-insects of exceedingly powerful flight, and more likely than perhaps any other to pass safely across 300 miles of ocean. That the solitary exception to the absence of wholly apterous genera of European Heteromera from Madeira should be the genus *Meloe*, is, therefore, one of those critical facts which almost demonstrate that it is not to land-continuity with the continent that the island owes its insect fauna.

Timarcha.—This, the only important apterous genus of Chrysomelidæ, is especially abundant in Spain and Algeria, and possesses forty-four South European and North African species; yet it is unknown in Madeira.

The occurrence of two isolated European species of characteristic Atlantic apterous genera—Tarphius and Hegeter—may seem to favour the opposite theory. The *Tarphius gibbulus* occurs in Sicily, and is the only European species of the genus, of which about forty inhabit the Atlantic islands. It is most nearly allied to the smallest of the Madeiran species, *T. Lowei*, which is abundant among lichen on weather-beathen rocks, and even ascends in the forest regions to the highest branches of the trees. These habits, with its minute size, are all in favour of this species, or some ancestral allied form, having been carried across by the winds or waves, thus transferring to Europe one of the peculiar types elaborated in the Atlantic isles. The *Hegister tristis* is an analagous case, this species of an otherwise exclusively Atlantic genus having occured on the opposite coast of Africa. These instances will furnish a reply to one of Mr. Murray's difficulties,—that all the migration has been in one direction, from Europe to Madeira, never from Madeira to the continent,—a difficulty, it may be remarked, which is wholly founded on an unproved and unprovable assumption; for how can it be determined that, in the case of *Acalles* for example, the genus had not been first developed in the Atlantic islands and then transferred to Europe, instead of the reverse? It is always assumed to have been the other way, but I am not aware that any proof can be obtained that it was so, and it is inadmissible to take this unproved assumption, and base an argument upon it as if it were an established fact.

We will next consider the facts presented by the distribution of those species of Coleoptera which range from Madeira to Europe or to any of the other Atlantic islands. If their distribution has been effected by land-continuity, we should expect that the proportion of winged and apterous species that extend their range beyond the island, should not be very strikingly different from the proportion that is found on the island. We do not find, for example, that the proportion of the wingless *Carabi* that have reached our own country from the continent by former land connection, is very different from that of the winged *Cicindelæ*.

Now, leaving out altogether those species which have certainly been introduced by man, and grouping the remainder for convenience in six divisions, we find that the Madeiran Coleoptera, which are not peculiar to it, may be classed as follows :—

31 species of Carabidæ, of which 26 are winged, 5 apterous. The whole fauna, however, presents the very different proportion of 38 winged, 43 apterous.

93 species of the families from the Hydradephaga to the Tomicidæ inclusive, of which 90 are winged, 3 apterous. Total fauna ; 220 winged, 27 apterous.

28 species of Curculionidæ, of which 26 are winged, 2 apterous. Total fauna ; 35 winged, 74 apterous.

15 species of Longicornia and Phytophaga, of which 15 are winged, none apterous. Total fauna : 48 winged, 1 apterous.

20 species of Heteromera, of which 16 are winged, 4 apterous. Total fauna : 28 winged, 27 apterous.

76 species of Staphylinidæ, of which all are winged, none apterous. Total fauna : 109 winged, 6 apterous.

The totals are, for the wide-ranging species, 249 winged, 14 apterous = 263 ; for the whole fauna, 478 winged, 178 apterous = 656.

It thus appears that, in every case, an immensely smaller proportion of apterous than of winged species are widely distributed. If we take the totals, we find that while about two-fifths of the whole number of species range to other countries, only about one-thirteenth of the apterous species do the same, although among the strictly endemic species there are 160 apterous to only 110 winged! We can hardly impute such a constant and overwhelming preponderance to the fact that apterous insects have less facilities for extending their range, when we know that nearly every apterous genus possesses species of almost universal European distribution. I may here recall the fact, that of the above-mentioned fourteen apterous species which range out of Madeira, two are Meloes and two ants'-nest beetles, whose presence we have already sufficiently accounted for. It may no doubt be said that much of the difference here shown is due to the fact that the peculiar Madeiran species have had time to become apterous, while the species common to other countries have not yet had time to lose their wings ; but this argument, although a valid explanation of some portion of the facts, if we admit that many of the latter have been *recently introduced by natural causes*, cannot be used by those who maintain a former land-connection as the sole origin of the fauna ; for on that theory all the species now inhabiting the island (and not introduced by man) must date back to the same remote period, and have had equal time in which to be modified.

Let us now consider what are the special relations of the apterous Madeiran species as throwing light upon their possible or probable mode of introduction.

We have three species which Mr. Wollaston himself states to be usually winged elsewhere, but which are apterous in Madeira. These are *Metabletus obscuroguttatus*, *Calathus fuscus*, and *Bradycellus fulvus*. I am inclined to believe that there are a few others which will come under this category, but it is very difficult to get information as to the winged or apterous character of particular species. These insects, however, have evidently become apterous since their introduction into Madeira. We have therefore no difficulty in accounting for their introduction, and, as no other change in their external characters has been effected, we may suppose it to have been comparatively recent.

Next we have those genera which, though apterous in Madeira, are wholly or partially winged elsewhere. These comprise a large number of species, and are twenty-two in number, as follows :— Carabidæ : *Cymindis*, *Dromius*, *Metabletus*, *Scarites*, *Apotomus*, *Loricera*, *Leistus*, *Calathus*, *Olisthopus*, *Argutor*, *Cratognathus*, *Bradycellus*, *Trechus*. Philhydrida : *Hydrobius*. Byrrhidæ : *Syncalypta*. Curculionidæ : *Phlœophagus*, *Tychius*, *Smicronyx*. Heteromera : *Phaleria*, *Helops*. Staphylinidæ : *Homalota* (1 sp.), *Othius*. Here we are carried back to a remoter epoch for the introduction of the winged ancestors of the Madeiran species, since not only have the wings become aborted, but the insects themselves have become modified into distinct and often very well marked species.

The next category consists of apterous genera which are peculiar to Madeira and the other Atlantic islands, but which are allied to winged groups, as follows :—

Elliptosoma.—Closely allied to *Loricera*, winged.

Eurygnathus.—An abnormal form of Licinides, most of which are winged.

Zargus.—An abnormal form of Chlæniides, winged.

Thalassophilus.—Allied to *Trechus*, winged.

Tarphius.—Belonging to the Colydiidæ, most of which, Mr. Pascoe informs me, have wings.

Coptostethus.—Allied to *Cryptohypnus*, winged.

Caulophilus.—Allied to *Phlœophagus*, winged.

Lipommata, *Mesoxenus*, *Caulotrupis*.—Anomalous genera of Cossonides, which are often winged.

Acalles, *Torneuma*.—Aberrant genera of Cryptorhynchides, most of which are winged.

Echinosoma.—Doubtful affinities.

Atlantis, *Cyphoscelis*, *Laparocerus* (Laparocerides).—A very isolated group.

Anemophilus, *Scoliocerus*.—Allied to Trachyphlœides, some of which are winged.

Lichenophagus.—Allied to *Cænoposis* and *Omias*, some of which are winged.

Xenorchestes.—Allied to *Choragus*, winged.

Ellipsodes —Closely allied to *Crypticus*, some of which are winged.

Hadrus.—Belongs to an apterous group of Opatrides, many of which are winged.

Macrostethus.—Belongs to Cœlometopides, all of which are apterous, but comes next to the *Tenebrionides vrais*, of Lacordaire, which are mostly winged.

Xenomma.—Belongs to the Aleocharides, which are winged.

Mecognathus.—Allied to *Sunius*, winged.

Metopsia.—Allied to *Phlœobium*, winged.

Here we have indications of an introduction of forms at a still more remote epoch. In many cases the modifications of structure have been so great as to produce distinct generic forms, while these remain still allied to winged European genera. In other cases, however, the modifications are still greater, and the affinities are with groups which in Europe are wholly apterous. Such cases as *Hadrus* and *Macrostethus*, which belong to small groups of wholly apterous genera, are difficulties on the theory of transmission over the sea. But two considerations render this difficulty less real than apparent. They all carry us back to a very remote epoch ; and, knowing what we do of the instability of the apterous condition, we may fairly conclude that the groups in question were, at that time, in a partially winged state. At or near this same remote epoch, the Madeiran group, as indicated by the submarine bank now connecting the several islands, probably formed one more extensive island, and the distance of ocean to be traversed would then have been considerably less than it is now.

If the various groups of facts which I have here set forth, respecting the distribution of apterous and winged species and genera, are fairly considered as a whole, I think they will be seen to be quite inconsistent with the theory of that distribution having been effected by a former land connection with Europe ; and, considering that we are necessarily ignorant of many of the ways by which organisms are transmitted across ocean barriers, such transmission seems to be indicated in the case of the Madeiran Coleoptera, not by means of drift wood and ocean currents, which Mr. Murray thinks must be the most efficient means of transport, but by some mode in which their wings are called into play, which can only be by a passage through the air when assisted by gales and hurricanes.

There is one other group of islands which seems well adapted to offer a crucial test of the correctness of the theory of land-connection. The Azores are more than twice as far from Europe as the Madeiras, and, what is of still more importance, they are cut off from it as well as from the Madeiras by a broad belt of ocean of the enormous depth of nearly 15,000 feet. We may feel pretty confident, therefore, that if both groups have once been united to the continent, the separation of the Azores is by far the more ancient event ; and any theory which requires the Azores to be the most recently separated must be strongly supported by independent evidence to render such an improbable supposition acceptable. If the Azores date the origin of their insect population from a remote epoch when they were connected with

Europe, we should expect to find that almost all the species have since become modified, and that these islands would offer us a larger proportion of highly specialised and ultra-indigenous forms than Madeira itself. The exact contrary, however, is the fact, for out of more than 200 species only about sixteen are peculiar.

Taking the geodephagous group, the species of which, both Mr. Murray and Mr. Wollaston believe, are least liable to be introduced by man, we find that two only are peculiar, while sixteen are European. The Rhynchophora only equal the Geodephaga in number of species, and seven of these are peculiar. Leaving out a large number of species which have, there is little doubt, been introduced through human agency, there remain more than 100 species identical with those of Europe and the Atlantic islands, while only fourteen are peculiar. These facts imply that the insects, as a whole, have been brought to the islands through natural causes, and that the process is probably still going on. On looking to Physical Maps for information, however, a difficulty appears; for the ocean currents, as well as the prevalent regular winds, are all from the westward, while only four of the beetles are American, and these being all wood-borers, have no doubt been brought by the Gulf Stream where they have not been introduced by man. Fortunately, however, we have a means of getting over this difficulty; for our member, Mr. F. Du Cane Godman, who has given us the most recent and accurate information on the natural history of these islands, informs us (in his paper on the birds of the Azores in the "Ibis" for 1866) that the stormy atmosphere, to which we have seen that Madeira owes so many of its peculiarities, is still more marked a feature of the Azores, where violent storms from all points of the compass are frequent, and annually bring to their shores numbers of European birds. As a natural result of this constant influx, the birds of the islands are, all but two, of European species; and, what is very important, they decrease in numbers from the eastern to the western islands of the group. This is just what we should expect if they are stragglers from the eastern continent; but if they are the descendants of those which inhabited the country before its dismemberment, there would be no meaning in such a diminution. Now we can hardly doubt that these same storms also bring Coleoptera and other insects to the Azores, though it may be more rarely and in smaller numbers than in the case of the birds; and the large proportion of European species will then be very intelligible. The same explanation is suggested by the proportions of the most important groups, for while (after deducting all those species believed to have been introduced by man) the Geodephaga and Brachelytra are by far the most numerous, the Rhynchophora and the Heteromera are exceedingly few, a distribution which corresponds with their respective powers of flight. It is also a very important fact that only four non-introduced species can be traced to an American origin, while more than a hundred are European; since it shows of how little importance are ocean currents as a means of conveying insects over a wide extent of sea; whereas the great mass of the non-introduced species have evidently passed through the air, aided by their powers of flight, for a distance of about a thousand miles from Europe. The Azorean Elateridæ form a curious feature of its fauna, considering that the whole family is almost absent from Madeira and the Canaries. Of the six species two are European (one specially Portuguese), so that they may have been introduced with living plants. Two are common South American species, probably introduced in the floating timber, though they may also have come with living plants, which are often brought from Bahia. Two species, however, are peculiar, and one is closely allied to a Brazilian species, so that it must have been introduced by natural agencies before the settlement of the island; the other is of a genus confined to Madagascar.

Now it is a suggestive fact that the Mozambique current, bending round the Cape of Good Hope to the Equator, is one of the sources of the Gulf Stream; so that it is not impossible that a tree, carried down by a flooded river on the west coast of Madagascar, might ultimately reach the Azores. That it should convey living larvæ or pupæ of Elaters may also not be impossible; and if such a log reached the Azores but once in ten thousand years, and but one log in a thousand should convey living Elaters, we should still, if the calculations of geologists have any approximate value whatever, be far within the epoch of existing genera, and even of most existing species. A relation so isolated and extraordinary as that between a single insect of the Azores and those of Madagascar, may well be due to a concurrence of events as rare and improbable as this seems to be.

The Azores, and in a less degree the Madeiras, appear to me to teach us this important lesson in the laws of distribution of birds and insects—that it has been determined neither by the direction of ocean currents nor by that of the most prevalent winds, but almost wholly by such more exceptional causes as storms and hurricanes, which still continue to bring immigrants from the nearest lands.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, March 21.—Mr. R. Hudson, F.R.S., in the chair. The Secretary read a report on the additions to the Society's Menagerie during the month of February, 1871.—Mr. Sclater exhibited a skin of the Ceylonese *Prinia*, recently spoken of by Mr. W. Vincent Legge in a communication to the Society, and now forwarded by that gentleman, which appeared to be identical with *P. socialis* of continental India.—An eleventh letter was read from Mr. W. H. Hudson, on the ornithology of Buenos Ayres.—Dr. Hamilton communicated an extract from a letter received from China, relating to the reproduction of a Chinese Deer, *Hydropotes inermis*.—Mr. Sclater read a paper on the Birds of Santa Lucia, West Indies, containing an account of a collection recently made in that island by the Rev. Mr. Semper, and forwarded to Mr. Sclater by Mr. G. W. des Vœux. Amongst these specimens were two examples of an *Icterus*, believed to be undescribed and proposed to be called *I. laudabilis*.—Dr. R. O. Cunningham read a paper on some points in the anatomy of the "Steamer Duck," *Micropterus cinereus*, based upon specimens of this bird obtained by him during his recent voyage as Naturalist to the Survey of the Straits of Magellan. A communication was read from Mr. R. Swinhoe, containing a revised catalogue of the Birds of China and its islands. To this were added descriptions of new species, together with references to former notes and occasional remarks.

Chemical Society, March 16.—Prof. Williamson, F.R.S., president, in the chair. Mr. C. H. Piesse was elected a fellow. Mr. C. H. Gill read a note "On the examination of Glucose containing Sugars." It is known that coloured sugar solutions are decolourised and clarified by the addition of basic lead acetate before they are submitted to optical examination. Mr. Gill now found that the power of invert sugar to rotate a ray of polarised light is greatly altered by the presence of that reagent. The alteration takes place only on the levulose in the liquid, the dextrose suffers no change of optical properties. This alteration is not permanent—on removing the lead or acidifying the liquid the original rotatory power is restored. Mr. Gill employs these latter reactions in order to obtain correct numbers with the saccharometer. He uses a strong solution of sulphuric dioxide, which removes the lead, and at the same time bleaches the liquid, but is incapable of inverting cane sugar in the cold even in twenty-four hours. The presence of the lead salt in sugar solutions is also disadvantageous when the glucose has to be estimated by Fehling's copper solution, as it partly becomes reduced, and thus necessitates the use of a greater volume of the saccharine solution; the removal of the lead does away with this source of error.—Mr. D. Howard made some remarks on the boiling point of a mixture of amylic alcohol and water.—Mr. Perkin stated that he had succeeded in obtaining bromacetic acid by gradual addition of bromine to heated acetic anhydride, boiling for some time, mixing with water and subsequent distillation.—Mr. Warrington spoke briefly of an easy and sufficiently correct determination of ammonic sulphocyanide in commercial sulphate of ammonia.

Entomological Society, March 20.—Mr. A. R. Wallace, president, in the chair. Prof. P. M. Duncan, F.R.S., and Mr. E. S. Charlton were elected members. The Rev. L. Jenyns, in a letter to Mr. Dunning, made some remarks on the statement of Mr. Bond, at the last November meeting, respecting the swarming of *Chlorops lineata* in the Provost's Lodge at King's College, Cambridge. Mr. Jenyns had a similar swarm in 1831, and occurring probably in the same room.—Mr. Verral exhibited a fly, *Pipiza noctiluca*, from Perthshire, to the head of which a substance was adhering, probably the pollen-mass of an orchid.—Mr. Muller exhibited a gall, in shape like a grain of wheat, on the leaves of a *Carex*, sent to him by Lord Walsingham from Thetford.—Mr. C. O. Waterhouse read a description of *Apterocyclus honolulensis*, a new genus and species of Lucanidæ from the Sandwich Islands.—Mr. Wollaston communicated a paper on additions to the Atlantic *Coleoptera*, in which he recorded the results of recent observations, with corrections on synonymy, &c. He adhered to his original opinion that the Atlantic archipelagos were the remnants of a former continent, with a possible connection with south-western Europe, and, in this respect, he differed from the views of Mr. Wallace that the islands had received their insect fauna by means of atmospheric agencies, thus agreeing more with the theory enunciated by Mr. Andrew Murray in his recent essay on the geographical distribution of *Coleoptera*. A discussion ensued in which the President, Mr. Bates, and Mr. Murray took part; the two first-named gentlemen considering that the remarkable absence in the islands of reptiles and mammals, was an insuperable objection to the idea of a former Atlantic continent; Mr. Murray, on the contrary, contended that the great homogeneity exhibited in the fauna and flora could only be explained by supposing the islands to have once been connected by a land passage.

Biblical Archæological Society.—The inaugural meeting, which was largely and influentially attended, was held on Tuesday, March 21, at 8.30 P.M. Dr. Birch of the British Museum took the chair, supported by Prof. Donaldson, Captain Wilson, and Messrs. Bonomi, Boyle, Drach, &c. After the usual preliminary business had been transacted the chairman delivered an opening address, enumerating the circumstances which had led to the formation of the Society, and stating the various advantages offered to the scientific world by its institution. A concise summary was then given of the results of past and pending archæological investigations in Assyria, Egypt, Palestine, and Western Asia; these results it was now proposed to extend and systematise by the labours of the Society of Biblical Archæology, while the council hoped eventually to be able to undertake excavations of their own among the still unopened tumuli of Mesopotamia. Prof. Donaldson moved that the inaugural address be printed, and made some excellent remarks upon the necessity of exercising great tact in further investigations, as well as great promptitude in securing such valuable antiquities as were likely to be destroyed by superstitious barbarians.—Mr. Boyle, of Lincoln's Inn, seconded the resolution.—Captain Wilson, in proposing a vote of thanks to the chairman, expressed his pleasure in belonging to two societies, whose labours, though in different spheres, would mutually and materially assist each other.—Mr. S. M. Drach moved that the name of the indefatigable secretary, Mr. W. R. Cooper, should be added in the vote of thanks, seconded by the Rev. T. Gorman, and carried unanimously.—Mr. Bonomi, Rev. A. Mozley. and other gentlemen also addressed the meeting.—A list of papers to be read at future meetings, contributed by Sir Henry Rawlinson, Mr. H. Fox Talbot, Dr. Birch, and Messrs. G. Smith, Bonomi, and Lowne, was then announced, and the society adjourned to Tuesday, 4th April, 1871.

CAMBRIDGE

Philosophical Society, March 13.—"On the Attraction of an infinitely thin shell bounded by two similar and similarly situated concentric ellipsoids on an external point," by Prof. Adams, F.R.S.—"On the theory of the forms of floating leaves in certain plants," by Mr. Hiern, St. John's College. This problem was treated mathematically, the leaf being considered as at its edge a flexible body, acted upon by an outward radial force, that of growth, and an external pressure due to the velocity of the stream. The curves which would be assumed under various conditions were calculated, and shown to agree closely with the forms which are to be found in certain aquatic plants.—"On the effect of exhaustion and inflation of the tympanum in deadening sounds, and on the test of loudness," by Mr. Moon, Queen's College.

MANCHESTER

Literary and Philosophical Society, March 7.—Mr. E. W. Binney, F.R.S., president, in the chair—"The Action of Sulphurous Acid on Phosphates," by Dr. Wilhelm Gerland, Macclesfield.—"Further observations on the Strength of Garden Nails," by Dr. J. P. Joule, F.R.S. The author thought it desirable to ascertain how far hardness had to do with the strength and elasticity of these small specimens of cast iron. For this purpose he plunged some of them at a heat near the melting point into water, then selecting those which had been hardened sufficiently to resist the action of the file. Others he cooled slowly from a bright red heat. The experiments were conducted in the manner described in the last number of the Proceedings:—

	No of Experiment.	Length of Nail between Supports.	Breadth of Nail at Fracture.	Depth of Nail at Fracture.	Deflection.	Breaking Weight. lbs.
Hard Nails.	1	1·0	0·11	0·122	·0067	129
	2	1·04	0·12	0·12	·0037	84
	3	1·0	0·12	0·122	·0028	81
	4	1·02	0·143	0·102	·0077	129
	5	1·1	0·138	0·13	·0071	203
	Average	1·032	0·1262	0·1192	·0056	125·2
Soft Nails.	6	1·0	0·112	0·117	·0088	141
	7	1·05	0·139	0·114	·0087	150
	8	1·02	0·130	0·138	·0051	176
	9	1·04	0·117	0·090	·0101	101
	10	1·04	0·121	0·108	·0073	113
	Average	1·03	0·1238	0·1134	·008	136·2

Reducing to a length of 3ft. and 1in. square section, and making a deduction of one-eighth from the deflections, on account of the taper of the nails, the above results, along with those in the last number of Proceedings, become

	Breaking Weight.	Deflection.
Nails in original state . .	2673	·922
Hardened ditto . .	2002	·677
Softened ditto . .	2448	·924

—Dr. Joule exhibited three photographs of the sun taken on the 1st December, 1858. The images, ·43in. diameter, were produced by the achromatic object-glass of a telescope with half-inch stop. Exposure, by means of an apparatus completely detached from the camera, during a small fraction of a second. He had been induced to examine them after seeing the beautiful photograph of the late eclipse by Mr. Brothers. On examining the three images a nebulosity is observed, very similar to that in Mr. Brothers' photograph. In all three, taken at an interval between each of about a minute and a half, the nebulous appearance appears situated on three quarters of the limb, the remainder being quite free. There are also indications of a radial structure, so that he thinks it highly probable that the representations are actually those of the corona. Since communicating the above, he has carefully examined the two other photographs of the sun which he possesses, and which were taken early in the month of November 1858. These, one of which must have been exposed at about two hours twenty minutes after the other, present nothing remarkable to the naked eye; but when viewed through a glass of moderate power, a thin crescent-shaped envelope is observed on each, with this remarkable circumstance, viz., that in the two it appears on opposite limbs, suggesting the idea of a semi-revolution in the above interval of time at a velocity not much less than that due to Kepler's law of planetary motion. In one of the photographs there is, under the crescent and apparently on the rim of the sun itself, a narrow band in breadth about $\frac{1}{100}$ of the diameter of the disc, and of at least double the intensity of the sun. This may probably be referred to the actinic action of the chromosphere and the red flames.—"On Anthraflavic Acid, a Yellow Colouring Matter accompanying Artificial Alizarine," by Edward Schunck, Ph.D., F.R.S.

DUBLIN

Royal Dublin Society, March 20.—Professor R. Ball in the chair. Prof. Traquair read a paper on the restoration of the tail in *Protopterus annectens*. Two specimens of moderate size were exhibited and described, in both of which the caudal extremity of the body had been evidently truncated by violence, and a restorative process had taken place. In the first specimen the reproduced portion measured half-an-inch in length, and contained a prolongation of the notochord of the lateral muscle, and of the spinal cord, but neither vertebral arches, spines, nor fin rays. In the second the reproductive process had gone on to a much greater extent, the new portion being two inches long, and contained besides a notochordal axis, a reproduction of all the other essential parts of the normal tail of Protopterus. The neural and hæmal arches, spines, and fin supports were, however, entirely cartilaginous and rather irregular in their disposition. They were not traceable beyond a distance of one-and-a-half inches from the origin of the reproduced part, while the notochord extended to the very tip.—Dr. Moore read a paper on a Fungoid Disease which attacks and destroys plants belonging to the Pandaneæ; he also exhibited a specimen of *Silenipedium caudatum*, which had flowered in the Glasnevin Gardens for the first time in Ireland.—Professor Dyer read a paper on the Germination of Seeds While it was true that in most Dicotyledons the root end of the embryo is developed into a tap-root, and that in most Monocotyledons the radicle was not developed, but the roots were pushed out through the base of the cotyledon; there could be no doubt that there were exceptions to this rule, which were both sufficiently numerous and important to be borne in mind, and which it was a great mistake not to find noticed in our Manuals of Botany. Many examples were given, such as palms with their exorrhizal roots, and Tropæolum with its endorrhizal roots. Prof. Dyer, in the course of his interesting paper, alluded to and illustrated Prof. Dickson's news on the embryo of Zostera.—Mr. W. F. Kirby read some notes on three species of trap-door spiders whose nests are in the Museum of the Society.—The Royal Dublin Society ordered a letter to be written to the Director of the Jardin des Plantes, Paris, informing him that Dr. Moore, Director of the Botanical Gardens, Glasnevin, had received instructions from the Council to assist as far as he possibly can in supplying the losses incurred by the recent bombardment.—A letter has been received by the Society from Prof. Milne-Edwards, stating that Prof. Decaisne will send over lists of their desiderata, and that in a few days the venerable Director of the establishment, M. Chevreul, will forward to the Society a letter of thanks for the warm sympathy and extreme kindness shown to the Jardin des Plantes by the vote of the Society.

EDINBURGH

Botanical Society, January 12.—Alexander Buchan, M.A., President, in the chair. "Note on the Practical Application of Meteorology to the Improvement of Climate." By Alexander Buchan, M.A. "Notes on the Structure and Measurements of Cells in Hepaticæ." By James Williamson Edmond, M.B. "Notes on the Distribution of Algæ." By George Dickie, M.D., Professor of Botany, Aberdeen. "On the Flora of the South of France." By Mr. James F. Robinson. "Memoranda on Fir Cones in the Museum at the Royal Botanic Garden." By Alexander Dickson, M.D., Professor of Botany, Glasgow.

PARIS

Academy of Sciences, March 20.—M. Faye, president, in the chair. Baron Thénard, who had been arrested by the Prussians as a hostage, was present at the sitting. After the reading of the *procès verbal*, he made a speech, in which he thanked the Academy for the protest entered by it against his arrest by Prussians when Paris was actually besieged by Prussian guns.—M. Chevreuil delivered a very long and very able speech on the changes of different kinds which silk undergoes from chemical agents when passing through different colouring processes.—M. Mathieu, the oldest member of the Institute, presented the *Annuaire des Bureaux des Longitudes*, which should have appeared on the 1st January.—M. Delaunay said he had been to inspect the Villejuif pyramid, which was erected on one of the extremities of the basis measured by Picard two centuries ago. The pyramid was not destroyed, owing to the precautions taken for its protection. The meteor of the 17th March was seen from several stations, viz., at Paris by M. Prevot, surgeon to the marines, at $10^h\ 58^m$, the luminous track remained visible for more than a hour; M. Samberg, Professor of Physics, at Rochelle, gives for the time $10^h\ 30^m$. The track was visible for an hour; no explosion took place. The colour was green, and the duration of the apparition was twenty seconds. Many sparks were noticed.—A discussion took place between MM. Delaunay, Becquerel, and Saint-Claire Deville on the temperature of the past winter. It was proved that the temperature is always less severe at the Jardin des Plantes than at the Astronomical Observatory, and that at the Astronomical Observatory it is less severe than at Montsouris. It results from this discussion that it is useless to attempt to find the absolute temperature of any region independently of local circumstances, and it is only by the comparison of many different observations that the facts relating to temperature can be ascertained.—M. Quatrefages presented a description of some helminthoid worms which locate themselves in the throats of snakes. These helminthoids cause such an irritation that the throat is closed, and the animal perishes by suffocation. In the secret committee, which was opened at five o'clock, a sharp discussion was raised between members with respect to M. Deville's proposition.

BOOKS RECEIVED

ENGLISH.—Essays on Darwinism: T. R. Stebbing (Longmans)—A Manual of Structural Botany: M. C. Cooke (Hardwicke)—Aunt Rachel's Letters about Air and Water (Longmans)—Dynamics of Nerve and Muscle: Dr. Radcliffe (Macmillan)—Cassell's Natural History, new ed part I. (Cassell)

FOREIGN—(Through Williams and Norgate)—Handbuch der Chemie: D. K Kraut—Vorlesungen uber nautische Astronomie 1^{er} Band, 1^{te} Abthielung: Dr. J D. C. Weyer—Handbuch der Anatomie des Menschen; J. Henle—Atti della r università di Genova, vol. I. Epilogio della Briologia Italiana: G. de Notaris.—Die Elemente der Krystallographie: J. M. Matzdorff—Ueber Entwickelung und Bau des Gehorlabyrinthe: Dr. A Boettcher—Archiv für Anthropologie, 4^{er} Band, 1870.

DIARY

THURSDAY, MARCH 30

ROYAL SOCIETY, at 8 30—Experiments in Pangenesis, by breeding from rabbits, of a pure variety, into whose circulation blood, taken from other varieties, had previously been largely transfused: F. Galton, F R S—Contributions to the History of Orcin No 1. Nitro-substitution Compounds of the Orcins: Dr. Stenhouse, F R.S
SOCIETY OF ANTIQUARIES, at 8 30—Ballot for the Election of Fellows
CHEMICAL SOCIETY, at 8.—Anniversary Meeting.
ROYAL INSTITUTION, at 3—Davy's Discoveries: Dr. Odling
LONDON INSTITUTION, 7 30—On Economic Botany: Prof. Bentley.

FRIDAY, MARCH 31.

ROYAL INSTITUTION, at 9—Solar Myths: Prof. Max Müller.

SATURDAY, APRIL 1

ROYAL INSTITUTION, at 3—Spirit of the Age: Mr. O'Neil.
ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.

MONDAY, APRIL 3.

ANTHROPOLOGICAL INSTITUTE, at 8—Report on Settle Cave Explorations: W. Boyd Dawkins, F.R S—On the Anatomical Writings of Prof. Calori: D Barnard Davis, F.R S.—Builders of Megalithic Monuments in Britain: A. L. Lewis
ENTOMOLOGICAL SOCIETY, at 7.
LONDON INSTITUTION, at 4—On Astronomy: R. A. Proctor.
ROYAL INSTITUTION, at 2.—General Monthly Meeting.

TUESDAY, APRIL 4.

ZOOLOGICAL SOCIETY, at 9—On some new and little-known species of Madrepores, or Stony Corals, in the British Museum collection: W. Saville Kent, F.Z S.—Notes on some Indian Siluroid Fishes: Francis Day.

WEDNESDAY, APRIL 5.

GEOLOGICAL SOCIETY, at 8—On a new Chimæroid Fish from the Lias of Lyme Regis Sir P. de Malpas Grey Egerton Bart, M P, F.R S—On the Tertiary Volcanic Rocks of the British Islands: Archibald Geikie, F R S—On the formation of "Cirques," and their bearing upon theories attributing the excavation of Alpine Valleys mainly to the action of Glaciers: Rev T G Bonney, F.G S.
ROYAL MICROSCOPICAL SOCIETY, at 8—On the Mode of working out the Morphology of the Skull: W K Parker, F.R S.—On Linear Projection considered in its application to the delineation of objects under Microscopic Observation: Charles Cubitt, C.E.

THURSDAY, APRIL 6.

ROYAL SOCIETY, at 8 30
SOCIETY OF ANTIQUARIES, at 8 30
LINNEAN SOCIETY, at 8.—On the stigmas of *Proteaceæ* G Bentham, Pres. L. S.—On the generic nomenclature of *Lepidoptera*: G. R Crotch
CHEMICAL SOCIETY, at 8.—On Burnt Iron and Burnt Steel: W. Mattieu Williams—On the formation of Sulpho Acids: Henry E Armstrong.
ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

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THURSDAY, APRIL 6, 1871

THE UTILISATION OF NATURAL HISTORY MUSEUMS FOR SCIENTIFIC INSTRUCTION IN GERMANY

IN the following notes we propose to consider the Natural History Museums in Germany, and to see to what extent and in what manner, indirect or direct, they are utilised for the scientific instruction or education of the people. These institutions are very numerous, there being one in nearly every larger German town, and even two or three in places like Vienna and Hamburg. Some comprise collections of zoological, anatomical, palæontological, botanical, and mineralogical objects, while others are limited to one or more of these branches; but whatever their contents may be, we do not recollect visiting one of these Museums in which the objects were left unnamed or unarranged. The majority are State establishments, under the direction of a single head, who is responsible to the Minister of Public Instruction. If the establishment is very extensive, the collections of the various branches are placed under more than one director, the administration and responsibilities being divided. Their development is in some measure dependent on the direct assistance of the Government, but still more on the energy and capability of the director, inasmuch as, of two Museums originally supported by the same grant of money, one has remained stationary for years, whilst the director of the other, making the best use of his independent position, has known how to raise the value of his Museum as a purely scientific or instructive institution, thus establishing claims for additional assistance, which could not be neglected by the Government.

We may divide the German Museums into—(1) Those founded with the intention of exhibiting objects of Natural History to the general public; and (2) those established for educational purposes.

There are not many of the former class. To it belong the Museums of the formerly independent "Reichsstädte," Hamburg, Bremen, and Frankfort-on-the-Main, one of the Vienna Museums, and the collections in Stuttgart and Darmstadt. There are others, like that in Mayence, but they have more the character of well-arranged local country museums. Although originally founded for the purpose of exhibiting curiosities, they soon took another position by receiving objects in which the general public takes a very limited interest (as, for instance, botanical, geological, or mineralogical specimens), and by systematically collecting materials for the purposes of purely scientific research. In several instances the scientific results were sufficiently important and extensive to form not only a nucleus but the sole subject-matter of distinct periodical works, such as the "Annalen des Wiener Museums," the "Museum Senckenbergianum," the "Abhandlungen des Hamburger Museums." The Frankfort Museum became the head-quarters for the Zoology of North-eastern Africa; Bremen possesses a unique collection of African birds, celebrated not only for the great number of standard specimens, but also for their beautiful state of preservation. In the Vienna Museum particular attention was paid to European fresh-water fishes; and travellers like Natterer, Russeger, Kotschy, enriched it with collections so numerous that the Austrian Naturalists have been engaged in their examination till within a very recent period. The Stuttgart and Darmstadt Museums are now celebrated for their valuable collections of South-German fossils, worked out by G. von Jäger, Kaup, and others.

In the Museums of this class great attention is paid to the local Flora and Fauna, recent and extinct. Thus the Stuttgart collection may be mentioned as a model of what a Museum ought to be; besides a most complete series of the plants and fossils, it contains a collection of the animals of Suabia in all stages of growth and development and of variation, in a perfect state of preservation, and particularly attractive from the life-like manner in which the specimens are mounted.

It was the natural consequence of the growth of a Museum, especially in the smaller of the towns mentioned, that it became a conspicuous object; that, although opened to the general public on certain hours of Sunday only, the crowds of visitors increased; that the interest in it spread into wider circles; and that, more or less in connection with it, societies were formed whose aim was the distribution of knowledge, and which were accessible to all. Although no great benefit is individually derived by the majority of the members of such societies from their rather superficial connection with Science, yet we ought to remember that even the fragments of knowledge picked up by them have the effect of expanding their ideas. A community of feeling grows up between the professed naturalist and the uninitiated, by which the former is encouraged in his further pursuits, and induced to consult in his labours and communications the requirements and understanding of the latter. Some of the German Governments, especially the Austrian, have also acknowledged the value of such societies by granting them an annual subsidy.

One out of several instances may be brought forward in evidence of the correctness of the above remarks, viz. the example of the town of Frankfort, to which many years ago a small miscellaneous collection and library was bequeathed, under the name of "Museum Senckenbergianum." By the able management and disinterestedness of Dr. Eduard Rüppell, it grew into a considerable zoological collection, of which the inhabitants were justly proud. An interest in Natural History being awakened, a body of men, chiefly wealthy merchants, formed themselves into a society, founding a Zoological Garden, and in connection with it a monthly periodical (noticed in one of our previous numbers), in which, at first, only objects of local interest were discussed, but which now has among its contributors eminent naturalists as well as amateurs, and does more for the distribution of sound natural history knowledge among the general German public than all the other scientific periodicals put together.

While thus we fully acknowledge the value of the Museums of this class as offering materials for really scientific original research, and as creating and maintaining an interest in Science among the general public, we understand that they are not utilised in a direct manner for methodical scientific instruction. Although Natural History is taught in nearly all the so-called Gymnasiums and Polytechnic Schools, the time devoted to it is limited, and those Museums are only visited at intervals by the classes under the guidance of teachers.

THE DESCENT OF MAN

The Descent of Man, and Selection in relation to Sex. By Charles Darwin, M.A., F.R.S., &c. In two volumes. Pp. 428, 475. (Murray, 1871.)

I.

IF Mr. Darwin had closed his rich series of contributions to Science by the publication of the "Origin of Species," he would have made an epoch in Natural History like that which Socrates made in philosophy, or Harvey in medicine. The theory identified with his name has stimulated ethnological and anatomical inquiries in every direction; it has been largely adopted and followed out by naturalists in this country and America, but most of all in the great work-room of modern science, whence a complete literature on "Darwinismus" has sprung up, and there disciples have appeared who stand in the same relation to their master as Muntzer and the Anabaptists did to Luther. Like most great advances in knowledge, the theory of Evolution found everything ripe for it. This is shown by the well-known fact that Mr. Wallace arrived at the same conclusion as to the origin of species while working in the Eastern Archipelago, and scarcely less so by the manner in which the theory has been worked out by men so distinguished as Mr. Herbert Spencer and Prof. Haeckel. But it was known when the "Origin of Species" was published, that instead of being the mere brilliant hypothesis of a man of genius, of which the proofs were to be furnished and the fruits gathered in by his successors, it was really only a summary of opinions based upon the most extensive and long-continued researches. Its author did not simply open a new province for future travellers to explore, he had already surveyed it himself, and the present volumes show him still at the head of his followers. They are written in a more popular style than those on "Animals and Plants under Domestication," as they deal with subjects of more general interest; but all the great qualities of industry and accuracy in research, of fertility in framing hypotheses, and of impartiality in judgment, are as apparent in this as in Mr. Darwin's previous works. To one who bears in mind the too frequent tone of the controversies these works have excited, the turgid rhetoric and ignorant presumption of those "who are not of his school —or any school," and the still more lamentable bad taste which mars the writings of Vogt and even occasionally of Haeckel, it is very admirable to see the calmness and moderation (for which philosophical would be too low an epithet) with which the author handles his subject. If prejudice can be conciliated, it will surely be by a book like this.

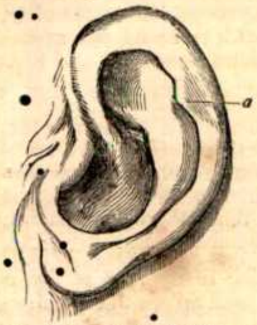
It consists of two parts. The first treats of the origin of man, his affinities to other animals, and the formation of the races (or sub-species) of the human family. Besides the obvious interest to all Mr. Darwin's readers of a discussion on the subject of their "proper knowledge," naturalists will find the detailed application of the laws of natural selection to a single common and well-known species an excellent test of their truth and illustration of their difficulties. It is in dealing with the latter, which are never extenuated or passed by, that the author introduces the subject of sexual selection. This is dealt with in the second part, which forms more than two-thirds of the work, and that not only as it affects man, but in its entire range. Reserving this division of the book for a future article, we will endeavour here to give a summary of the course of argument in the earlier portion.

The author, justly assuming that the general principles of natural selection are admitted by all who have examined the evidence on the subject, with the exception of many of "the older and honoured chiefs in natural science," proceeds at once to discuss the proofs of the origin of man considered apart from those affecting all animals in common. The first group of facts adduced to show his kinship with other forms of animal life, relate to the strict correspondence of his bodily parts with those of other mammalia. To say that these structures are the same because they have the same uses, is untrue, for many of them have no use in the sense of active function, and we constantly find the same structures in animals turned to different uses, and the same uses subserved by different structures. To say that the bodies of men and animals are alike because they are formed on the same plan, or because they are the realisation of the same idea in the Creator, is true enough, but is beside the mark; for natural science inquires how or by what steps these things have become so, not why and from what first cause. If one sees two men very much alike, one naturally supposes that they are brothers; if they are rather less so, they may be cousins; if only agreeing in general characters, we recognise them as at least belonging to the same race or nation; and so, when the facts to be accounted for are once ascertained, nothing but prejudice or repugnance to acknowledge our true relations, can explain why it was so long before naturalists admitted the hypothesis of community of origin between men and other animals. What is called the Darwinian theory accounts for the way in which diversities have arisen, and thus has converted an apparently obvious hypothesis into a well-grounded theory. But in expounding the likeness between men and animals, the author does not confine himself to anatomical structure, but shows how the same resemblance extends to the laws of disease, the distribution of parasites, and other minute particulars.

The next argument brought forward is the equally familiar one drawn from the likeness of the human embryo to that of other vertebrata. Then follows an account of the rudimentary organs in man, which in all other species are justly held among the most important indications of affinities. One such rudiment is mentioned which is, we believe, hitherto unrecorded. It is a slight projection of the rim of the helix of the auricle, which would correspond when unfolded to the point of an erect ear. (See illustration.) This occasional abnormity may, perhaps, be recognised by future anatomists as the *Angulus Woolnerii* after its first observer.

In the second chapter Mr. Darwin shows that a consideration of the mental faculties of man, including the use of language, which has been held the greatest difficulty to admitting his kinship to other animals, may rather strengthen than weaken the arguments derived from his bodily structure. Memory and curiosity, jealousy and friendship, and even the power of correct reasoning, and of communication by sounds, are shown to belong to many of the lower animals, while the faculty of reflection and self-consciousness, and "the ennobling belief in the existence of an Omnipotent God," cannot be ascribed to the lowest tribes of the human family. At the same time it is argued that the use of articulate language, the power of forming abstract ideas, and even the sense of right and wrong, may have been gradually acquired by steps which here and there it is not impossible to trace. The question of the origin of the moral sense leads to the proposition of the following theory. Some natural emotions are of great intensity but short endurance, and their force is not easily recalled by memory; others, though less powerful at certain times, exert a constant influence, or one which is only interrupted by being overpowered for a time by the former. Accordingly, during the greater part of life, and always when there is leisure for reflection, the gratification connected with the more violent passions, such as hunger, sexual desire, and revenge, appears small, whereas the social instincts of sympathy and the pleasures of benevolence exert their full power. Hence we find social virtues, as courage, fidelity, obedience, among savages and even animals, long before the "self-regarding" virtues begin to appear. This theory is analogous to that by which Mr. Bain explains the higher character of the pleasures of sight compared with those of smell; they can be more easily recalled ;and corresponds to the distinction drawn by the same writer between the acute and the more "massive" and permanent pleasures.

In the fourth chapter Mr. Darwin discusses the manner in which man was developed. It is shown that the broad facts on which the theory of Natural Selection rests apply to him. He is prolific enough to share in the struggle for existence. In him, as in all organic forms, there is a constant tendency to growth, which being checked and modified by external influences, proceeds in the direction of least resistance, and so produces the variations which are often ascribed to an assumed inherent tendency. Among the various forms produced, those will survive which are best fitted for the surrounding conditions, and they will transmit their character to their descendants, still subject to the same liability to vary. Next the author argues that the mental endowments of man, including language, his social habits, his upright position, and perfect hands, are of direct advantage to him in the struggle with other animals and with his fellows. It has always appeared that the difficult point in the development of man by Natural Selection is at the period when he was more defenceless than an anthropoid ape and less intelligent than the lowest savage; but Mr. Darwin thinks that the transition may have been safely made in some large tropical island where there was abundance of forest and of fruit. That man, once developed, can maintain himself, is obvious from his present existence. The arguments in favour of civilised man being the descendant of savages, which have been so admirably developed by Sir John Lubbock and Mr. Tylor, are of course brought forward in support of the author's view, and the important question is discussed how far we may hope for future improvement in the race by means of continued Natural Selection. Thus, while admitting that the process undergoes many checks and complications among human beings, the author does not assent to the arguments urged by Mr. Wallace that it would cease to operate as soon as the moral faculties came into play.* One human peculiarity which is apparently inexplicable by Natural Selection, the nakedness of the body and presence of a beard, is referred by Mr. Darwin to the operation of Sexual Selection. To this same agency is attributed the origin of the so-called Races of Man, which is discussed with admirable clearness and impartiality in the last chapter, and this leads to the complete exposition of the theory of Sexual Selection which occupies the second part of this work, and must be considered in a future article.



Human Ear, Modelled and Drawn by Mr. Woolner. *a*. The projecting point.

It only remains here to add a word on the account of the affinities and genealogy of man contained in the sixth chapter. As a kind of retribution for the attempt to raise Cuvier's order *Bimana* into a sub-class, not only have most naturalists now reverted to a modified definition of the *Primates* of Linnæus, but Mr. Darwin shows reasons for refusing to the genus *Homo* even the rank of a family in this order, which Prof. Huxley admits, and regards it simply as an aberrant member of the Catarrhine division of the *Simiadæ*. This conclusion, which seems to us to be a just one, will only be distasteful to those who so little appreciate the true characters of man as a spiritual being, that they could feel self-complacency in the brevet-rank of a sub-class.

Mr. Darwin mentions Africa as the possible seat of the Catarrhine progenitors of man, but shows the futility of speculations on this point, until we know more of the recent changes of the earth, the records of palæontology, and the laws affecting the rapidity of animal modifications. He does not advert to Prof. Haeckel's hypothesis of a "Lemuria" in the Indian Ocean, but agrees with him in next tracing the phylum of man to the *Prosimiæ*. These again were developed from "forms standing very low in the deciduate mammalian series" (possibly, as Prof. Huxley suggests, most nearly allied to the existing *Insectivora*), and thus, through the Marsupials and Monotremes from the Reptilian stock, and thence through the *Dipnoi* and Ganoids from the *Urtyhus* of the vertebrate series, represented by the Lancelet alone. Nor does Mr. Darwin stop here, but adds the weight of his judgment to the theory based on the observations of Kowalewsky and Kuppfer, which deduces the primeval *Vertebrata* from a form resembling a Tunicate larva. Perhaps the most brilliant of the many new suggestions in these volumes is one thrown out incidentally in a note to p. 212, and based upon this supposed relation of man to the Ascidians. Beyond the organic world Mr. Darwin does not attempt to trace the genealogy of man. Considering how essential this extension of the theory of evolution is held by men so distinguished as Haeckel, and how keenly the question of Abiogenesis has recently been discussed, the reticence shown in avoiding allusion to the subject is perhaps the most remarkable among the many remarkable characters of this great work. P. H. PYE-SMITH

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* In reviewing in these columns the contributions of the latter eminent writer, we took occasion to quote the estimate he expresses of Mr. Darwin's claims. Should anyone be disposed to overlook the original value of Mr. Wallace's work, he will be corrected by a somewhat similar passage in the present volume. See pp. 137, note, and 416.

OUR BOOK SHELF

Elementary Natural Philosophy. Being a course of nine lectures by J. Clifton Ward, F.G.S., Associate of the School of Mines. (London: Trübner and Co.)

THE attempt to crowd the Elements of Natural Philosophy into nine lectures cannot be otherwise than a failure. This is signally the case with the little book before us. We need hardly go farther than the table of contents to justify the statement. A single lecture is devoted respectively to Magnetism, Voltaic Electricity, Light, and Heat; Pneumatics and Hydrostatics together occupy one lecture, whereas to Frictional Electricity and Sound are given two lectures apiece. Nor does the author confine himself to a simple summary of the leading facts in each of these subjects, he tries to rush over all the field occupied by larger text-books. Hence, important facts are often lightly passed over and comparatively trivial matters made unduly prominent. In Voltaic Electricity, for example, two pages are occupied with a description of the effects of electro-chemical decomposition, when seen on the screen by the aid of the solar microscope. We recognise here, and indeed on every page of the book, those lecture-experiments with which Dr. Tyndall has made the students of the School of Mines so familiar. Mr. Ward has not only drawn largely upon his notes of those lectures, but he imitates Dr. Tyndall's language and style.

Notwithstanding this, we are quite sure Mr. Ward has only himself to blame for the errors which even a cursory glance has revealed to us. On p. 85 we read "Magnetism may be produced by *friction* (of *soft iron* with loadstone or other magnet) by magnetic induction and electricity." Magnetism is *not* produced by friction of soft iron. On pp. 36 and 37 Mr. Ward has fallen into a vulgar and serious error in explaining the electric wind. Speaking of the so-called electric fish, here is what he says:—"If the interior of the Leyden jar be charged positively, negative electricity will be attracted to the head of the fish, from the somewhat blunt point of which it will stream and cause a movement from the knob; while the gliding off of the repelled positive from the finer pointed tail, will counterbalance this movement, and keep the body in equilibrium." The author also speaks of a lighted candle extinguished by the *draft of electricity* streaming from a point. This, of course, is grossly incorrect; it is the movement of contiguous air particles charged similarly by contact and then repelled, that extinguishes the candle, or supports the gold leaf fish.

Though there are some good points in this little book, we regret our inability to recommend it either to schools or students. We venture to think the author betrays his want of experience in teaching science by the over-crowding of his facts; the first lecture, for instance, is accompanied by thirty-three distinct experiments. Teaching—especially science teaching—requires "precept upon precept, line upon line, here a little and there a little:" otherwise there is an almost certain danger of the learner obtaining loose and superficial knowledge, the end of which is not sound instruction, but disastrous conceit.

W. F. B.

Essays on Darwinism. By J. R. R. Stebbing. (Longmans and Co., 1871.)

MR. DARWIN, in his recent work, very truly observes that "false facts are highly injurious to the progress of science, for they often long endure; but that false theories are comparatively innocuous." Mr. Stebbing's work can then do little harm, as it supplies us with no new "facts" whatever, whether true or false. The author is an advocate who serves Mr. Darwin with more zeal than discretion, and who seems but little, if at all, able to appreciate the arguments and objections adduced on the other side. Some who are already convinced of the truth of Darwinism will read with pleasure a series of eloquent and interesting essays in its favour; but, though calculated to confirm a disciple, they are singularly ill-calculated to convert an opponent. Before Mr. Stebbing again writes upon this subject we strongly recommend him to peruse carefully Mr. Grote's "Examination of the Utilitarian Philosophy."

Das Wesen und die Ziele der Chemischen Forschung und des Chemischen Studiums. Akademische Antrittsrede gehalten von Dr. Rudolph Fittig. (Leipzig: Quandt and Händel, 1870. London: Williams and Norgate.)

So busy are the majority of German chemists in research, that it is seldom we are privileged to have their opinions on the object of the science, and the position it should occupy as a study. Dr. Fittig has availed himself of his appointment as Professor of Chemistry in the University of Tubingen to deliver an inaugural address, in which these points are discussed with great clearness and ability. Starting with the assumption that the majority of men estimate the value of a science only by its power to satisfy want and contribute to the comfort of life, Dr. Fittig goes on to claim for chemistry from this point of view the first place among the sciences. "Where," he asks, "is there another science which, in the application of its results to man, almost from his first breath to his last, is so true a companion as chemistry?" and he proceeds to show that it is useful, not so much in explaining what the nourishing constituents of food are, as in disclosing the laws of agriculture, and thus teaching us how to produce means of nourishment. Further, he points out that there is not an article of clothing for the preparation of which chemical knowledge has not been employed, and the same knowledge is necessary to show how the spread of disease may be prevented, and cured when it has taken hold. While these practical results are obtained by the study of chemistry, Dr. Fittig is careful to show that it is a total misunderstanding to suppose that its chief purpose is to discover brilliant colours or new medicines. Thus, without undervaluing the practical importance of the discovery of the aniline colours, it is nevertheless true that the splendid results obtained by Hofmann would have had the same interest for the chemist, had these compounds been colourless and without any technical use. So we are told, "The task of chemistry is to explain the composition of bodies and all phenomena resulting from change of this composition *in order to derive the regular connection and cause of these phenomena, and therefore also of the natural laws which regulate the building up and decomposing of substances.* . . . . We are compelled to multiply the number of substances already existing in nature, not for the sake of producing new bodies and benefiting the world, *but to discover the eternal laws of nature.* He is no true chemist who only prepares new compounds without any definite aim (although, perhaps, he has prepared a large number of compounds hitherto unknown and possibly very beautiful in appearance), and his work has no direct value for science, and can only become valuable when employed by others in its true scientific sense. . . . . True scientific researches must never be given over to chance, they must be systematically planned, begun with a clear consciousness of what is to be attained, and finished in the same spirit." Dr. Fittig has done well to point out so clearly the true aim of the science of chemistry, and to disparage the false estimation of its value, which would make it simply a means of discovering bodies with some technical or useful application. And even in this direction, which must always be looked upon as of secondary importance, we are convinced that greater progress will be made if chemistry is regarded and studied from the high point of view so forcibly pointed out by Dr. Fittig in his interesting address.

F. J.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Chemical Research in England

In confirmation of your remark in the last number of NATURE that, in regard to scientific discovery, we in this country "are conspicuous for our prominent position in the rear," will you permit me to state the result of an inquiry which I lately made into the comparative activity of this and other countries in the prosecution of chemical research?

In the year 1866 there were published 1,273 papers on new discoveries, by 805 chemists, 1·58 paper being thus the average produce of each investigator. Of these, Germany contributed 445 authors and 777 papers, or 1·75 paper to each author; France, 170 authors, and 245 papers, or 1·44 paper to each author; whilst the United Kingdom furnished only 97 authors, and 127 papers, or 1·31 paper to each author; all other countries yielding 93 authors and 124 papers, or 1·33 paper to each author. Thus, not only are we far behind in the aggregate of activity in discovery, but our individual productiveness is also markedly below that of Germany and France. From a purely national point of view, our case is even worse than it appears to be from a comparison of these figures, since a considerable proportion of the papers contributed by the United Kingdom were the work of chemists born and educated in Germany.

It will be seen that the above comparison covers chemistry only; but there is every reason to believe that in other sciences, the progress of which depends, like that of chemistry, upon experimental investigation, our position is still worse. It is highly remarkable that a country which, perhaps more than any other, owes its greatness to the discoveries of experimental science, should be distinguished for its neglect of experimental research. But the causes of this anomaly are sufficiently obvious; they are:

1st. The want of suitable buildings and apparatus for the prosecution of such investigations.

2nd. The non-recognition of experimental research by any of our universities.

With regard to the first of these causes, the prosecution of experimental discovery in this country is rendered extremely difficult, if not impossible, to those who do not possess ample private fortunes; and even to such as have this advantage, it is by no means easy. A laboratory of research is not a convenient or agreeable adjunct to a dwelling-house, and it is generally prohibited by the terms of the lease or covenant; indeed it is agreed on all hands that most of the operations which are required for the prosecution of inquiries in chemistry, physics, and physiology, ought only to be carried on in buildings specially devoted to the purpose. But where are such buildings to be found? Our chemical laboratories are only adapted for beginners, there is not in any one of them a separate department constructed and fitted for original research. Still less is this the case in physical and physiological laboratories; indeed until Sir William Thomson instituted one in Glasgow some three or four years ago, there was not in the United Kingdom a physical laboratory even for beginners. In Germany, on the other hand, the noble State laboratories of Berlin, Leipzig, Bonn, Heidelberg, Königsberg, and Stuttgart, are provided with special departments where the experimental investigator finds ample convenience and the necessary but costly instruments of precision provided for his use, the payment of a moderate fee only being required to secure all these advantages.

With regard to the second case, the highest degrees, and even honours in experimental science, are given in all our universities without any proof being required that the candidate possesses the capacity to conduct an original experimental investigation, or that he is competent to extend the boundaries of his science. On the other hand, in all the Prussian Universities, and in the best German Universities generally, no candidate is even admitted to examination for his degree unless he first submits to the senate a dissertation on some original experimental investigation conducted by himself. This investigation must also have a sufficient importance; for, as a matter of fact, more candidates are rejected on the ground of insufficiency of dissertation than through failure to pass the subsequent examinations. The entire ignoring of research in the granting of degrees in this country not only effectually prevents the training of students in experimental investigation, and the actual execution of researches by students; but it has also a direct tendency to divert the attention of professors and teachers from original research—they are not called upon to devise, as is the case in Germany, suitable subjects for research to be pursued by their students; and thus, not only is their attention withdrawn from this all-important field of experimental science, but, as their students have to be trained for subjects which are foreign to research, they feel that to devote any considerable portion of their own time to it would be to that extent to neglect their class duties.

E. FRANKLAND

Dublin Observatory

IN an account of the Observatory of Trinity College, Dublin, given in NATURE of March 16, 1871, there is a slight mistake in the date of the erection of the Transit Instrument, which is there assigned to 1808. It was erected many years before; for in the second volume of the Transactions of the Royal Irish Academy, Dr. Usher describes observations made with it in 1785.

I should not have thought the correction of this error necessary but for the fact that this transit marked the epoch of a most important improvement in astronomical instruments. It was the first in which the illumination of wires was effected through the axis by an internal reflector. This invention is described by Usher in the volume already referred to.

1808 was the date of the circle's erection; it having been ordered in 1783. This delay was in one respect fortunate. Ramsden, having quarrelled with Usher, resolved that the latter should never have the circle. On Usher's death Ramsden set to work to complete it, but found, to his dismay, that the extremities of its radial arms had become "rotten," having been acted on by the sulphurous atmosphere of London.

As originally constructed, it was ten feet diameter. He removed the rims (which, I believe, had been also acted on), cut away about six inches from each of the arms, and found the remainder sound. But as he was doubtful about its permanence, he let it lie several years longer, and found his apprehensions verified. He cut off six inches more from each arm and awaited the result, notwithstanding the urgent expostulations of Brinkley; and it was not until a short period before his death that he was satisfied that no farther change was probable. He then completed it at its reduced diameter of eight feet. But it was not divided till after his death (by Berge, his successor).

It is not easy to explain why this destruction was confined to the ends of the arms. To judge from the analogy of the Palermo Circle, the diameter of these arms at the outer extremity was very small; and if they were of cast brass, the molecular condition of the metal there, in consequence of the more rapid cooling, may have been different from that of the more massive portions.

A still more remarkable instance of this destructive action occurred to a circle described by Mr. Bond in the Philosophical Transactions, 1806, and known as the Westbury Circle. This was ultimately established at the old Observatory of the Glasgow University, and in an atmosphere still more sulphurous than that of London. When this University was broken up, and its instruments sold, this circle was purchased by the late Sir James

South. But on its arrival at his Observatory it was found to have suffered so much that it actually fell to pieces! Only a few of the more massive parts were entire; and of the rims of the circle nothing remained except that which carried the divisions, which, as I was informed by Troughton, was of "Dutch brass," and was quite unchanged.

The excellence of this Dutch brass is, I believe, recognised also by watchmakers, and it seems to deserve inquiry to what its superiority over English brass is to be attributed.

It is worthy to be mentioned that among the instruments ordered from Ramsden by Usher was an equatorial telescope driven by clockwork. But owing to Ramsden's feud with Usher, this was not executed; and this important aid to the astronomer, which had been proposed by Hook nearly a century before, lay dormant till it was applied by Frauenhofer, forty years later, to the Dorpat telescope. T. R. R.

Armagh

Morell's Geometry

IN answer to your criticisms on my work, "The Essentials of Geometry," I have given an explanation as to the sources of that book. I proceed to give very briefly my arguments for what is there advanced.

I shall do so under two heads, which will be in the form of indictments against my reviewer.

For I charge him, first, with overlooking the nature and object of the book; and, secondly, with overlooking the context of passages he criticises, an omission that changes the entire aspect of the case.

First it is explained (Preface, p. vii.) that my little volume is an attempt at a manual and memento for students, of which so many exist in France and Germany. It is notorious that such works do not dispense with others, or touch the plan of school text-books, and then much of their contents consists of results with hints of demonstrations. Then at p. viii. of Preface it is added that an inspection of the methods employed will show that German and French geometricians . . do not condemn the student to keep a geometrical figure rigidly in the place in which it is laid down on paper. Revolution and superposition are allowed . . . simplifying and shortening the proof, &c.

Passing to the second indictment, I find that the criticism levelled against my so-called theorems of parallels commits two serious offences. First, it garbles my matter; secondly, it overlooks the principles first laid down in the preface, applying from logic two propositions treated by me both manually and logically; thirdly, it overlooks claims in pp. 15, 24, 25, where the technical terms in p. 20 are explained; lastly, it ignores the fact that all our theorems about parallels rest on assumptions and not on logic.

It is evident that if superposition be allowed, two parallels as they cannot cut (hyp) must coincide, thus the angles they form with a secant will be equal.

Again, the charge of want of logic in my proof of the inequality of triangles with three equal sides falls to the ground, if the first clause of p. 44 be read. For as this is a case of superposition by making the bases coincide, the arms of both triangles must coincide as radii of equal length intersecting at only one point on the same side of the base. The proof is equally direct from symmetry, from inversion and juxtaposition, and from subposition, as in the Notes to Todhunter's School Euclid.

As to the critic's difficulties about explaining the coincidence of two semicircles, to any one used to the free handling of geometrical figures in France and Germany, the thing wears a ludicrous aspect.

Then about the statement that two equal adjacent dihedral angles are right angles, a moment's reflection shows that if a dihedral angle be defined (Bos. p. 32) as an angle formed by the revolution of a movable plane at its common section over a stationary plane, when it reaches the point where the two adjacent angles are equal, they must be right angles. The definition may be disputed, but the conclusion is correct.

As regards the criticisms on my definitions, I do not think it necessary to enter into this matter. The ground of definitions is a wide and a disputed one, and I am content to err, if err I do, with very high authorities. Euclid has defined a straight line to be that which lies evenly or equally between its extreme points. This definition affords no assistance in arriving at the properties of straight lines. In Dr. Simson's edition, a point is defined to be that which has no parts or no magnitude. This is objectionable, as being wholly negative. Again Dr. Simson, in the notes to his edition of Euclid, admits that the 11th axiom is not self-evident.

In conclusion, the reviewer is of course as likely to attack the free treatment of theorems and problems practised, especially in Germany, by the conception of the generation of all figures from their elements by the movement of points and lines. But it can scarcely be charged against the author of the "Essentials" that he has not shown some of these shorter methods of demonstration as used in France and Germany.

The nature of revolution is fully illustrated at pp. 6, 8, and the treatment of angles as ratios in note 2, p. 9. Our limits prevented anything more than indications, but *verbum sat.* to the great logicians of England. J. R. MORELL

On the Derivation of the name "Britain"

HAVING been from home, I did not see the letter of "A. H." in your publication of March 16 until yesterday. His only objection to my derivation of the name *Britain* is that the word *tin* in his opinion was "not used in this island so early as the argument for its forming part of the word *Britain* requires. The following remarks will show that it *must* have been used in this island quite as early as the word *Britain*.

His assertion that "our word *tin* is of comparatively modern formation," cannot be established. It *must* have been familiar to the Cornish centuries before Diodorus Siculus described St. Michael's Mount, in Cornwall, under its name of *Iktin*, from whence tin was exported by the Phœnicians as far back as the time of Moses (Numb. xxxi. 22); and from none but the Phœnicians could the Cornish have derived the word *tin*—for that metal, as well as its name, was unknown to them before they were visited by the Phœnicians. The name *Iktin* (Tin-port) by which the Mount was called in the time of Diodorus, proves the existence of the word *tin* prior to that period, and the present Cornish word *stean* can only be a corruption of the very anciently adopted word *tin*—a corruption arising probably during the Roman period, so that instead of *tin* being a corruption of the Welsh *ystaen*, or of the Latin *stannum*, as "A. H." imagines, the reverse is evidently the case.

Assuming, with most authors, the original Phœnician word to be *tin*, that name continues unchanged in the Saxon, English, Dutch, Danish, and Icelandic languages; but the Swedish name is now *tinn;* the German, *zinn;* the French, *étain* and *tain;* the Latin, *stannum;* the Italian, *stagno;* the Spanish *estano;* the Portuguese, *estanho;* the Irish, *stan;* the Welsh, *ystaen;* the Cornish, *stean;* the Armoric, *stean* and also *staen*—the initial letter or sound *s* in each of the last nine names being a mere prefix, as in the modern word *sneeze* for *neeze* (Job xli. 18). With this exception, and except the ordinary terminations of the Latin, Italian, Spanish, and Portuguese names, these thirteen different spellings are merely the different ways in which different nations of Europe pronounce the Phœnician word *tin*.

Diodorus speaks of *Iktin* as an island adjoining *Britain;* and this island (for it *is* an island two-thirds of the day) was no doubt long before his time called sometimes *Iktin* and sometimes *Bretin;*—*Iktin* when it was regarded as a "port," and *Bretin* when regarded as a "mount"—*ik* being the Cornish for "port," and *bre* the Cornish for "mount." It was however most generally known as a mount, and as the most remarkable object in *Mount's* Bay, to which it has therefore given its *English* name, having long before the Christian era, in all probability, given its ancient Cornish name of *Bretin* to the island in which we live.

Plymouth, March 29 RICHARD EDMONDS

Records of European Research

THE Chemical Society has taken up a good cause, that of reporting foreign labours much more fully than could be worth the while for any periodical publication to undertake.

I have suggested in another quarter the advisability, if possible, of joining in this scheme. But funds are requisite to have the work well done.

It has struck me that, as a matter of completeness and economy, it would be far better if the learned societies subscribed, and the Royal Society made a grant besides its subscription, to engage an efficient staff to report foreign contributions not merely to one branch of science, but to all, forming, let us say, a quarterly *comptes rendus* of European research.

The practice of quoting the titles of foreign papers is aggravating in the extreme, especially when accompanied by the remark that "this laborious but lengthy paper is not suited for 'abstraction!'"

I seldom go out of our foggy little island without hearing our want of familiarity with what is going on elsewhere roundly and deservedly abused. But I feel bound to say that, more especially in France, I can justly retaliate.

Russian science is said to be very productive. That language, at all events, is beyond most of us.

New University Club MARSHALL HALL

Aurora Australis

OBSERVING in NATURE of Oct. 27, an account of a brilliant aurora observed in England on the 23rd to the 25th of that month, I think it may excite attention to the subject by informing your readers that at the same date a splendid display of Aurora Australis was recorded at most of the meteorological stations in this colony.

The extent of sky covered was not so great as in the fine display on 5th April last, which was also coincident with a remarkable Aurora Borealis, but the red tint was so much deeper than usual, that many persons in this place attributed the phenomenon to the glare of the great fire which destroyed the town of Lyttelton, and the news of which was just then received by telegraph. JAMES HECTOR

Colonial Museum and Observatory, Wellington, N.Z., Jan. 2

Ocean Currents

MY rejection of the idea that permanent differences of atmospheric pressure could produce any effect on Ocean Currents, was meant to be as sweeping as Mr. Johnston considers it. I believe that the idea is repugnant to the most elementary conceptions of hydrostatic equilibrium; and I am particular in so far repeating the gist of my former letters, because Mr. Johnston, in his letter in NATURE for March 9, reiterates his suggestion that difference of atmospheric pressure is a power in the production of Ocean Streams, and whether he suggests that it is a supplementary power, or a chief one, is nothing to the purpose, if, as I distinctly maintain, it is not a power at all.

My rejection of the idea that the formation of these differences of pressure can produce any appreciable effect, is quite as decided; but Mr. Johnston is mistaken when he speaks of my denying also the influence of the movement of these differences of pressure; for my remarks concerning them were to the very opposite purport; and I pointed out that such movements do sometimes give rise to rapid and dangerous sets, known as storm-currents, which in their irregular and exceptional nature, differ essentially from those regular permanent or periodic currents usually understood by the general term Ocean Currents, though they may occasionally modify them both in direction and velocity.

I would also call Mr. Johnston's attention to the fourth paragraph of his letter, and assure him that I have never, directly or indirectly, maintained that the Trade Winds "would account for the whole of the phenomena of Ocean Currents;" but I have maintained, and do still maintain, that all the phenomena alluded to may be very satisfactorily accounted for by a reference to the prevailing winds of the different parts of the world; and that the Gibraltar Current is to be attributed, not to the local, partial, and peculiar wind of the Straits, but to the great body of the west wind of the North Atlantic, which also produces a northerly current on the coast of France, known distinctively as Rennell's Current, and a southerly current on the coast of Portugal.

I have discussed this question so fully in another place, that I should be only repeating myself were I to say more about it here; but I may add that though, as Mr. Johnston asserts, under-currents cannot be caused primarily by the action of the winds, they can be, and frequently are, caused secondarily by that action; and many a ship has owed her safety from the apparently imminent danger of a lee-shore, to the "under-tow," or reflux of the water swept towards the shore on the surface. If there is a deep-flowing outward current in the Straits of Gibraltar, I believe it to be exactly of the nature of an "under-tow;" it seems to me probable enough that there is occasionally such an outward current; but I cannot admit that the *one* observation of it which Dr. Carpenter considers he obtained, after several attempts made in vain, has abundantly proved its existence; still less can I admit that it is necessary to call in difference of temperature and density to account for it.

Mr. Croll considers that there is a similar escape of water, underneath, from the northern to the southern hemisphere, and his arguments appear to warrant the suggestion, although no such under-current, or system of under-currents, has yet been observed. I see no improbability in the idea; but so many mistakes have, at different times, been made by trusting rather to theory than to positive evidence of fact, that we cannot be too cautious in admitting the existence of such under-currents, without any reliable observations. For that recorded by Captain Maury, to which Mr. Johnston refers, has, from the vague manner in which it is described, no scientific value whatever. There is no mention of locality, season, direction of wind, swell or surface current, no mention of the relation between the effective area of the "block of wood loaded to sinking" and the barrega or breaker which floated it; the depth is spoken of as indifferently one hundred or five hundred fathoms; above all, no mention is made of any means being taken to distinguish between an apparent and real set of the breaker. It is quite clear that if the loaded block was lowered into still water, the breaker to which it was attached was, to a certain extent, moored, and the surface drift of the boat away from it would give it all the appearance of moving in the opposite direction. No mention is made of the method adopted to discriminate; or, in fact, of any method at all being adopted, or any attempt made to eliminate or neutralise the many errors which necessarily find their way into such an observation; all that we are told is that "it really appeared as if some monster of the deep had laid hold of the weight below, and was walking off with it." To such an account one is almost attempted to add—very like a whale.

I have dwelt on the thoroughly unsatisfactory nature of this experiment, because, from the description of it having been repeated in every edition of the "Physical Geography of the Sea," I find it constantly referred to—as Mr. Johnston has now referred to it—as a conclusive proof of the existence of strong counter under-currents at great depths; whereas in reality it is a proof of nothing, unless, perhaps, of the careless style of observing which was accepted as sufficient twenty years ago.

The other instance which Mr. Johnston brings forward would be really remarkable, if we only had some evidence of it as a fact; he speaks of the warm water of the Atlantic dipping down beneath the cold and "specifically lighter" water of the east Greenland current. It has been well known, long before the late German expedition, that at the meeting of the two waters there is a distinct line of demarcation, but such a line does not necessarily indicate a dip of either water, such as Mr. Johnston describes; as indeed has been very fully shown by the survey of the nearly vertical "cold wall" of the United States, along which the line of demarcation is more distinct than anywhere in the world. And besides, can we admit that the water of the East Greenland current is "specifically lighter" than that from the Atlantic? that the cold water is lighter than the hot, the salinity of the two being very nearly equal? Captain Maury speaks of hot water, like oil, running over cold; Dr. Carpenter illustrates the same idea in a long glass trough, showing plainly enough the way in which he conceives the interchange to take place. I do not attach so much value as Dr. Carpenter does to this illustration, which represents a system of motion entirely different from that of the ocean-currents; but accepting it as the exposition of the views held by the leading supporters of the claims of temperature and density, it is utterly antagonistic to the idea of this extraordinary dip of warm water said to take place near the east coast of Greenland. Whether we consider it from a purely theoretical or from a geographical point of view, the idea is wholly unsupported, and can only be classed as one of those crude speculations which, in every branch of science, do so much harm by tending to unsettle the minds of those who indeed take an interest in the subject, but have not made it a special study. J. K. LAUGHTON

Draper's Experiment simplified

WISHING to repeat Draper's Experiment, and casting about for a simple method of performing it, it occurred to me *to take* advantage of the intense heat evolved in the combustion of sodium, and, beginning with the entire spectrum, watch its degradation as the heat declined; to which end I formed a shallow, conical cup of thin copper wire, half an inch in diameter, and, putting therein a piece of sodium, applied a spirit-lamp till it burst into flame; very soon the mass melted and rose to an intense, white heat, the air streaming in through the spiral greatly favouring the combustion, a full spectrum of the utmost purity and splendour was formed, which continued as long as the white heat lasted, but afterwards declined from, and rapidly at, the violet end through the whole spectrum to the red, which persisted longest. On repeating the experiments, and raising a very narrow slit to the spectroscope, I found, as I anticipated, the sodium line reversed, and I had before me a miniature sun, a glowing centre emitting light of every wave-length, while the melted sodium, flowing through the intervals between the wires of the lower part of the cone and being dissipated by the heat, surrounded the liquid centre with an atmosphere absorbing light of its own refrangibility.

Thinking some of your readers would like to repeat so simple and inexpensive an experiment, I have ventured to send you this.

T. F.

St. Mary Church, Torquay

A Wind-direction Rain-gauge

In your issue for yesterday, March 30, at page 433, you give a paragraph respecting a "Wind-direction Rain-gauge." Might I be allowed to observe that a gauge on the same principle has been in use at this place for many years, "it is arranged for *four vessels*" only, showing N. to E., E. to S., S. to W., and W. to N.

Wm. Lyall

Literary and Philosophical Society, Newcastle-upon-Tyne

Entomological Queries

Can any of your entomological readers refer me to any works or memoirs on British Ants published since the appearance of Westwood's "Classification of Insects," and not quoted by Mr. Smith in his volume on "Formicidæ" in the British Museum Catalogue? What is the scientific name of the Texas agricultural ant and of the smaller ant which it ejects from its colonies? And where can I find M. Lespes' paper or papers on the "Domestic Economy of Formicaries," especially with regard to the Clavigers and other blind beetles?

A. Emmet

Feb. 21

Rain produced by Fires

In your No. of Feb. 16, there is a letter from Mr. Laughton on the Artificial Production of Rains, which is worthy of notice from a strictly scientific point of view. I have little doubt that rains have been in comparatively rare cases caused by large fires. We may dismiss from our minds the idea that rains can be produced, even when the conditions are favourable, by all the powder that is burnt during a battle on land or sea. It is said that "in a problem of this nature, negative examples have more weight than positive." But it is surely more philosophical to hold that the one class of instances is as valuable as the other. If rains have sometimes been produced by fires, it is as well to try to eliminate the conditions under which they occur as in those cases in which they do not occur.

It is curious enough that much of the popular belief as well as a disbelief in the connection between rains and fires must be ascribed to the late Prof. Espy. After laying down with scientific accuracy the atmospheric conditions for such an occurrence, he somewhat lost sight of the principles in his instructions to the farmers of the United States for burning their felled timber for the production of rain. I need not enter into these. The negative cases are found to be numerous enough. Great fires rage over the Prairies and through the woods in America for weeks during the autumn, and the air becomes darkened by a veil of smoke, while no clouds are to be seen. This usually occurs when the wind is from the west and the air dry, and naturally blue and bright till obscured by smoke. In such instances the theory of fires producing rains justly enough becomes unpopular.

On the other hand Espy laid down with great precision the conditions under which rains will result from great fires in "a high dew-point and a calm atmosphere." In short, the air must be pretty well saturated with moisture, and verging upon that unstable state of equilibrium under which cumulus clouds are formed. As Espy has shown, and every observer of the phenomena will confirm, the gorgeous cumulus clouds of summer are not seen when the air is much disturbed by winds. Their very formation and existence depend upon ascending currents. Previous to Espy's investigations, it was supposed that the formation of cumulus was due to the expansion of the vapour of water by the heat of the sun and its consequent diffusion upwards through the permanent gases till it was condensed above. He clearly showed by experiment that vapour has little power of permeating air under the ordinary pressure of the atmosphere. And he drew the inference that it could be only carried into the higher strata by ascending currents. This, I think, is amply borne out in the formation of all clouds.

Fires then, are only likely to produce rains during comparatively calm weather. In the positive instances given by Espy, most of the observers state that the air was calm and sultry. One of the staff of the United States surveyors when in Florida mentions, that by firing the bush "whenever there was no wind stirring, we were sure to get a shower." Great fires are comparatively rare in this country, and I have never seen the formation even of cloud here from such a cause. However, I did once observe this phenomenon. Many years ago when sailing up the Mississippi near its mouth, in a clear and cool evening, after the subsiding of a "Norther," a great fire was burning among the reeds on its west bank. Above the dark smoke the true cumulus cloud was distinctly formed. Its bright and rounded form was beautifully brought out in the setting sun. No other clouds were visible around, and these were soon left behind as we ascended the river.

If we reflect on the matter it is difficult to conceive how clouds could be formed by means of fires during windy weather. The ascending columns could not be formed under such conditions, for the heated air would be rapidly swept off, and diluted with the mass of air rushing past. This may be illustrated by other examples better known than the formation of the cumulus. Volcanoes are well known to produce at times clouds as well as rains. But all the vapour or heat that Vesuvius could emit during an active eruption would not produce rains when a strong and dry north-west wind was blowing across its top. So also the beautiful and true cumulus cloud that so often hovers over the Falls of Niagara is only seen in calm weather. Under favourable atmospheric conditions I have lately been informed by Dr. Henry Washington that the Niagara cloud sometimes gives rise to rains and electrical phenomena. The true inference seems to be that great fires will not produce rain, excepting "the air is calm, and the dew-point high."

Robert Russell

Pilmuir, Leven, Fifeshire,

A BILL TO ESTABLISH THE METRIC SYSTEM OF WEIGHTS AND MEASURES

The following Bill, prepared and brought in by Mr. J. B. Smith, Sir Charles Adderley, Sir Thomas Bazley, Mr. Graves, Mr. Baines, Mr. Albert Pell, Mr. Muntz, and Mr. Dalglish, has been ordered to be printed by the House of Commons:—

Whereas it is desirable that the weights and measures of the United Kingdom should be decimalised, and made to correspond with those of other countries.

And whereas the use of metric weights and measures is now legal, but no provision has been made for procuring the standards of said metric weights and measures, and for verifying and stamping those in use under the said Act.

Be it enacted by the Queen's most excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons in this present Parliament assembled, and by the authority of the same as follows:—

1. From and after the expiration of ( ) years after the passing of this Act, the length of the metre to be prepared under the authority of the Privy Council for Trade, verified by comparison with the original standard in Paris, having the words and figures "Standard Metre, 1871," engraved upon it, and kept in the custody of the Warden of the Standards, shall be and is hereby declared to be the unit or only standard measure of lineal extension, wherefrom or whereby all other measures of extension whatsoever, whether the same be lineal, superficial or of capacity, shall be derived, computed, and ascertained, and all such measures shall be taken in decimal multiples or decimal parts of their respective units.

2. The unit of the measure of surface shall be the square of ten metres, which shall be and is hereby denominated the "are."

3. The unit of the measure of capacity, as well for liquids as for dry goods, shall be the cube of a tenth of the metre, and the same shall be and is hereby denominated the "litre."

4. The unit of weight shall be and is hereby denominated the "gram." A thousand grams shall be and is hereby denominated the "kilogram." A standard of the kilogram shall be prepared under the authority of the Privy Council for Trade, verified by comparison with the original standards in Paris, and have the words "Standard Kilogram, 1871," engraved upon it, and the same shall be kept in the custody of the Warden of the Standards

5. For the more convenient use of metric weights and measures, it shall be lawful to take the double and the half of all the said units, and their decimal multiples and decimal parts.

6. The said weights and measures hereby established shall be and are hereby denominated the standard metric weights and measures, as shown in the table hereto annexed.

7. Copies and models of the same standard metre and kilogram shall be sent to the Lord Mayors of London and Dublin, to the Lord Provost of Edinburgh, and to all counties, shires, stewartries, ridings, divisions, cities, towns, liberties and places in which by law copies and models of the standard imperial weights and measures are required to be kept, and to such other places and persons as the President of the Committee of the Privy Council for Trade may from time to time direct.

8. All judges, magistrates and other person or persons who now are or shall hereafter be authorised by law to order or provide copies of the present imperial standard weights and measures, shall at all times hereafter have like power and authority in every respect to order and provide copies of the standard metric weights and measures, and to charge the expenses thereof upon the fund or funds, money or moneys, that would have been liable in case it had been copies of imperial weights and measures that had been ordered or provided.

9. All and every the provisions and provision which are by law in force with respect to the inspection, verification, reverification, stamping, counterfeiting and modes of conviction, with the penalty or penalties relating thereto, of the present imperial standard weights and measures, shall apply to and be in force with regard to the standard metric weights and measures in every respect as if the said standard metric weights and measures were comprised in and designated by the imperial weights and measures in the Acts relating to such inspection, verification, reverification, stamping, counterfeiting and modes of conviction, and the penalty or penalties relating thereto as aforesaid.

10. From and after the expiration of ( ) years from the passing of this Act, the imperial and all local or customary weights and measures shall be abolished, and every person who shall sell by any denomination of weights or measures other than those of the standard metric weights and measures, or such decimal multiples or decimal parts thereof as are authorised by this Act, shall, on conviction, be liable to a penalty not exceeding the sum of 40*s.* for every such sale.

11. From and after the expiration of ( ) years after the passing of this Act, if any person or persons shall print, or if the clerk of any market or other person shall make any return, price list, price current, or any journal or other paper containing price list or price current in which the denomination of weights and measures quoted or referred to shall denote or imply a greater or less weight or measure than is denoted or implied by the same denomination of the metric weights and measures under and according to the provisions of this Act, such person or persons or clerk of the market shall forfeit and pay any sum not exceeding 10*s.* for every copy of every such return, price list, price current, journal, or other paper which he or they shall publish.

12. As soon as conveniently may be after the passing of this Act, accurate tables shall be prepared and published, under the authority of the Committee of Privy Council for Trade, showing the proportions between the imperial weights and measures heretofore in use and the standard metric weights and measures hereby established, with such other conversions of weights and measures as the said Committee of the Privy Council for Trade may deem necessary, and after the publication of such tables all future payments to be made shall be regulated according to such tables.

13. And whereas the weights and measures by which the rates and duties of the customs and excise and other Her Majesty's revenue have been heretofore collected, are different from the metric weights and measures directed by this Act to be used: It is hereby enacted, that so soon as conveniently may be after the passing of this Act, accurate tables shall be prepared and published under the direction of the said Committee of the Privy Council for Trade, in order that the several rates and duties of customs and excise, and other Her Majesty's revenue, may be adjusted and made payable according to the respective quantities of the standard metric weights and measures directed by this Act to be used, and that on the expiration of ( ) years after the passing of this Act, the several rates and duties thereafter to be collected by any of the officers of Her Majesty's customs or excise, or other Her Majesty's revenues, shall be collected and taken according to the calculations in the tables to be prepared as aforesaid.

14. From and after the passing of this Act, and until the use of the metric weights and measures shall be made compulsory, the said metric weights and measures shall be deemed and taken to be legal weights and measures, and as such may be used for all purposes whatsoever.

15. As soon as conveniently may be after the passing of this Act, the metric standards to be provided under this Act shall be placed in the custody of the Warden of the Standards, and the Committee of the Privy Council for Trade shall cause the metric weights and measures in use under the present Act to be verified and stamped in the same manner as the imperial weights and measures are now required to be.

16. From and after the passing of this Act the "Metric Weights and Measures Act, 1864," shall be and is hereby repealed.

FLOATING ISLANDS IN VICTORIA

GIPPSLAND is a province of Victoria. It is bounded by the Australian Alps on all sides except on the south, which the sea washes for over 100 miles. It may be called the Piedmont of Australia, rich fertile plains intersected by rivers flowing into a lake system extending all along the coast, and separated from the sea by a sandy narrow ridge, with one navigable opening. From a local paper, the *Gippsland Times*, I send the following description of "floating islands" on the lakes.

The alluvial deposit constantly brought down from the mountain ranges by the numerous rivers in this district, enables us to see a very decided process of land making continually going on, and thus teaches a useful lesson in geology.

AUSTRAL-ALPINE

Melbourne

"As one of the Gippsland Steam Navigation Company's steamers was recently crossing Lake Wellington, the man at the wheel suddenly observed land right in the track of the steamer, apparently only a short distance from the straits separating Lakes Wellington and Victoria. He called the captain's attention to the strange sight, and on coming up close, the land was discovered to be a small island, about thirty yards in length by twenty broad. It was covered with a rich coating of luxuriant grass; and small trees, tea tree, and bush shrubs appeared to be growing in profusion. The only occupants of this remarkable apparition were a few pigs, feeding away contentedly and apparently enjoying their novel journey by water. A second island of the same description, but much smaller, was noticed a little farther on, but this had evidently detached itself from the larger piece of land, or most probably had been separated by the rooting depredations of the porkers. From what portion of the main land this floating island came, is, of course, matter of conjecture, but it is known that a portion of the soil at Marley Point, on the southern shore of Lake Wellington, became detached recently, and floated miles across the lake with some twenty or thirty head of pigs aboard. As long as the wind drove it in that direction, the island drifted towards M'Lennan's Straits, but a change of wind brought it back again, after a three days' trip, within a mile of the spot from which it had broken away. We believe it is the opinion of the district surveyor, Mr. Dawson, that the area of the Roseneath run, west of Lake Wellington, has been increased some twenty or thirty acres by the addition of drift islands."

WILHELM von HAIDINGER

WILHELM VON HAIDINGER is no more. He died after some years of failing health, though the illness to which he finally succumbed on the 19th of March was a short one. Among his veteran contemporaries in the mineralogist's craft, such as Breithaupt, Karl F. Naumann, Gustav Rose, and Karl C. von Leonhard, he must have stood second on the ladder of time, the venerable Breithaupt being some four years his senior. His father, Karl Haidinger, was a mineralogist, and indeed was for several years Professor of Mining at Schemnitz. But, while Wilhelm was yet an infant, his father died at Vienna, where he had, in his latter days, filled a post in the Imperial Mint.

The young Haidinger seems in some sort to have inherited his father's taste for minerals, for he joined the class of Mohs at Gratz, where that distinguished mineralogist was giving a new impetus to the study of his science by popularising it in what was termed a natural history system of classification, and by a systematic method of discriminating the different species of minerals; and subsequently young Haidinger went to Freiberg to complete his training in Mining. Count Breunner, who came to England in 1822, and was made a Doctor in Civil Law at Oxford, invited the young mineralogist to accompany him. He embraced the offer, and they travelled together through England, and together reached Edinburgh, where the energetic and winning character of the young Austrian, fresh with the lore of the famous lecture-room at Gratz, at once made him friends in the Northern Athens, in the University of which capital Jameson had already made Minerals a fascinating study. Among the friends he there made was Mr. Allan, the wealthy banker, who during the next year invited young Haidinger to make a home of his house while employed in translating the Mineralogy of Mohs into English. So after returning to Vienna, he once more, in 1823, came to Edinburgh, and made Mr. Allan's house his head-quarters till 1827. He appears to have been a sort of tutor to Mr. Robert Allan, the eldest son of his generous friend; and with him he travelled during these four years through Cornwall, and then through Norway, Sweden, Denmark, Germany, Austria, Italy, and France. It was mainly during these travels that the famous collection, afterwards the property of Mr. Robert Greg, and now in the British Museum, was formed.

During these four years he brought out his translation of Mohs' treatise, and wrote several Mineralogical papers for the Wernerian Society and the Transactions of the Royal Society of Edinburgh. Subsequently he joined with his brothers in starting a porcelain manufactory at Elbogen near Carlsbad. Here he continued till 1840, still, however, bringing out from time to time memoirs on new minerals or new observations on minerals already known. The minerals Edingtonite, Sternbergite, Fergusonite, Herderite, Erinite, Picrosmine, Johannite, Botryogen, and Hartite, are among those he studied and described previous to and during this period.

In 1840 he returned to his native city, Vienna, to devote himself more exclusively to the scientific pursuits he loved. Thenceforward his memoirs will be found distributed at pretty regular intervals through the Sitzungsberichte of the Vienna Academy.

Among the subjects that he worked at during the next period of his life were the optical phenomena exhibited by crystals in regard to light and colour; more particularly those of pleiochroism. He invented, for the investigation of these, the Dichroiscope, a simple but useful little instrument, enabling an observer to examine and compare the different characters of the absorption exercised by a birefringent crystal on light traversing it, according as the plane of vibration of the light is parallel or perpendicular to any one of the principal sections of the crystal. The description of Hauerite, a new mineral, in fact, a manganese pyrites, was given in 1847; that of Kenngottite in 1857. The Haidinger brushes, a subjective phenomenon due to the eye itself, and observed in looking towards a window through a tourmaline or Nicol prism, was an illustration of the acuteness of his powers of observation. A compendious and valuable treatise on Mineralogy, brought out in 1845, to take the place of an earlier treatise, was also, during this period of his life, continually undergoing revision for new editions; while new investigations of minerals were also appearing under his name

From the moment of the foundation of the Geological Institute for the Empire in Vienna, Haidinger was the obvious man to lead that younger generation by whose labours the new Institute was to be reared and supported. So he was its Director until some two or three years ago, when he retired from the position he had filled so well, with a Ritter's rank and a well-earned pension.

For the last twelve years of his life he had given his attention, almost to the exclusion of other scientific inquiry, to the subject of meteorites. He laboured indefatigably almost to the last in collecting specimens from any new falls of meteorites reported in any portion of the globe, that they might be added to the noble collection in the Imperial Museum; and he was always at work at the interpretation of the strange phenomena witnessed by those who have described the fall of meteorites in any language or country.

Such is a rapid review of the main features in the life of a man who seems always to have been at work; whose pen was one of the readiest and busiest; whose nature was ever genial and generous; and who, at the age of seventy-seven, has finished an honourable life's work, and leaves behind him a name which Austria may cherish as that of one of her illustrious sons, and which many an Austrian and many a foreigner will remember with warm respect; while those who enjoyed nearer relations with Wilhelm von Haidinger will assuredly ever remember him with affectionate regard.

N. S. M.

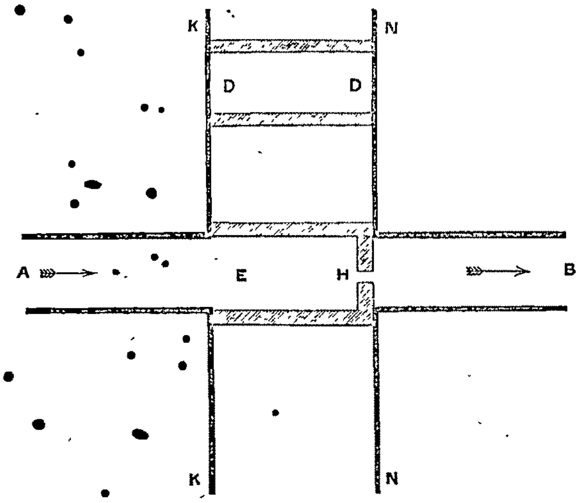
A TUBULAR POSTAL SERVICE

SOME large iron pipes have just been laid from the General Post Office to the Branch Office at Charing Cross, through which pipes packages of letters are blown in either direction at will, by compressed air. These tubes are to be extended from Charing Cross to the Houses of Parliament; and when the total expenses of laying pipes and of transmitting small packages through them is known by experience, very possibly the system may be extended, and letters intended for quick delivery may be sent by this method at a moderate charge.

This plan of sending messages through pipes for short distances has been employed in the City for many years in connection with the late Electric and International Telegraph Company. Seventeen or eighteen years ago, Mr. Latimer Clark laid down tubes from the Central Office of the Company in Lothbury to the Telegraph offices in Cornhill and Mincing Lane. By means of a steam-engine which worked a great air-pump, messages enclosed in small gutta-percha carriers, each somewhat resembling a sausage in shape and size, were drawn from Cornhill and Mincing Lane to Lothbury. Additional and smaller pipes were afterwards laid down by him, so that the vacuum could be applied to the further ends of the carrying pipes, in order that messages might be sent in the opposite direction also. They were then easily transmitted to and from Cornhill, but the Mincing Lane station being two-thirds of a mile off, it was found that the friction of the air in the pipes was too great, so that carriers could be sent in one direction only, namely, from Mincing Lane to Lothbury. Some years later, when Mr. C. F. Varley became engineer to the International Telegraph

Company, he employed compressed air to drive carriers to out-stations, and a vacuum to bring them back again. When a vacuum is employed, the carriers are driven by the ordinary atmospheric pressure only of fifteen pounds to the square inch, but when condensed air is employed, almost any pressure may be applied, so that the carriers can be driven with enormous velocity. He also substituted felt for gutta-percha carriers, since the latter were sometimes partially melted by the heat occasioned by friction, and coated the insides of the pipes with sticky matter. Further, he designed some pneumatic valves; the carriers, on arriving at the end of their journey, were made to strike against a brass button, the motion of the button set a valve to work, the valve opened the door of the chamber at the end of the pipe, the carrier then fell out, and dropped down on the table below. Thus the carriers were made to let themselves out when they arrived at the end of their journey, by which plan much hand labour on the part of assistants was saved. These improvements worked well, and are working well at the present time. Seven or eight City telegraph stations have been thus pneumatically connected for many years.

But a further improvement in the system has been made within the past year or two by Mr. C. W. Siemens. He lays down the pipes in circuits, and has pressure in the rear, and a vacuum in front of each carrier, so that as the motive forces all act in one direction, there may be several carriers flying through the tubes at the same time. If these carriers were not stopped anywhere, they would all find their way back to the central station. He has invented also a "shunt," whereby any intermediate station can stop its own carrier, and pick it out of the tube without interfering with the motion of other carriers which may be flying through other parts of the circuit. Suppose the carriers to be three minutes apart in point of time, and that five stations are on the circuit, each station knows at what time its own carrier is due, so is able to take it out without interfering with other carriers. If the carriers be not sent at regular intervals of time, the receiving station can be told when its carrier is started, by telegraph. The plan of picking out the carriers is simple, and the principle may be explained by the aid of the accompanying diagram. A B is the main tube, and the direction taken by the carriers is denoted by the arrows; K K and N N are metal plates, between which the two short tubes D D and E H slide in an air-tight manner. These two short tubes are attached to a lever joint. When the assistant does not wish to intercept a carrier, the tube D D is left in the place occupied by the tube E H in the cut, and D D being open at both ends, carriers pass through it without interception. Upon sliding E H into the gap in the main tube, however, the carrier is stopped by it. It is brought to a standstill very gently, because it compresses some air in front of itself, which air issues with restricted freedom through the hole H; thus the carrier makes for itself an air-cushion to break the violence of the blow. When the carrier is caught, the tube D D is brought into the line of the main tube, after which an air-tight door in the side of E H is opened, and the captured carrier, with its messages, is taken out.



For short distances to and from telegraph stations near the Bank and the Post Office, the pneumatic tubes are from one-and-a-half to two-and-a-half inches in diameter; they vary in diameter according to distance. But the one large Siemen's circuit at present laid in London goes from Telegraph Street to Charing Cross and back, the General Post Office and the Temple Bar Office being the intermediate stations; this pipe is three inches in internal diameter. The carriers travel at the rate of about a mile in three minutes, but the rate varies with the pressure.

It may be asked why these pneumatic tubes are useful in connection with telegraphic offices? The fact is, that there are many disadvantages in sending messages very short distances by the electric telegraph. Suppose one telegraphic wire be suspended between two stations half a mile apart, and another be suspended between two stations three hundred miles apart; let thirty messages be received all at once for transmission over each of these two wires, it is plain that some of these messages will have to wait half an hour before their turn comes to be signalled over the wire. The public will not complain of a delay of half an hour in the delivery of a message in a town three hundred miles off, but they would make a great outcry if a message took half an hour to go half a mile by the electric telegraph. Therefore, it is the simplest and most expeditious plan for the central telegraph station in a great city to blow the messages bodily through tubes, to branch stations not far off; the plan saves time and saves labour. Complaints published in the newspapers about delays in telegraphic messages, refer for the most part to telegrams sent from one part of London to another, and the delays are often caused by the pressure of a sudden influx of work upon particular wires. WILLIAM H HARRISON

NOTES

WE learn that the volume containing the various observations of the recent total eclipse will be edited by the Astronomer Royal.

IT is stated that Mr. Abel, Prof. Ramsay, and Mr. Huggins, have been invited to lecture this year to the members of the British Association at the forthcoming meeting at Edinburgh.

THE meeting of the Royal Colonial Institute on Monday is likely to have a practical result. The paper read was by Mr. Hyde Clark on the "Appointment of a Reporter on Trade Products for the Colonial Office." After an interesting discussion the President, Lord Bury, M.P., on the part of the Council, proposed that a Committee should be named to apply to the Secretary of State for the Colonies for such a department, and for the provision of a Colonial Museum on the same basis as the department provided at the India Office for India.

AT the last anniversary meeting of the Chemical Society it numbered 551 ordinary members and 36 foreign members. Six of the former have withdrawn from the Society,—on the other hand forty-two new members have been elected into the Society. It has lost five ordinary members by death, viz., Mr. George Jolley, Dr. W. A. Miller, Dr. Aug. Matthiessen, Dr. J. S. Muspratt, and Mr. W. W. Rouch; and the deaths must also be recorded of two foreign members, viz., Prof. Gustav Magnus and Prof. Weltzien. The election of the president, the officers, and the other members of Council for the ensuing year was then proceeded with, and the following is the list of the gentlemen elected:—President: Frankland, E., D.C.L., F.R.S. Vice-Presidents, who have filled the office of President: Brodie, Sir B. C., F.R.S.; De la Rue, Warren, Ph.D., F.R.S.; Hofmann, A. W., D.C.L., F.R.S.; Playfair, Lyon, Ph.D., C.B., F.R.S.; Williamson, A. W., Ph.D., F.R.S.; Yorke, Col. P., F.R.S. Vice-Presidents: Debus, H., Ph.D., F.R.S.; ilbert, J. H., Ph.D., F.R.S.; Noad, H. M., Ph.D., F.R.S.; Odling, W., M.B., F.R.S.; Redwood, T., Ph.D.; Stenhouse, J., Ph.D., F.R.S. Secretaries: Harcourt, A. Vernon, M.A., F.R.S.; Perkin, W. H., F.R.S. Foreign Secretary: Müller, H., Ph.D., F.R.S. Treasurer: Abel, F.A., F.R.S. Atkinson, E., Ph.D.; Bassett, H.; Bloxam, C. L.; Dupré, A., Ph.D.; Field, F., F.R.S.; Holzmann, M., Ph.D.; M'Leod, H.; Mills, E. J., D.Sc.; Roscoe, H. E., Ph.D., F.R.S.; Russell, W. J., Ph.D.; Smith, R. Angus, Ph.D., F.R.S.; Voelcker A., Ph.D., F.R.S.

DR. GEORGE BURROWS, F.R.S., Physician Extraordinary to the Queen, has been elected President of the Royal College of Physicians, in succession to Sir James Alderson.

A THIRD (revised) edition is now in the press of Mr. Darwin's "Descent of Man."

MR. JAMES CROLL calls our attention (*à propos* of our notice of his paper "On the Cause of the Motion of Glaciers," No. 68, p. 309) to the fact that he does not conclude the age of the sedimentary rocks to be 1,036,800,000 years, but *assumes* the period—for reasons stated on a former occasion—to be only 100,000,000 years. The drift of the paper was to point out a "new method" of determining the (mean) thicknes of the sedimentary rocks. The method leads to the conclusion that their thickness cannot be much over 2,500 feet!

DR. HOOKER has just started on a botanical expedition of eight or ten weeks into the interior of Morocco, a hitherto almost untried field. He is accompanied by Mr. R. Ball and one of the gardeners from Kew to assist in collecting plants.

WE learn from the *Journal of Botany* that Dr. Karl Heinrich Schultz-Schultzenstein, of Berlin, one of the most eminent botanists in Germany, was found dead in his bed on the morning of March 23rd. He had been engaged at his desk till past midnight. The deceased, though in his seventy-third year, was remarkably active, and was lecturer on physiology, as well as on botany, in the University of Berlin, with which he had been connected since 1822.

THE Medical Scholarship for Women in Edinburgh University of the value of 50*l.* for three years, offered for competition by Mrs. Garrett Anderson, M.D., and two other ladies, has been gained by Miss Annie Barker, daughter of Dr. Barker of Alderhot. It was awarded according to the results of the preliminary examination in Arts in the University.

WE learn from Paris that M. W. De Fonvielle was sentenced to death by the insurgents in consequence of an article published in the *Times* of March 27. We have however had the satisfaction of receiving from him this week our usual budget of Paris news. The only men of science who have ranked with the insurgents are M. Le Français, a former teacher in an elementary public school, M. Jules Allix, inventor of a new system of orthography, M. Charles Emmanuel, who opposes the theory of the rotation of the earth from west to east; Dr. Robault, a homæopathic practitioner, and M. Leroy, a foreman employed by Messrs. Hachette and Co. for reading for the press. A false rumour was circulated in the Quartier Latin that they were to send a delegate to take possession of the Observatory, the Ecole de Médecine, the Ecole de Droit, College de France, Institute, and Jardin des Plantes. All these establishments have, however, been left undisturbed in the hands of the scientific authorities.

LAST week the *Journal Officiel* of the insurgents printed the account of the sitting of the Institute. But "reactionary" papers having sharply commented on the piracy, the *Officiel* abstained from mentioning the sittings of that assembly. The students have to a man ranked amongst the defenders of order. Almost every educational establishment is closed during "the revenge of science" proclaimed by the Commune. Pupils of the Polytechnic School were sent home, lectures at the Sorbonne, College de France, and Conservatoire des Arts have been stopped. Libraries are closed, and no books are being published or selling in Paris. There is no question of reform as long as the rebels enjoy their rule, and the Garde Nationale their thirty sous a day.

THE Hunterian Professor of Comparative Anatomy at the Royal College of Surgeons concluded his course on the Characters and Modifications of the Teeth of Mammalia on Wednesday, the 29th ult. In the last lecture the methods of drawing inferences as to the affinities and habits of extinct animals from dental characters were illustrated by the much controverted case of *Thylacoleo carnifex* of Owen, an extinct Australian marsupial, known at present only by its skull and teeth. The animal is supposed by its original describer to have been one of the fellest and most destructive of predatory beasts, and to exemplify the simplest and most effective dental machinery for predatory life and carnivorous diet known in the Mammalian class, a proposition which Prof. Flower contested, showing by comparison with all the recent marsupials, that its affinities are with the existing diprotodont species (kangaroos, potaroos, and phalangers), none of which are purely predaceous and carnivorous, and that, therefore, there can be no reason for inferring that *Thylacoleo* had such habits, unless any special modifications of its teeth towards the carnivorous type could be indicated. This was, however, shown not to be the case, by comparison with all the various known truly predaceous carnivores, whether belonging to the placental or to the marsupial type of mammals. There are, therefore, no grounds for the assumption on which the name of the animal is based. But, on the other hand, neither can it, according to Prof. Flower, be classed among herbivores in the ordinary sense of the word; and all arguments against its "herbivority" derived from the structure of its molar teeth have no bearing on the proof that it was lion-like in its habits, as there are numerous alternative suppositions. Indeed, the teeth of this remarkable animal are so highly specialised and unlike those of any actually existing species, that it is impossible from analogy with recent forms to deduce its mode of life with any certainty, its organisation having in all probability been in conformity with some surrounding conditions which have now passed away.

AT a meeting of the American Ethnological Society of New York, held in October 1869, a committee was appointed for the purpose of organising a new body upon the basis of the society just mentioned, to be entitled the Anthropological Institute of New York. This committee lately issued invitations to the members of the Ethnological Society, and others interested, to attend at the house of Mr. E. G. Squier on the 19th of March, in order to complete the proposed arrangements by adopting a constitution and by-laws, to be formally presented to the meeting. In the present interest which attaches to studies relating to the past and present history of mankind and the development of civilisation, it is much to be hoped that this new society will establish itself on a firm foundation in New York, and carry out the mission projected for it by its founders.

FOR some years Dr. Burmeister, an eminent German naturalist and physicist, from Halle, has resided in Buenos Ayres, in charge of the National Museum in that city, and by his investigations and publications concerning specimens belonging to the Museum has given to it a great reputation. Quite recently, as we learn from *Harper's Weekly*, a murderous attack was made upon him by one of his servants, which was happily frustrated; but the newspaper comments upon the transaction developed the existence of so much animosity or jealousy toward foreigners on the part of the people that the doctor has finally determined to resign his position and return to Germany. Dr. Sarmiento, the President of the Republic, it is said, has endeavoured to change this determination, but apparently without effect. In parting with Dr. Burmeister, Buenos Ayres will lose one who has given to the country that position in science through his writings that Dr. Sarmiento has in literature, and his loss will not easily be made good. It is an interesting fact that both the National Museums of Chili and of the Argentine Republic are presided over by German naturalists, the director of the latter being Dr. R. L. Phillippi, well known in the scientific community.

In June next Professor Birkett will commence his course of lectures on the Nature and Treatment of New Growths, at the Royal College of Surgeons, on the conclusion of which Mr. Hulke will deliver three lectures on the Minute Anatomy of the Eye.

We learn from the *British Medical Journal* that Dr. E. Klein of Vienna has been appointed Assistant Professor in the new laboratory in connection with the Brown Trust for Experimental Pathology, which is about to be erected in London. Dr. Klein has been Professor Stricker's assistant for several years, and has contributed much to that author's "Handbook of Histology," now being published in English by the New Sydenham Society. By this change, Austria will lose and this country gain one of the most promising of young histologists. As an investigator and teacher of the structure of tissues, Dr. Klein has been for several years held in much esteem in Vienna.

The following are the arrangements for the Lectures at the Royal Institution of Great Britain after Easter, 1871:—On Tuesdays, April 18 and 25, and May 2, William Pengelly, F.R.S., will deliver three lectures "On the Geology of Devonshire, especially of the New Red Sandstone." On May 9 and 16, Charles Brooke, F.R.S., will lecture "On Force and Energy." On May 23 and 30, and June 6, the Rev. Professor Haughton, M.D., F.R.S., will lecture "On the Principle of Least Action in Nature, Illustrated by Animal Mechanics." On Thursdays, April 20 to June 8, Professor Tyndall, LL.D., F.R.S., wil deliver eight lectures "On Sound;" and on Saturdays, April 22 to June 10, J. N. Lockyer, F.R.S., will deliver eight lectures "On Astronomy." The lecture hour is three o'clock. The following are the probable arrangements for the Friday evenings after Easter, 1871, to which members and their friends only are admitted:—April 21, Professor Blackie, F.R.S.E., "On the Pre-Socratic Philosophy." April 28, Professor Odling, F.R.S. May 5, W. R. S. Ralston, M.A., Trinity College, Cambridge, "On Russian Folk-Lore." May 12, Professor Huxley, F.R.S. May 19, Colonel Jervois, R.E., C.B., Secretary of the Defence Committee, and Deputy Director of Fortifications, "On the Defence of the United Kingdom." May 26, Sir J. Lubbock, Bart, M.P., F.R.S., "On Relationships." June 2, Professor Thomas Andrews, F.R.S., Principal of Queen's College, Belfast, "On the Gaseous and Liquid States of Matter." June 9, Professor Tyndall, LL.D., F.R.S.

Prof. Winchell, director of the Geological Survey of Michigan, has lately presented a report of the progress of the survey from its inauguration, May 1869, to November 1870. He sketches an outline of the nature and extent of the researches he proposes in connection with the investigations, and expresses a desire for sufficient appropriations to enable him to complete his work in the shortest possible time, two years being suggested as sufficient with proper means. His plan includes, in addition to pure geology and mineralogy, such subjects as palæontology, climatology, natural history, ethnology, &c. The sum of 61,300 dols. is asked for by the Professor for the purpose of completing his field work, as well as of preparing the necessary maps and illustrations for his report.

We have information of the departure of M. Miclucho Maclay, of Russia, in the Russian steamer *Witiaz*, for a seven or eight years' cruise in the Pacific—the first two of which are to be expended in the investigation of the island of New Guinea. This region, as is well known, abounds in objects of natural history of the greatest interest, although comparatively little, so far, is known of its features in detail.

The Royal Belgian Academy of Science, Letters, and the Fine Arts has just issued its thirty-seventh *Annuaire*, containing a historical sketch of the Academy, and biographical sketches of the following members who died during the year:—François Joseph Navez, painter, with a list of his works and pupils; Edward Ducpetiaux, political economist and prison reformer; Charles-Fréd.-Phil. von Martius, the celebrated botanist; Edouard Gerhard, philologist and archæologist; Prudens Van Duyse, poet; and Charles Aug. de Bériot, composer.

The new explosive dualin has been used in the blastings required for the great Hoosac tunnel in the United States. Over 1,000lb. have been exploded since December 1, and it appears to possess the full strength of nitro-glycerin, besides being perfectly safe from any ordinary blaster to handle. It will not explode by concussion, and can be tamped as hard as powder with perfect safety. There seems no reason why it will not eventually entirely supersede common powder for all blasting purposes.

The *Scientific American* announces a substitute for lime in the lime-light of the oxyhydrogen jet. It appears that a prism cut out of the mineral dolomite will emit a light as powerful if not superior to the calcium light. The dolomite is made up of nearly equal parts of the carbonate of lime and magnesia, and the combination of these two earths produces effects superior to what can be obtained from either of them alone. The light is said to be suited for photographic purposes, especially for copying pictures. As dolomite is an abundant rock, its application for purposes of light may prove of peculiar value.

The report of the Manchester Field Naturalists' Society for 1870 has, according to the Secretary, "little to say except that the year's proceedings have been marked by smoothness and success, with no particular incidents to give it distinction above preceding years, and certainly without any of an infelicitous kind." The meetings have been well attended; there has been a considerable entry of new members; and the Treasurer's report is satisfactory. The report is taken up with brief *résumés* of the proceedings at each meeting, and a summary is given of a useful paper by Mr. R. B. Smart "On the Variation of Species" in the vegetable kingdom. We much regret that, from a society numbering its members by hundreds, we cannot obtain a proportionate amount of work; indeed many of the smaller bodies put the Field clubs of our large towns to shame in this respect. Mr. Grindon's "Flora of Manchester" is not only out of date, but also out of print; and a complete fauna and flora of the district would be both useful to naturalists and creditable to the society. Surely among so many members some may be found both able and willing to undertake such a work. We observe that the Secretary, in the present report, speaks of plants by their English names, some of which are of his own invention. As their scientific equivalents are omitted we are left in the dark as to some of them; "dimplewort" is, we believe, *Cotyledon umbilicus*, but "blushwort" baffles our ingenuity. The President for 1871 is Mr. Thomas Turner, F.L.S., and Mr. Grindon continues to act as Secretary.

THE time for the trial of machines for separating the fibres of the Rhea plant which are to be sent in by competition for the Indian Government prize of 5,000*l.*, has been postponed till April 1872. It is requested that notice of intention to compete be given before May of this year. Arrangements have been made for supplying some of the plant to intending competitors.

THE FIRST GERMAN NORTH POLE EXPEDITION

A NUMBER of Petermann's "Mittheilungen" published in January 1871, consists of an account of the first German North Pole Expedition by Captain Koldewy and Dr. A. Petermann. The vessel in which this expedition was undertaken was the *Germania*, a cutter of only eighty tons burden. Twelve persons sailed in her, Captain Koldewey, the commander of the party and joint author of the present memoir, R. Hildebrant, chief mate, and ten sailors. They started from Bergen May 24, 1868, the Swedish expedition sailing about the same time in a steamer. The voyage extended over four months. Dr. Petermann considers that the only practicable routes to the North Pole are either through Behring's Straits or the sea between Greenland and Spitzbergen. The latter was attempted by the Expedition.

The year turned out to be a most unfavourable one, the sea being more than usually obstructed with ice. After vainly attempting to reach the east coast of Greenland, the *Germania* crossed over to Spitzbergen, but was stopped by pack-ice. Greenland was again visited with a like result; but on a second trial of the Spitzbergen route a fortunate break in the ice occurred, and on September 14, lat. 81°·5 was reached, this being the highest point ever yet attained by a ship, although with sledges 82° 45 was reached by Parry in 1827. The east coast of Spitzbergen was visited by means of the straits (Hinlopen Strasse) which separate the smaller northern portion of this group of islands from the larger southern portion. Here a new island was discovered, and the surrounding coast-line mapped. Dr. Petermann names the island William Island, and the straits which separate it from the mainland Bismark Straits; we also find on the map Augusta Bay and Cape Moltke. Dr. Petermann rejoices greatly that the Germans have thus at last left their mark on the map. He says that it has been very trying to him to have seen for the last thirty-two years in maps of all parts of the world containing new geographical discoveries no names but "Victoria," "Wellington," "Smith," "Jones," &c. Captain Koldewey describes the glaciers of Spitzbergen as differing from those of Switzerland in the following points:—They for the most part stretch right down into the sea, where they end in a perpendicular wall. The upper surface is somewhat polished and free from all roughness and steep ice blocks. Moreover in the glaciers examined at Augusta Bay and William Island there are no crevasses. Moraines are present, those of the great glacier in Augusta Bay consisting of limestone and basalt. The actual scientific results of the expedition are very small, owing to the badness of the weather. Some fragmentary monthly isothermals of sea temperature have been constructed by Dr. Petermann from the observations made during the voyage, and are marked on the two maps which accompany the memoir. Captain Koldewey considers that the route by the East coast of Greenland is the one which should be attempted by future expeditions. The route north of Spitzbergen is impracticable, because a branch of the Gulf Stream here meets directly the cold polar current, and a barrier of ice is the result. For further exploration he advises the employment of a schooner rigged vessel of from 150 to 200 tons, with auxiliary steam power.

REPORT ON DEEP-SEA RESEARCHES

Carried on during the months of July, August, and September, 1870, in H.M. Surveying Ship "Porcupine."

By W. B. CARPENTER, M.D., F.R.S., AND J. GWYN JEFFREYS, F.R.S.

(*Concluded from p.* 417.)

WE commenced our observations on the morning of Oct. 1 at the point of greatest depth (Station 65). The temperature of the surface at 6 A.M. was only 63°, which was at least eight degrees lower than the average temperature at that hour within the Mediterranean. The bottom temperature at 198 fathoms was 54°·5; and the specific gravity of the bottom water was 1028·2. The coincidence both in temperature and specific gravity with the bottom-water at Station 64 was thus very close. The place of the ship having been determined by angles taken with the shore, the rate of the surface-movement was tested as on former occasions; and was found to be 1·277 mile per hour, its direction being E. ½ S. The "current drag" was then sunk to 150 fathoms,—the greatest depth at which it was thought safe to use it; and the boat from which it was suspended moved E. ¾ N. at the rate of 0·840 mile per hour. This observation indicated a very considerable retardation in the rate of *in*-flow; but gave no evidence of an *out*-flow. It did not, however, negative the inference deducible from the temperature, and still more from the specific gravity of the water beneath, that an out-flow takes place in that lowest stratum which we could not test by the "current drag."

We then steamed across the deep channel towards the Spanish side; and passing a bank of 45 fathoms which rises near its middle, we sounded again at Station 66, about six miles to the northward of Station 65. The surface-temperature at 9 A.M. was here found to have risen to 69°; and since not more than half this increase could be attributed, according to our experience elsewhere, to the increase of direct solar radiation at this period of the day, the cause of the additional elevation has to be sought elsewhere. The length of sounding-line run out was 147 fathoms; but on attempting to reel it in, the lead was found to have fixed itself between rocks; and all Capt. Calver's skill in the management of his ship proved inadequate to free it. As we were thus anchored by our sounding-line, it was requisite to set ourselves free, by putting a breaking strain upon it; and we thus lost, with the lead, one of our water-bottles, and a pair of thermometers, one of which was specially valued by us as having been used throughout the *Porcupine* Expedition of 1869, in which the temperature soundings had proved of peculiar importance. The "current-drag" was here let down to 100 fathoms; and the boat from which it was suspended moved along in the direction of the surface-current, and at the rate of 1·28[illegible] mile per hour, which was almost precisely that of the surface-current in the previous observation.

Deeming it important to obtain the temperature and specific gravity of the bottom-water on the Spanish side of the deeper portion of the channel, we slightly shifted our ground, and again let down our lead, with thermometers and water-bottle, at Station 67, where the depth proved to be 188 fathoms. On beginning to reel in the line, we found the lead to have anchored as before, and for some time feared that we should sustain a second loss of the water-bottle and thermometers attached to it. The means taken by Captain Calver for its extrication, however, proved on this occasion successful; and we had the satisfaction of seeing the whole apparatus safely brought up,—the lead bearing evident marks of having been jammed between rocks and then violently strained. The temperature of the bottom proved to be 55°·3, that of the surface being 73°; and the specific gravity of the bottom-water was 1028·1, that of the surface being 1026·8. Here again, therefore, the evidence afforded by the temperature and specific gravity of the bottom-water was conclusive as to its Mediterranean character. Its density corresponded rather with that of the bottom-water, than with that of the intermediate stratum, at the opposite end of the Strait; but the more rapid westerly motion of the latter would seem to indicate that the water which here flows over the "ridge" is derived from it, rather than from the deeper layer, and that its diminution in density is due to the dilution it sustains in its course. In either case, the denser Mediterranean water discharged by this undercurrent must flow up-hill; but the incline is so gradual that a very small force, if constantly sustained, would suffice to produce the elevation needed to carry it over the ridge.

Whilst we were prosecuting these inquiries, our attention was attracted by the long chains of aggregate salpæ which were floating close to the ship near the surface of the very calm sea. We were able to collect four or five different species of these, and to submit them during life to microscopic examination. The reversal of the direction of the circulation took place in all at more regular intervals than we have usually found to be the case in the compound ascidians; and we were able to distinguish an unmistakable rudimentary eye, which had not, we believe, been previously noticed. We hope to be able hereafter, by the detailed study of these specimens, to make some additions to the knowledge previously acquired of this very interesting group. As the nature of the bottom put it out of the question to attempt to dredge on this ridge, our only means of investigating its zoology lay in the use of the "hempen tangles." A "sweep" taken with these brought up a few echinoderms and polyzoa of no special interest.

We now took our final leave of the Mediterranean basin with mingled feelings of disappointment and satisfaction. The zoological results of our cruise had been by no means equal to our expectations; but, on the other hand, we could console ourselves with the belief that our determination of the peculiar physical conditions of this great inland sea, and in particular our elucidation of the mystery of the Gibraltar current, would be fairly regarded as a success. And we venture to think that this will be admitted by such as may follow us through the discussion of general results, to which we shall presently proceed.

As Captain Calver considered himself bound not to make any unnecessary delay in returning homewards, and to take every advantage of the continuance of the fair weather and favourable breeze which we enjoyed during nearly the whole remainder of our voyage, we were reluctantly compelled to give up the idea of prosecuting any further researches in the Deep Sea; and devoted ourselves to the examination of the specimens previously collected, and to the correlation of our temperature and other results, —specially directing our attention, however, to the surface-temperature of the *embouchure* of the Strait, with the view of ascertaining whether a sudden *fall* would be observable on quitting it, corresponding to the *rise* which had been noticed on the outward voyage on entering it. This change proved to be very decided. As we kept along the southern coast of Portugal towards Cape St. Vincent, the surface-temperature averaged 73°·5. At 6 P.M. we were turning the corner of the Cape, and found the surface-temperature 72°·5. And at 8 P.M., when we were fairly in the Atlantic, we found that the surface-temperature had fallen to 69°, thus showing a difference of 4°·5. On the following day, when we were off Lisbon, the surface-temperature was 69°·5; and it gradually diminished as we proceeded northwards from that point. Although the season of the year led us to expect a rough passage across the Bay of Biscay, the weather continued remarkably fine until we reached the "Chops of the Channel," where we fell in with rather a fresh breeze; this did not interfere, however, with our anchoring at Cowes on the afternoon of the next day (October 8th), after an absence of just two months, during which a greater number of most important public events had occurred than had ever before been crowded within so short a period.

General Oceanic Circulation.—The difference as to level and density between two bodies of sea-water, which produces the vertical circulation in the Strait of Gibraltar and the Baltic Sound, may be brought about otherwise than by the excess of evaporation which maintains it in the one case, or by the continual dilution with fresh water which maintains it in the other. It may be easily shown that a constant and decided *difference of temperature* must have exactly the same effect. Let the Mediterranean basin be supposed to be filled with water of the same density as that of the Atlantic and up to the same level; and to be then cooled down below the freezing-point of fresh water by the withdrawal of solar heat, whilst the surface of the Atlantic continues to be heated as at present by the almost tropical sunshine of the Gibraltar summer. The cooling of the Mediterranean column, reducing its bulk without any diminution of weight, would at the same time lower its level and increase its density. An in-draught of Atlantic water must take place through the Strait to restore that level; but this in-draught would augment the weight of the column, giving it an excess above that of the column at the other end of the Strait; and to restore the equilibrium a portion of its deeper water must be forced out as an under-current towards the Atlantic, thus again reducing the surface-level of the Mediterranean. Now, so long as the warm Atlantic water which comes in to maintain that level is in its turn subjected to the same cooling, with consequent lowering of level and increase of density, so long would the vertical pressures of the two columns, which would be speedily restored to equilibrium if both basins were subjected to the same heat or the same cold, remain in a constant state of inequality; and so long, therefore, must this vertical circulation continue.

Now, the case thus put hypothetically has a real existence. For the Mediterranean, cooled down by the withdrawal of solar heat, let us substitute the Polar basin, and for the Atlantic, the Equatorial Ocean. The antagonistic conditions of temperature being constantly sustained, a constant interchange between polar and equatorial waters through the seas of the Temperate Zone must be the result. The reduction in the temperature of the Polar column must diminish its height whilst augmenting its density; and thus a flow of the upper stratum of equatorial water must take place towards the poles to maintain the level thus lowered. But when the column has been thus restored to an equality of height, it will possess such an excess of weight that its downward pressure must force out a portion of its deeper water; and thus an underflow of ice-cold water will be occasioned from the polar towards the equatorial areas.

The agency of polar cold will be exerted, not merely in reducing the bulk of the water exposed to it, and thereby at the same time *lowering its level* and *increasing its density*, but also in imparting a *downward movement* to each new surface-stratum as its temperature is reduced, whereby a continual in-draught will be occasioned from the warmer surface-stratum around. For the water thus newly brought under the same same cooling influence will descend in its turn; and thus, as the lowest stratum will be continually flowing off, a constant motion from above downwards will continue to take place in the entire column, so long as a fresh stratum is continually being exposed to the influence of surface-cold.

On the other hand, the agency of equatorial heat, though directly operating on only a thin film of surface-water, will gradually pump-up (so to speak) the polar water which has reached its area by creeping along the deepest parts of the intermediate oceanic basins. For since, as already shown, an in-draught of the upper stratum surrounding the polar basin must be continually going on, the place of the water thus removed must be supplied by water drawn from a still greater distance; and thus the movement will be propagated backwards, until it affects the upper stratum of the equatorial basin itself, which will flow off pole-wards, bearing with it a large measure of heat. The cold and dense polar water, as it flows in at the bottom of the equatorial column, will not directly take the place of that which has been draughted off from the surface; but this place will be filled by the rising of the whole superincumbent column, which, being warmer, is also lighter than the cold stratum beneath. Every new arrival from the poles will take its place below that which precedes it, since its temperature will have been less affected by contact with the warmer water above it. In this way an *ascending movement* will be imparted to the whole equatorial column, and in due course every portion of it will come under the influence of the surface-heat of the sun. This heat will of course raise the level of the equatorial column, without augmenting its absolute weight; and will thus add to the tendency of its surface-stratum to flow towards the lowered level of the polar area. But as the *super*-heating extends but a short way down, and as the temperature of the water beneath, down to the "stratum of intermixture," is very moderate, whilst the water below that stratum is almost as cold as that of the polar basin, it is evidently in the latter that the force which maintains this *vertical circulation* chiefly originates.

Here, then, we have a *vera causa* for a general oceanic circulation, which being sustained only by the unequal distribution of solar heat, will be entirely independent of any peculiar distribution of land and water, provided always that this does not prevent the free communication between the polar and equatorial oceanic areas, at their depths as well as at their surface. That this agency has been so little recognised by physical geographers, we can only attribute to the prevalence of the erroneous idea of the uniform deep-water temperature of 39°, of which the temperature-observations made in our expeditions of 1868 and 1869 have shown the fallacy. Until it is clearly apprehended that *sea*-water becomes more and more dense as its temperature is reduced, and that it consequently continues to sink until it freezes, the immense motor power of polar cold cannot be apprehended. But when once this has been clearly recognised, it is seen that the application of *cold at the surface* is, in the case of sea-water, precisely equivalent as a moving force to the application of *heat at the bottom*, the motor power of which is universally admitted, —being practically utilised, in keeping up the circulation through the hot water warming apparatus now in general use.[*] The movement thus maintained would not, on the hypothesis, be a rapid one, but a gradual *creeping* flow; since the absence of limit would prevent the power which sustains it from acting as an *accelerating force*, as it would do if the equatorial and polar areas were connected only by a narrow channel, like the Atlantic Ocean and the Mediterranean Sea.

That the vertical circulation here advocated on *a priori* grounds, actually takes place in any mass of salt water of which one part is exposed to surface-cold and another to surface-heat, is capable of ready experimental proof:—Let a long narrow trough with glass sides be filled with salt water; and let heat be applied at one end (the equatorial) by means of a thick bar of metal laid along the surface, with a prolongation carried over the end of the trough into the flame of a spirit-lamp; whilst cold is applied at the other (the polar) by means of a freezing-mixture contained in a metallic box made to lie upon the surface, or (more simply) by means of a piece of ice wedged in between the sides of the trough. A circulation will immediately commence in the direction indicated by the theory; as may be readily shown by introducing some *blue* colouring liquid at the polar surface, and some *red* liquid at the equatorial surface. The blue liquid, as it is cooled, at once descends to the bottom, then travels slowly along until it reaches the equatorial end of the trough, then gradually rises towards the heated bar, and thence creeps along the surface back to the polar end. The red liquid first creeps along the surface towards the polar end; and then travels through exactly the same course as the blue had previously done.[†]

That such a vertical circulation really takes place in oceanic water, and that its influence in moderating the excessive cold of the polar areas and the excessive heat of the equatorial region is far more important than that of any surface-currents, seems to us a legitimate deduction from the facts stated in the Report of the "Porcupine" Expedition for 1869. For, on the one hand, it was shown that there is a general diffusion of an almost glacial temperature on the bottom of the deep ocean-basins, at depths exceeding 1000 fathoms, occupied by polar water, more or less diluted by admixture according to the length of the course it has had to travel; whilst between this stratum and that other stratum of warmer water which (on the hypothesis) is slowly moving pole-wards, there is a "stratum of intermixture," in which there is such a rapid change of temperature as might be expected from the relation of the upper and lower masses of water. This "stratum of intermixture" showed itself in a most marked manner in the Atlantic temperature-observations of the present expedition; the descent of the thermometer, which had been very slow with increase of depth between 100 and 800 fathoms, becoming suddenly augmented in rate; so that between 800 and 1000 fathoms it fell nine degrees, namely, from 49°·3 to 40°·3.

On the other hand, it was shown in the previous report that there is evidence of the slow pole-ward movement of a great upper stratum of oceanic water, carrying with it a warm temperature; which movement cannot be attributed to any such local influences as those which produce the Gulf-stream or any other currents put in motion by *surface*-action. Of such a movement, it was contended, we have a marked example in that north-easterly flow which conveys the warmth of southern latitudes to the west of Ireland and Scotland, the Orkney, Shetland, and Faroe islands, Iceland, Spitzbergen, and the polar basin generally. This flow, of whose existence conclusive evidence is derived from observations of the temperature of these regions, is commonly regarded as a prolongation of the Gulf-stream; and this view is maintained not only by Dr. Petermann,[*] who has recently collected and digested these observations with the greatest care, but also by Prof. Wyville Thomson,[†] as well as by Mr. Croll.[‡] Having elsewhere fully stated our objections to this doctrine, and discussed the validity of the arguments adduced in support of it, [§] we shall here only record the conclusions which a careful examination of the present state of our knowledge of the subject has led us to form:—

I. That there is no evidence, either from the surface-temperature of the sea, or from the temperature of sea-board stations along the western coast of Southern Europe, that the climate of that region is ameliorated by a flow of ocean-water having a temperature higher than that of the latitude: the surface-temperature of the Mediterranean Sea, which is virtually excluded from all oceanic circulation, being higher than that of the eastern margin of the Atlantic in corresponding latitudes; and the climate of sea-board stations on the Mediterranean being warmer than that of stations corresponding to them in latitude on the Atlantic coast —and this not merely in summer, but also in winter. This oceanic region may therefore be designated the *neutral area*.

II. That the evidence of climatic amelioration increases in proportion as we pass northwards from the *neutral area*; becoming very decided at the Orkney, Shetland, and Faroe islands. But that, as was shown by the *Porcupine* temperature-soundings of 1869, the flow of warm water which produces this amelioration extends to a depth of at least 700 fathoms.

III. That this deep stratum of water can be shown, by the correspondence in the rate of its diminution of temperature with depth, to be derived from the neutral area to the south-west; where, as is shown by the *Porcupine* temperature-soundings of 1870, it is separated by a distinct "stratum of intermixture" from the deeper stratum that carries polar waters towards the equator.

IV. That the slow north-easterly movement of such a mass of water cannot, on any known hydrodynamical principles, be attributed to propulsive power derived from the Gulf-stream; the last distinctly traced edge of which is reduced to a stratum certainly *not exceeding* 50 fathoms in depth, and not improbably less.

V. That on the other hand, this slow pole-ward movement of the upper layer of the North Atlantic, down to the "stratum of intermixture," is exactly what might be expected to take place as the complement of the flow of glacial water from the polar to the equatorial area; the two movements constituting a general *vertical* oceanic circulation.

VI. That there is a strong probability that the quantity of water discharged by the Gulf-stream has been greatly over-estimated, in consequence of the rate of the surface-current having been assumed as the rate of movement through the whole sectional area, which is contrary to all analogy; whilst there is also a strong probability that there is a *reverse* undercurrent of *cold* water through the narrows, derived from the polar current, that is distinctly traceable nearly to its mouth. The upper stratum of this southerly current comes to the surface between the Gulf-stream and the coast of the United States; whilst its deeper and colder stratum underlies the Gulf-stream itself.[||]

VII. That there is a strong probability that the quantity of heat carried off by the water of the Gulf-stream has been greatly over-estimated; the temperature-soundings taken during the cruise of the *Porcupine* in the Mediterranean having shown that the very high temperature of the surface extends but a little way down; whilst the temperature observations in the Atlantic show that the descent into a cold stratum beneath may be very rapid. Hence the average of 65° assumed by Mr. Croll on the basis of observations made at considerable intervals of depth, is altogether unreliable.

VIII. That the most recent and trustworthy observations indicate that the edge of the Gulf-stream to the north-east of the banks of Newfoundland is so thinned out and broken up by interdigitation with polar currents, that its existence *as a con-*

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[*] The only scientific writer who has even approached what appears to us the truth on this point, is Captain Maury, who has put forward the doctrine of a general interchange between the equator and the poles, resulting from a difference of specific gravity caused *inter alia* by difference of temperature. But as Mr. Croll remarks, "although Captain Maury has expounded his views on the cause of ocean currents at great length in the various editions of his work, yet it is somewhat difficult to discover what they really are. This arises from the general confused and sometimes contradictory nature of his hydrodynamical conceptions." See Mr. Croll's Paper "On the Physical Cause of Ocean Currents," in the "Philosophical Magazine" for October, 1870

[†] This experiment has been exhibited, by the kindness of Prof. Odling, at the Royal Institution, and at the Royal Geographical Society.

[*] Geographische Mittheilungen, 1870, p. 201

[†] Lecture "On Deep-sea Climates," in NATURE, July 28, 1870.

[‡] Memoir "On the Physical Cause of Ocean Currents," in "Phil. Mag., Oct 1870

[§] Proceedings of the Royal Geographical Society, for Jan. 9, 1871.

[||] That there is a slow southerly movement of Arctic water, beneath the Gulf-stream, is indicated by the fact that icebergs have been seen moving southwards in direct opposition to its surface-flow; their deeply-immersed portions presenting a larger surface to the lower stratum than their upper part does to the more superficial layer, as in the case of our "current-drag" And similar evidence is afforded by the southward drift of the buoy which was attached to the Atlantic Cable of 1865, but which broke away from it, apparently carrying with it a great length of the wire rope by which it had been attached.

tinuous current beyond that region cannot be proved by observations, either of temperature or movement.

IX. That the Gulf-stream and other local currents put in motion by the trade-winds or other influences acting on the *surface* only, will have as *their* complement in a *horizontal* circulation return *surface* currents; and that the horizontal circulation of which the Atlantic Equatorial Current and the Gulf-stream constitute the first part is completed—so far as the Northern Hemisphere is concerned—partly by the direct return of one large section of the Gulf-stream into the Equatorial Current, and as to the other section, by the *superficial* polar currents which make their way southwards, the principal of them even reaching the commencement of the Gulf-stream.

In conclusion it may be added that the doctrine of a general vertical oceanic circulation is in remarkable accordance with the fact now placed beyond doubt by the concurrent evidence of a great number of observations, that whilst the density of oceanic water, which is lowest in the Polar area, progressively increases as we approach the Tropics, it again shows a decided reduction in the intertropical area. It has been thought that an explanation of this fact is to be found in the large amount of rainfall and of inflow of fresh water from great rivers in the intertropical region; but it is to be remembered that the surface evaporation also is there the most excessive, so that some more satisfactory account of the fact seems requisite. Such an explanation is afforded by the doctrine here advocated; the Polar water which flows towards the Equator along the bottom of the ocean basins, being there pumped up and brought to the surface.* And it is not a little confirmatory of the views advanced in this Report that in a recent elaborate discussion of the facts relating to the comparative density of oceanic water on different parts of the earth's surface, the doctrine of a general vertical circulation is advocated as affording the only feasible rationale of them.†

SCIENTIFIC SERIALS

THE *Zeitschrift fur Ethnologie* (1870 Heft III. and IV contains the following notices:—Orton's "Andes and the Amazon."—Waring's "Stone Monuments, Tumuli, &c.," "Manuscrit Troano," giving an account of the MS. in question, which is written in the Maya language; the reviewer calls this "surely the wildest production that ever saw the light with the sanction of an Imperial Government," though he admits that still wilder productions are published in his own country, now also under "Imperial government."—Benfey's "Gesch. de Sprachwissenschaft" is highly praised.—Burgen's "Temples of Satrunjaya," with forty-five photographs.—Hamy's "Paléontologie Humaine."

THE last part of the "Neues Jahrbuch für Mineralogie, Geologie," &c., published 1871, contains the following papers:—R. D. M. Verbeek on the Nummulites of the Borneo Rocks, with three plates illustrating new species, &c., one species, *N. Biaritzensis*, is also found in these beds, and extends through all the nummulitic formation from the Pyrenees to Borneo. He believes that this formation extends to Java and most of the islands of the East Indian Archipelago, but hitherto this formation has not been recognised.‡—Dr. R. Lincke on the Buntersandstein in Thuringen, which is the commencement of an elaborate monograph on these beds.—Dr. Alfred Stelzner on Quartz and Allied Minerals.—Adolph Pichler, Additions to the Mineralogy of the Tyrol; and, by the same author, Additions to the Palæontology of the Tyrol, and the usual mineralogical, geological, and palæontological notices.

OF the Transactions of the Natural History Society of Rhenish Prussia and Westphalia, including also the reports of the Society of Natural History and Medicine of the Lower Rhine, we have received the twenty-sixth volume, containing an account of the doings of the respective societies in the year 1869. The papers published by the first-mentioned society are well known to naturalists, and often of very great value. In the present volume we find the following:—"Contributions to the Rhenish Flora," by Dr. P. Wirtzen, including a discussion of the species of dog-roses, with the description of a so-called new species, *Rosa exilis*, a notice of *Asplenium Heufleri*, the description of a new plantain from Saarbruck, *Plantago Winteri*, a notice of the various forms of *Rubus tomentosus*, and of anomalies in other species of *Rubus*, and notices on the geographical distribution of certain plants; also, by the same author, a supplement to his manual of "The Flora of Rhenish Prussia; a paper "On the Height of the Water of the Rhine at Cologne from 1811-1867," by M. H. von Dechen; the continuation of Kaltenbach's valuable memoir on the German Phytophagous insects, in which the species feeding upon each species of plant are noticed, the plants being arranged in the alphabetical order of their botanical names, now reaching to the end of the letter S.; a contribution to the knowledge of the cryptogamous flora of the Saar district, by M. F. Winter, containing notices of Equisetaceæ, Lycopodiaceæ, and Ferns; and a paper, illustrated with three plates, "On the Fossil Echinodermata of North Germany," by Dr. C. Schluter. In the last-mentioned paper, the author notices the described species of Jurassic and Cretaceous Echinoderms found in North Germany, and describes several new forms. The reports of the second society mentioned, which holds its meetings in Bonn, include an immense number of short notices of communications on almost all branches of science, but especially on Natural History and Chemistry; many of them are of considerable interest.

IN the March number of the *Journal of Anthropology* there is a careful anatomical description of the body of a negro by Dr. Kopernicki. Detailed measurements are added, together with the weights of the principal organs, and the diameter of more than twenty of the nerves. A remarkable feature in the case was the state of atrophy in which the supra-renal bodies were found; and if, in the absence of other fatal lesions, this may be assumed as the cause of death, there is here recorded a case of Addison's disease occurring in a negro. In the same journal is a translation of a review by Rutimeyer of Prof. Bischoff's work on the skulls of the anthropoid apes, in which both the text and the atlas of plates which accompanies it are severely criticised. Both the original pamphlet and the review have, however, lost much of the interest they possessed at the times of the publication, 1864 and 1868 respectively.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 30.—"Contributions to the History of Orcin.—No. 1. Nitro-substitution Compounds of the Orcins." By John Stenhouse, LL.D., F.R.S. The action of nitric acid upon orcin has been studied by several chemists, but with comparatively negative results Schunck in this manner obtained a red resinous substance, which, by further treatment with the acid, was oxidised with oxalic acid; and in 1864 De Luynes found that orcin dissolved in cooled fuming nitric acid without evolution of nitrous fumes, and that the addition of water precipitated a red colouring matter; the long-continued action of the vapour of fuming nitric acid on powdered orcin likewise produced a red dye apparently identical with the above. These, however, are resinous uncrystallisable substances. Although under ordinary circumstances only resinous products are obtained by treating orcin with nitric acid, yet, when colourless orcin in fine powder is gradually added to strong nitric acid, cooled by a freezing mixture, it dissolves with a pale brown coloration, but without the slightest evolution of nitrous fumes. If this solution be now slowly dropped into concentrated sulphuric acid, cooled to −10° C., the mixture becomes yellow and pasty, from the formation of nitro-orcin, which is but slightly soluble in sulphuric acid. When this is poured into a considerable quantity of cold water, the nitro-body separates as a bright yellow crystalline powder, quite free from any admixture of resin. The crude nitro-orcin was collected, washed with a little cold water, and purified by one or two crystallisations from boiling water (40 parts). It was thus obtained in large yellow needles, which are readily soluble in hot water and but slightly in the cold; the addition of a strong acid precipitates almost the whole of the nitro-orcin from its cold aqueous solution. It is soluble in alcohol, very soluble in hot benzol, and crystallises out in great part on cooling; it is less soluble in ether, and but moderately so in bisulphide of carbon. It dyes the skin yellow, like picric acid, but is tasteless. It volatilises slightly at 100° C., melts at 162° C., and decomposes with slight explosion immediately afterwards. When heated with concentrated sulphuric acid it dissolves, forming a deep yellow solution, which deposits crystals on cooling, and is immediately precipitated by water. It dissolves in hot strong nitric acid with evolution of nitrous fumes and formation of oxalic acid. Like picric acid, when treated with calcium hypochlorite it yields chloropicrin at the ordinary temperature. Its aqueous solutions are coloured dark brown by ferric chloride, and completely precipitated by lead subacetate. The analysis of the substance dried at 100° C. was made with the following results derived from three experiments:—

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* That water of a *lower* should thus underlie water of a *higher* degree of salinity in travelling from the Pole to the Equator, is not difficult to account for, when the relative temperatures of the two strata are borne in mind

† Densité, Salière, et Courants de l'Océan Atlantique, par Lieut. B. Savy; Annales Hydrographiques, 1868, p. 620

‡ It is not out of place to mention here that Baron Reichtofen has quite recently found this nummulitic formation in China, it is described in *Silliman's Journal* for February 1871. It has been found also in Japan.

	Theory	I.	III.	II	Mean.
C_7 =	84 = 32·43	32·58	32·63	32·68	32·63
H_5 =	5 = 1·93	2·06	2·18	2·03	2·09
N_3 =	42 = 16·22	..	..	..	..
O_8 =	128 = 49·42	..	..	..	..
	259 100·00				

These results correspond to the formula $C_7H_5(NO_2)_3O_2$ that of trinitro-orcin. It is a powerful acid, much resembling picric acid, but distinguished from the latter by the greater solubility of its salts. I propose, therefore, to call this new substance *trinitro-orcinic acid*. The preparation and composition of a large number of compounds of the acid were then detailed.

Geological Society, March 22.—Prof. John Morris, Vice-President, in the chair. Messrs. A. R. Selwyn, Director of the Geological Survey of Canada; J. Bridges Lee, the Rev. Thomas Robert Willacy, B.A., and James Putnam Kimball, Ph.D, New York, were elected Fellows of the Society. The following communications were read: 1. "On the Passage Beds in the neighbourhood of Woolhope, Herefordshire, and on the discovery of a new species of *Eurypterus*, and some new Land-plants in them." By the Rev. P. B. Brodie, M.A., F.G.S. The author described as the "passage-beds" between the Silurian and Old Red Sandstone formations near Woolhope, a series of shales and sandstones, which at Perton attain a thickness of about 17 feet. Here the section includes, in descending order:—(1) Thin-bedded sandstones; (2) Dark brownish shales; (3) Yellow sandstone; (4) Olive shales; (5) Thin bedded-sandstone; (6) Olive shales, similar to No. 4. At some localities vegetable remains (*Lycopodites*, and perhaps *Psilophyton*) occur in the olive shales, which also contain several Crustacean fossils, including *Pterygotus Banksii* and a new species of *Eurypterus*, named by Mr. Woodward *E. Brodiei*. Upon this species, Mr. Woodward presented a note supplementary to Mr. Brodie's paper. Mr. Duncan inquired whether any metamorphoses had been recognised among the Eurypteridæ, and, if so, whether the variation in the thoracic plates mentioned by Mr. Woodward might be connected with them. Mr. Woodward, in reply, remarked on the difficulty of distinguishing even the sexes in Eurypteridæ. The thoracic plate in the fossils resembled that of *Limulus*, and the variety might be connected with sex. In some *Slimoniæ* from Lesmahago the only difference to be found was in the thoracic plate, and it had been suggested that this was due to difference of sex. He had already suggested that the small *Pterygotus* and the great *Slimonia* might be only the male and female forms of the same species. On fragmentary remains it was, however, unsafe to attempt to base species; but he thought *Eurypterus Brodiei* was a well-marked species. Rev. E. Winwood inquired whether there was any evidence as to *Euypterus* being freshwater or marine. The chairman observed that the seeds from the passage-beds did not appear to him other than those of land-plants, and had been previously described by Dr. Hooker as spore-cases of Lycopodiaceæ.—2. On the Cliff-sections of the Tertiary Beds west of Dieppe in Normandy and at Newhaven in Sussex." By Mr. William Whitaker, F.G.S. The author gave details of the sections of the Tertiary beds at the above places, and noticed the occurrence of London clay. Below this formation at Dieppe is a mass of sand, the same as that of the "Oldhaven beds" in East Kent, but here less markedly divided from the clay above; and beneath this sand come the estuarine shelly clays, &c., of the Woolwich beds. In the older accounts of the Newhaven section a much less thickness of the Tertiary beds is chronicled than may now be seen; indeed the successive descriptions end upwards with higher and higher beds, owing to the destruction of the coast and the wearing back of the cliff into higher ground, the highest point seeming to have been at last reached. Here the Oldhaven sand is absent, but the Woolwich clays are in greater force; and the ditch of the new fort shows some very irregular masses of gravel more or less wedged into those clays. Both sections show the comparatively wide extent of like conditions to those of the Woolwich beds, of West Kent. The Chairman, in inviting discussion, called attention to the existence of Tertiary beds of similar character near Epernay and Rheims, and in other parts of France. Mr. Evans, remarked on the bearing which this extension of soft, yielding strata had on the excavation of the Channel. The disturbances in the sands and clays might be due to the springs having formerly, owing to the distance of the sea and the river-valley not having been excavated, stood at a higher level; and having thus softened or even washed away, the bed beneath the gravels. Mr. Pattison mentioned that in all the combes along the French coast towards Tréport there were traces of soft Tertiary beds, possibly Thanet sands. Mr. Whitaker, in a reply to a question from the Chairman, stated that, to the best of his belief, the sandstones at Dieppe were not calciferous. The sands were above the Woolwich beds, and therefore not Thanet sands.—3. "On New Tree Ferns and other Fossils from the Devonian." By Prof. J. W. Dawson, L.L.D., F.R.S., F.G.S. The author referred to the numerous species of ferns known in the Upper and Middle Devonian of America, and to the fact that he had described several large petioles as probably belonging to arborescent species, and also two trunks covered with aerial roots, viz. *Psaronius erianus* and *P. textilis*. He also referred to *Caulopteris Peachii* of Salter as the only tree-fern known in the Devonian of Europe. He then described remains of four species of tree-ferns in collections communicated to him by Dr. Newberry of New York. The first of these, *Caulopteris Lockwoodi*, was found by the Rev. Mr. Lockwood at Gilboa, the locality of the Psaronites already mentioned, in rocks of the Chemung group. It is a fragment of a well-characterised stem, with parts of five petioles attached to it, and associated with remains of the leaves. It must have been entombed in an erect position, and is not improbably the upper part of one of the species of *Psanonius* from the same locality. The second species, *Caulopteris antiqua*, Newberry, is of much larger size, but less perfectly preserved. It is a flattened stem on a slab of marine limestone from the Corniferous formation in the lower part of the Middle Devonian (Erian) of Ohio. The third species. *Protopteris peregrina*, Newberry, is from the same formation with the last, and constitutes the first instance of the occurrence of the genus to which it belongs, below the Carboniferous. The specimens show the form and arrangement of the leaf-scars, the microscopic structure of the petioles, and also the arrangement of the aerial roots covering the lower part of the stem. The fourth species is a gigantic *Rhachiopteris*, or leaf-stalk, evidently belonging to a species quite distinct from either of the above and showing its minute structure. It is no less than four inches wide at the base. In the cellular tissue of this petiole are rounded grains similar to those regarded by Corda and Carruthers, in Carboniferous and Eocene specimens, as starch-granules. In addition to these species, the paper described a new *Næggerathia* (*N. gilboensis*), and noticed a remarkable specimen from Caithness, in the collection of Prof. Wyville Thomson, throwing light on the problematical *Lycopodites Vanuxemii* of America; also interesting specimens of *Psilophyton* and other genera seen by the writer in the collection of Mr. Peach of Edinburgh. Dr. Duncan doubted the desirability of basing generic and specific terms on imperfectly preserved and indistinct specimens, and pointed out the disagreements among botanists that had resulted from so doing. He would prefer calling fossils such as those described "cryptogamous forms from certain strata." He was doubtful also whether the supposed petrified starch was not merely orbicular silex. The chairman remarked on the four different conditions exhibited by existing tree ferns, first, with roots running down the stem: secondly, the lower portion with oval scars; these are, thirdly, farther up the stem, rhomboidal vertically; and, fourthly, higher up still, rhomboidal horizontally; so that were the plant fossil, distinct genera and species might be founded upon the different parts.

MANCHESTER

Literary and Philosophical Society, March 21.—Mr. E. W. Binney, president, in the chair. Dr. John Hopkinson was elected an ordinary member of the society. "On the Mechanical Equivalence of Heat," by the Rev. H. Highton. The following is an abstract of the arguments as given in the paper and brought out in the subsequent discussion:—1. The author apologised for having mentioned other names in connection with great discoveries which were undoubtedly due primarily to Dr. Joule, and spoke of the very great value of Dr.

Joule's experiments, even when he did not agree with the deductions drawn from them. 2. The subject is of extreme importance both for the interpretation of physical phenomena and or determining what limits are assigned by the stern laws of nature to the exercise of man's mechanical and scientific skill. 3. No doubt Dr. Joule has ascertained the heat ordinarily derived from the destruction of energy, by means of friction with various substances; but it has been assumed, *in defiance of facts*, that the numerical relations which connect heat and energy in the case of friction hold good when energy and heat produce or destroy each other by any other means. 4. In the case of friction itself, energy is not transformed simply into heat, but partly into heat and partly into another kind of energy, which is involved in the expansion of the solids or liquids acted on. 5. No doubt the coincidence between the mechanical equivalent of heat, found by Dr. Joule from friction, and that by M. Favre from working a magnetic engine, seems very striking; but (1) the value of Favre's experiment disappears on examination. It was but a single experiment, either never repeated, or never repeated with the same results; in a very delicate experiment there was only the difference of 300 units out of 18,000; and even the permanent enlargement which always takes place in magnets which are in use might account for this; and (2) numerous and long-continued experiments by M. Soret show results entirely discordant with the single one of M. Favre. 6. It seems incredible, that with the imperfectly constructed engine used by Joule and Scoresby, they should at the very first trial have succeeded in utilising two-thirds of the magnetism evolved, or capable of being evolved, by their battery; and Dr. Joule now tells us that according to his latest calculations of the mechanical equivalence of heat they utilised six-sevenths of the power of the battery. The only conclusion we can arrive at is, that the real power of the battery, and therefore of a grain of zinc, must have been much greater than he calculated. 7. For consider the disadvantages under which the engine acted: (1) the temporary and permanent magnets were never nearer than ¼ of an inch apart. Though Dr. Joule assures us this does not affect the *power* of the engine, it certainly produces a waste of zinc, as the near approach of the magnets creates counter-currents which check materially the consumption of zinc. (2) The copper wire was not tested for conductivity; a subject little thought of at that time, and it is found that a very small impurity in copper wire will very, very largely diminish the power of an electro-magnet. (3) The iron was not tested for specific capacity for magnetism, yet this is a most important point which is even now but little appreciated. It is found practically that, if two electro-magnets be made from the very same piece of iron, most carefully prepared, with the very same length of the same wire, without the slightest assignable cause, one will sometimes have three times the power of the other. Hence I conclude that the maximum energy capable of being evolved by a grain of zinc must be very much greater than that assigned to it by Dr. Joule. 7. Dr. Hopkinson's argument, in his paper lately read to this society, virtually amounted to this—that a well-constructed magnetic engine will get no more duty from a grain of zinc than an ill-constructed one; and consequently, I presume, that magnets might be weakened to any extent, and removed to ever so great a distance from one another, without necessarily affecting the efficiency of the engine. 8. Dr. Hopkinson has in his criticism strangely substituted $(a-b)$ for (b). In Joule and Scoresby's paper, the consumption of zinc is expressed not by $(a-b)$ but by (b); and consequently the duty of a grain of zinc not by $\frac{W}{a-b}$ but by $\frac{W}{b}$; and when the magnets are stronger and approach nearer to each other, even if W be not increased, (b) is diminished. 9. My argument was this, that since the accepted theory of the mechanical equivalence of heat is *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold, not heat, then the doctrine of the mechanical equivalence of heat cannot be true; we might with equal justice call it a mechanical equivalence of cold. It is no reply to say that such facts are simple deductions from the laws of thermodynamics. This would only show that the laws of thermodynamics are inconsistent with the doctrine of the mechanical equivalence of heat. 10. The argument from the fire syringe I withdraw, as inconclusive. But I think my case was sufficiently established without it. 11. Joule and Scoresby in their paper incorrectly assume that, if the quantities of electricity in the current at different times be represented by (a) and (b), the heat varies as a^2 to b^2. This is only true where the resistance is the same. In the case before us the working of the engine introduces a fresh element in resistance. 12. Again by assuming that $(a-b)$ represents diminution of quantity of the current, *and* the diminution in the zinc consumed, *and* the heat converted into useful work, they involve the supposition either that less zinc produced equal heat, or that heat was changed into useful work which was never produced at all, and therefore could not be absorbed. In fact, there was no *proof* that any heat was absorbed at all. 13. It is said that in electro-plating, electro-magnetic engines, worked by steam, are found more economical than batteries. This is in cases where a battery of many cells would be required; which is always wasteful, as a large number of equivalents of zinc must be consumed to deposit one equivalent of silver or other metal. 14. Besides, there is a far greater advantage in changing work into electricity, than electricity into work. In the former case all, or nearly all, the work is effective; in the latter, a very small portion of the electricity has hitherto been utilised.

—Dr. Hopkinson said that most of Mr. Highton's objections to the mechanical equivalent of heat appear to arise from a mistake as to what is meant by the term. The nature of this mistake may be best seen in the case of a perfect heat engine, of which t_1 and t_0 are the absolute temperatures of the source and refrigerator. Then from every unit of heat leaving the source we obtain $\frac{t_1-t_0}{t_1}$ J units of work. Now this a quantity variable with t_1 and t_0; it would be similar to most of Mr. Highton's arguments to infer that from a given quantity of heat a variable quantity of work could be obtained. But, of course, the case really is that of the unit of heat leaving the source, $\frac{t_0}{t_1}$ is lost in the refrigerator, whilst $\frac{t_1-t_0}{t_1}$ disappears *as heat* and is converted into the work done, and the principle of the equivalence of heat and work asserts that J is constant. It will be seen that this is the mistake Mr. Highton makes in his paper in the *Journal of Science* (end of article 6). He seems there to imagine it stated, that the work done is equivalent to the whole heat thrown into the gas, and he fails to perceive that a certain portion is used to raise the temperature of the air or turpentine. This will make my criticism of his paper in the *Chemical News* clearer. Mr. Highton argued against the mechanical equivalent, and what I pointed out was, that the chemical energy, which was converted into mechanical effect and not used to heat the wire, was proportional to $a-b$, that therefore, in order to prove that there was no mechanical equivalent Mr. Highton must show $\frac{W}{a-b}$ is variable. I do not assert that a badly constructed engine will get as much heat from fuel as a good one, but merely that the work done and the heat, which has disappeared as heat and been converted into work, are in a constant ratio. Now as regards Mr. Highton's argument from the case of elastic wires—that the wire will be cooled when stretched follows from the two laws of thermodynamics, a proof may be seen in Tait's Thermodynamics, p. 105. Mr. Highton replies, "Quite true; but this only shows that one of the laws of thermodynamics is inconsistent with the doctrine of the mechanical equivalence of heat." Now the first law of thermodynamics asserts nothing else than that there is a mechanical equivalent, constant in all cases; whilst the second law, as usually stated, involves the first law, and involves nothing else but Carnot's axiom and the principle that in conduction heat flows from the hot to the cold body, both of which no one will doubt. Mr. Highton's reply is very similar to stating that one of Kepler's laws is inconsistent with the planets moving in ellipses. What Mr. Highton proposes as a paradox is then a necessary consequence of the principle he attacks. Though the doctrine of the mechanical equivalent of heat finds its firmest basis in the immortal experiments of Dr. Joule, the fact, that assuming it we can explain many phenomena, is a valuable supplementary proof.

EDINBURGH

Royal Physical Society, February 22.—Dr. Robert Brown, President, in the chair. "On the Glacial Epoch," by the Rev. P. A. Brodie. The author of this paper proposed three questions—(1) Is he correct in supposing that the popularly received opinion with respect to the glacial epoch regards it as a period of comparatively limited duration, intermediate between the Tertiary and Quaternary eras, when a freezing climate prevailed contemporaneously over a great part of the globe? (2) Has he stated with sufficient distinctness the facts adduced in support of that opinion? (3) Do the arguments which he has brought forward prove the opinion to be erroneous?—"Notes on the Sea Otter (*Enhydra marina*, Flem.)," by Pym Nevins Compton.—"On the Tailless Trout of Islay," by Colin Hay and Peter M'Kenzie. Mr. Peach, before reading the communication he had been entrusted with, wished to say that a gentleman whom he met at the house of a mutual friend, mentioned the Tailless Trout of Islay, and as this excited Mr. Peach's curiosity, the gentleman procured from Messrs. Hay and M'Kenzie the bounteous supply now laid before you. The communication was kindly made in reply to questions put to them by Mr. Peach:—"The locality of the loch is about 1,000 feet above the level of the sea, and is on the estate of Mr. Finley of Elcnlossit, Islay, and at its highest water is not more than an acre in extent. It is so shallow that a man could wade all through it; the bottom loose stone quartz, same as the surrounding mountains, and we think it is the most elevated piece of water on the island in which trout exist. It is named Lochna Maorichean, meaning that a species of fresh water 'limpet' or 'whelk,' is found on its shores, but we can say nothing about these, all we have gone there for was to capture some of its strange denizens; as to its other productions, there are small tracts of weeds here and there lying on the surface of the water, with soft pulpy stems; of the parasites of the fish or in the water we cannot speak.—Dr. J. A. Smith exhibited a specimen of the *Cottus Grœnlandicus* (the Greenland Bull-head), recently taken at the Firth of Forth.—Mr. David Grieve exhibited a photograph of the Queensland Cicada, or as it is popularly termed, Locust.

MONTREAL

Natural History Society, Feb. 27.—The President, Principal Dawson, F.R.S., in the chair.—The President exhibited illustrations of new facts in Fossil Botany. The following is an abstract of his remarks:—"The first point mentioned was the occurrence in the Devonian Shales of Kettle Point, Lake Huron, of beds containing immense quantities of spore-cases, probably of *Lepidodendron*. These beds are referred by the Geological Survey to the horizon of the Genessee shales of New York, and are stated to be twelve or fourteen feet in thickness, and to extend over a considerable area of country. Specimens in the collection of the Survey show that the bituminous matter which causes the combustible quality of the shale, is due entirely to the immense quantities of spore-cases present, which, under the microscope, appear as flattened discs scarcely more than one hundredth of an inch in diameter. Specimens of the trunks of *Lepidodendron Veltheimainum* and *Calamites inornatus* occur in the same beds. This is probably the oldest bed of fossil spore-cases known; but in later geological periods similar beds occur, the Tasmanite, or 'white coal' of Tasmania, which consists of spore-cases of ferns, being a notable instance. The author next referred to the discovery of specimens indicating the existence of three or four species of Tree-Ferns in the Devonian of New York and Ohio. He had received from Prof. Newberry of New York a specimen, showing the upper part of a stem with five leaf stalks attached to it. This he had named *Caulopteris Lockwoodi*. Three other specimens collected by Prof. Newberry in Ohio indicated the existence of three distinct species belonging to two genera. The two most important had been named by Prof. Newberry *Caulopteris antiqua* and *Protopteris peregrina*. They are from the Coniferous Limestone, and thus carry down tree-ferns to the bottom of the middle Devonian. One of them has the cellular structure and vascular bundles in such preservation as to show their microscopic structure, which is precisely similar to that of modern ferns."—Mr. A. R. C. Selwyn, Director of the Geological Survey of Canada, read a paper "On the Occurrence of Diamonds in New South Wales," by Mr. Norman Taylor, late of the Geological Survey of Victoria, and Professor Thompson, of the University of Sydney.

PARIS

Academy of Sciences, March 27.—The hall was pretty well filled, and the correspondence was rather heavy. Letters from the provinces and from foreign parts were numerous, as the insurgents had not taken possession of the Post Office, and communications were not stopped between Paris and the outer world. M. Faye presided over the sitting, which was as orderly as in former times. No trace of public emotion was to be seen in the hall where the scientific assembly meet. Numerous details were given of the meteor which was seen on the 17th inst. in southern France, and left behind an immense luminous track. These details were very welcome, as during the investment certain bold theorists maintained that falling stars, bolides, and meteorites, were produced by the same causes. New facts having been brought forward at the last sitting to show that atmospheric changes are produced in high altitudes and gradually manifest themselves in the vicinity of the air, M. Wilfred de Fonvielle sent a communication upon the truth and genuineness of this observation. He quoted letters he had received from M. Buys Ballot, the learned director of the observatory at Utrecht, when he was waiting for a favourable wind in order to return to Paris by an aërial expedition during the investment. And he concluded by showing that the best way for ascertaining the state of things at a high level was to try scientific ascents. The Academy appeared to be much pleased with the idea, but it is impossible for it to recommend the application of the scheme as long as order is not established in Paris. M. Delaunay and M. Sainte-Claire Deville disputed as to the meaning of the thermometric measures which had been taken during the investment of Paris at the observatory at Montsouris and in the Jardin des Plantes. The distance of the two stations is something less than a mile, and the difference in altitude is about thirty feet. This circumstance may account for the difference in the two sets of observation.

DIARY

THURSDAY, APRIL 6.

LINNEAN SOCIETY, at 8.—On the stigmas of *Proteaceæ*: G. Bentham, Pres. L. S.—On the generic nomenclature of *Lepidoptera*: G. R Crotch

CHEMICAL SOCIETY, at 8.—On Burnt Iron and Burnt Steel: W. Mattieu Williams.—On the formation of Sulpho Acids: Henry E. Armstrong.

SATURDAY, APRIL 8.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.

TUESDAY, APRIL 11.

PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, APRIL 12.

SOCIETY OF ARTS, at 8.—On Boiled Oil and Varnishes: C. W. Vincent.

THURSDAY, APRIL 13.

MATHEMATICAL SOCIETY, at 8.

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NOTICE

We beg leave to state that we decline to return rejected communications; and to this rule we can make no exception.

THURSDAY, APRIL 13, 1871

THE PROPOSED SCHOOL OF PHYSICAL SCIENCE AT NEWCASTLE-UPON-TYNE

THE proposal to found a College in Newcastle for the teaching of Physical Science needs from us more than a mere passing mention. The idea of such an Institution is not a new one, indeed it is only justice to the coal-owners, chemical manufacturers, and engineers of that busy centre of commercial enterprise, to say that they have long confessed to a sort of shame that youths, intended for responsible positions connected with the various industries on the banks of the Tyne, Wear, and Tees, have had to seek their scientific training in metropolitan or continental schools; whilst young men a shade lower in the social scale, with no disposition to undervalue such knowledge, have had to depend upon their own limited resources for its partial acquirement, with the alternative of remaining in entire ignorance. The feeling of soreness on this point had attained sufficient force fifteen years ago to induce the North of England Institute of Mining Engineers to take preliminary steps towards establishing a college for the teaching of practical science. Meetings were held, architects consulted, plans drawn, and eventually those interested in the project were informed that about 35,000*l.* would be wanted from them if they were really in earnest. The Duke of Northumberland (Duke Algernon) was applied to, and his response was characteristic; it amounted practically to this:—"Raise 30,000*l.* and I will make it 40,000*l.*, but while you are about it, do the thing well." One would have thought that such an offer to so wealthy a body was equivalent to an order for the buildings to be advanced without delay. Unfortunately interests began to clash, individual promoters had pet ideas which they were unwilling to sacrifice, and there was no agreement as to the way the scheme should be carried out. The University of Durham, it was admitted on all sides, must have some share in the work, and no one could suggest a basis of connection that was mutually satisfactory, so by the end of the year 1855 the North of England had heard the last of the proposed "British College of Practical Mining and Manufacturing Science." It is perhaps no wonder if younger men, looking back at Duke Algernon's munificent offer, have accustomed themselves to think, "we would not have done as our fathers did."

One good result, however, had accrued from the failure. The necessity for scientific and technical education had been admitted, and public attention had been directed to the entire absence of local facilities for systematic teaching in those branches of theoretical knowledge most concerned in the industries of the North.

For a time the idea of an independent establishment in Newcastle seemed to be abandoned, and people were disposed to look to Durham University for a solution of the difficulty. The position of Durham was discussed, the revenue of its University talked of, and the relation of its expenditure to its educational results seriously debated—the outcome of all which was the appointment of a Commission to inquire into the administration of its various endowments. At this time the scientific instruction of the University amounted to twelve lectures annually from a Reader in Chemistry, some teaching from a Reader in Natural Philosophy, and a very few lectures from a Reader in Medicine. After the sittings of the Commission, the Senate took powers to establish a school of physical science in the faculty of Arts and to endow chairs therein, but as the stipends for these chairs were not forthcoming, their powers were never practically exercised. The readership in natural philosophy for some reason became vacant a few years afterwards, and as no fresh appointment was made, teaching in that department ceased even nominally to exist. We may be spared going into details as to the extent of the classes in the other two subjects; suffice it to say, that at the present time the physical science teaching supposed to be accomplished in Durham by the University cannot be regarded as anything more than the merest apology.

We have omitted any mention of Astronomy. Durham University has an Observatory and an Observer,—it has also a "Professor of Mathematics and Astronomy." We trust we do the present worthy occupant of the chair no injustice in supposing that his occupation has rested in the former rather than the latter branch of knowledge. With the Readership in Hebrew also on his hands, more than this could scarcely be expected.

Some knowledge of the history of previous movements, such as has been attempted in the above remarks, is needed if we would understand rightly the *status quo* at the commencement of the revived agitation.

It has been a matter of wonder to many that the northern coal owners and engineers, a body wealthy enough for almost any undertaking, and amongst whose faults cannot be reckoned want of regard to their own interests, even in that wide sense which embraces a care for the technical education of the rising generation, have remained passive during the long period which has elapsed since the failure of their first scheme; the more so as they appear to have in their "Institute" an organisation fitted to take the initiative in any public movement connected with theoretical or applied science. We are given to understand that one chief difficulty has been the want of house accommodation. The "Wood Memorial Hall," a building imposing in its design, if somewhat odd in its sculptural decorations, is expected to supply this deficiency, and its approaching completion indicates the opportunity for another effort. The new structure closely adjoins the buildings of the College of Medicine, the Natural History Society, and the Literary and Philosophical Society, suggesting lecture-room accommodation, a museum, and a library, ready to hand.

Meanwhile, a change seems to have been gradually taking place in the views held by those in authority at Durham; and when the Institute determined to invite all interested in the subject to a preliminary meeting a few weeks ago, it was with the conviction that a much more liberal response might be expected from the University than any hitherto accorded to proposals in which residence in Durham formed no part. As this first meeting appears to have been one for the expression of opinion only, and as this was elicited by the reading of a non-official letter from the Dean of Durham (*ex officio* Warden of the University) it is needless for us to dwell upon it further than to note the influential nature of the gathering, its unanimity as to the necessity for a thoroughly efficient school of physical science, and the determination that Newcastle was the only centre in which such an institution would have any chance of success.

At the adjourned meeting, the Dean of Durham and the Rev. Joseph Waite attended on behalf of the University, and as the proposals contained in the Dean's speech on that occasion seem to have been accepted as a general basis for the establishment of the College, we cannot do better than summarise them. They are roughly as follows:—

A College to be established in Newcastle for systematic teaching in Physical Science—literary subjects for the present to form no part of the curriculum.

Four Professorships to be constituted:—1. Pure and Applied Mathematics; 2. Chemistry; 3. Experimental Physics; 4. Mineralogy and Geology.

The curriculum of instruction to extend over two years; examinations to be held for degrees or diplomas in Physical Science, or other honorary distinctions subsequently determined upon.

Turning to the question of funds—the working expenses, including an adequate provision for the proposed chairs, were estimated at not less than 2,000*l*. per annum, in addition to the amount that might be received from students' fees.

Durham University offered to place in Newcastle two professors—one in Chemistry, the other in Experimental Physics—and to found ten scholarships, each of the value of 20*l*. per annum; five for first year, five for second year students. This offer was to be understood to extend to six years, but would be made in perpetuity if the results were such as to justify it. The government of the college to rest with a Newcastle board, upon which the University should have representatives. The Professors to be officers in the University, and degrees to be conferred in Durham; the examinations, on the other hand, to be conducted in Newcastle.

The one condition attached to the offer was that Newcastle should find a similar endowment of 1,000*l*. per annum, guaranteed for not less than six years.

The meeting appears to have been an eminently practical one, for more than half the required sum was subscribed in the room, and we learn that the amount named has since been considerably exceeded without any active canvass having been resorted to. The offer was naturally considered as accepted, and a Committee appointed to take steps for the carrying out of the scheme. The names of Sir W. G. Armstrong, the Rev. Dr. Lake, Mr. I. Lowthian Bell, Mr. Albany Hancock, and Mr. Newall, in connection with the executive, will carry assurance far beyond the northern counties that whatever is taken in hand will be efficiently accomplished.

The general design, sketched by the Dean of Durham, is so excellent that we are but little disposed to criticise its individual features, especially as the details which have been made public have not been put forward as representing foregone conclusions, so much as with the intention of supplying a basis for discussion.

If we read the scheme rightly, Durham is to provide two Professors and ten annual 20*l*. scholarships, and Newcastle to do likewise. Surely twenty scholarships in a newly-established college is an excessive allowance. Possibly this is not intended; at any rate such expenditure of funds would scarcely be entertained by the governing body without some reason not apparent to those less conversant with the requirements of the particular case.

Again, the selection of subjects as laid down by the Dean does not seem to be altogether happy. No scheme of scientific education in the present day can be regarded as satisfactory in which Biological Science is entirely ignored. It is true that the field of biology is too wide to be traversed in detail by a single lecturer or covered by a single course, and were the thing possible it would not be desirable. But it would be perfectly practicable for a Professor in Natural History to give in a short series of lectures much general information as to the organisation of the animal and vegetable kingdoms, and to select some special subject, either zoological or botanical, for more detailed exposition; and, as a means of training the observing powers, no portion of the curriculum would be so valuable. A college devoting its entire energies to one class of natural phenomena, to the exclusion of studies pertaining to natural objects and the phenomena of the organised world, can have no claim to be called a school of physical science in any wide or great sense.

Newcastle has already a position in respect to Natural History Science, and with an admirable Museum at hand, it would be a blot on the undertaking if its abundant local resources were not utilised.

THE UTILISATION OF NATURAL HISTORY MUSEUMS FOR SCIENTIFIC INSTRUCTION IN GERMANY

II.

THE German Museums of Natural History, founded for educational purposes, and connected with the various Universities, are by far the most important and influential. Those attached to the Universities of Berlin, Vienna, and Munich, are very large establishments, consisting of several sections which are independent of each other; and the aim pursued in each is to render the collections as complete as possible. Hence, although their chief purpose is to serve as schools of instruction for the students, they offer to the specialist abundant materials for original research, and have grown into attractive places of public resort. Those belonging to the smaller Universities are limited to instructive series of types; some, however, excel in one or more special branches. The teachers of the University are always the directors of the collections; thus the Professor of Zoology is the responsible head of the Zoological Museum, the Professor of Mineralogy of the Mineralogical, the Professor of Botany takes the charge of the Botanical Gardens, and so on. The appointments to the professorial chairs are not thrown open to competition; and the system of testimonials, humiliating alike to the candidate and his supporters, is unknown in German universities; men of repute for their knowledge and capability of teaching are chosen by the Senate, proposed to and appointed by the Government. We believe the Prussian Government has reserved to itself the right of appointment almost independent of the Senate, but, of course, is guided by the advice of men able to judge of the merits of the candidates proposed.

The lectures on the various branches of Natural History are either given in the Museum itself, or in a place adjacent. Each course occupies from 50 to 100 hours during a term of four months and a half. Some professors go with their students through a complete course of elementary instruction, whilst others leave this chiefly to private study, devoting the greater part of their time to inducting their pupils into the method of especial research. The attendance of students at the lectures is ensured by the circumstance that the Natural Sciences form part of the objects in which medical men, chemists, and teachers, are examined before they are allowed to enter upon the exercise of their calling. We are informed that, at a University with nearly 700 students (each remaining generally eight terms), some fifty would attend, within one term, the course of Natural Philosophy, or Botany; about twenty-five that of Zoology, or Comparative Anatomy; from fifteen to twenty that of Mineralogy, or Palæontology; and about thirty-five that of Geology. These were the numbers usually seen in the lecture-rooms, but there were, of course, other students who were prevented from attendance by various causes. The majority of those students who are desirous of receiving a perfect scientific education, and have the means for it, take advantage of the great variety of collections and instructors by prosecuting their studies at two or more Universities, finishing them at those places which offer the largest collections, and, in natural combination, the best instruction.

It may be mentioned here that the teaching of Science in German Universities is not entirely dependent on the public collections. Beside the staff of "ordinary" professors, there are younger men attached to the University, who have the right to teach, but can make only such use of the collections as the ordinary professor is disposed to grant. Most of them select, for their course of lectures, branches in the teaching of which they can dispense with the collections of the Museum—as, physiology of plants, histology and microscopy, history of development, general biology. This institution of "private docents," as they are called, is valuable not only to the students, but also to the body of instructors, inasmuch as it forms a preparatory school for men who intend to undertake the duties of an ordinary University teacher. The presence of an able and popular "private docent" has also not rarely had the beneficial effect of exciting to fresh exertions the ordinary professor, who had gradually lapsed into a course of stereotyped lectures. Nevertheless this institution can be regarded only as supplementary to the system of scientific education which is principally carried out in connection with the Museums.

We are not aware that there has ever been any lack of men combining an exact knowledge of some branch of Natural History with the aptitude for teaching it; nor have we ever heard of complaints that the duties of teaching seriously interfered with those of the curatorship; on the contrary, their union in one individual can have and has had only a beneficial effect. As teacher he knows best how to regulate the accessions and modify the arrangement of the collection, so as to meet the requirements of, and to be in accordance with, his system of teaching; and as curator he takes care that those parts which are not in direct connection with the lectures are not neglected, or that valuable specimens are not sacrificed for temporary purposes in the lecture-room or student's laboratory. Work in the Museum is as necessary for the training of the students as attendance in the lecture-room; and it is the duty of the teacher to devise suitable objects of research for his pupils. But if he had not the management of the collection, how could he be certain that the materials required are present, or will be made available? Would it be possible for him to superintend the student's work in a place where he is not the master? Were those duties assigned to two individuals, they would soon clash, to the injury of the service expected from the Institution.

The existence of numerous large or well-adapted collections, their utilisation for educational purposes, and the devotion of adequate time to instruction, are among the principal causes which have rendered the system of scientific education successful throughout Germany. But we must not forget that this success is due to the Universities only, and is limited to the classes receiving a University education. In the schools of lower degree, Science (with the exception of chemistry and natural philosophy) is only taught in the form of book-knowledge, in which the pupil takes but little interest, and therefore it has no great or lasting influence on the culture of his mind.

THE DESCENT OF MAN

The Descent of Man, and Selection in relation to Sex. By Charles Darwin, M.A., F.R.S., &c. In two volumes. Pp. 428, 475. (Murray, 1871.)

II.

THAT selection in relation to sex has been an important factor in the formation of the present breeds of animals was more than indicated in the "Origin of Species," and the theory has since been especially worked out by Professor Haeckel. It includes two distinct hypotheses. One is that in contests between males, the weakest would go to the wall, and thus either be killed outright, or at least debarred more or less completely from transmitting their characters to another generation. This may be regarded as a particular case of Natural Selection, and may be compared with the theory of protection by mimicry, suggested by Mr. Bates, and carried out by him and by Mr. Wallace. But though in the lists of Love the battle is often to the strong, even more frequently it is to the beautiful. This introduces a new process, of which the effects are not nearly so obvious as those of Natural Selection, either in its simplest form or in the more complicated cases of mimicry, and of sexual selection by battle. Many circumstances must combine in order that the most successful wooers shall have a larger and more vigorous progeny than the rest. In the first place, all hermaphrodite and all sessile animals may be excluded, and also those cases in which sexual differences depend on different habits of life. Mr. Darwin then shows that secondary sexual characters are eminently variable, and that males vary more than females from the standard of the species, a standard determined by the young, by allied forms, and sometimes by the character of the male himself when his peculiar functions are only periodical, or when they have been artificially prevented. Moreover it is the males who take the active part in pairing, and who not only fight for the possession of their mates, but display their colours, their voice, or whatever be their peculiar attractions, in order to gain the same end. This rule is confirmed by the exceptional case of the cassowary and a few other species in which the hens court the male birds, fight together in rivalry, and accordingly assume the brighter colours and more attractive shape usually worn by the male. Not only the parental and incubating instincts, but the usual moral qualities of the two sexes are in these cases reversed: "the females being savage, quarrelsome, and noisy, the males gentle and good." But it is further necessary to show that the females exert a choice among the males, and that the latter are polygamous, or arrive earlier at the place of pairing, as is the case with some birds, or else exceed in numbers, at least when both sexes are mature. On this point a series of observations is recorded relating chiefly to man, to domesticated mammals, and to insects. The rule as to transmission of male characters to both sexes appears to be that when variations appear late in life they are usually developed in the same sex only of the next generation, although they are, of course, transmitted in a latent condition through both; while, on the other hand, the differences which appear before maturity in the parent are equally developed in both sexes when transmitted to the offspring. The numerous apparent exceptions to these laws of inheritance and of sexual selection are examined with wonderful fairness and fertility in resource. I may particularly refer to the discussion of the ways in which the young and adults of both sexes differ among birds. The extreme intricacy of some of the questions considered is best shown by a postscript in which, with characteristic candour, the author corrects "a serious and unfortunate error" in the eighth chapter.

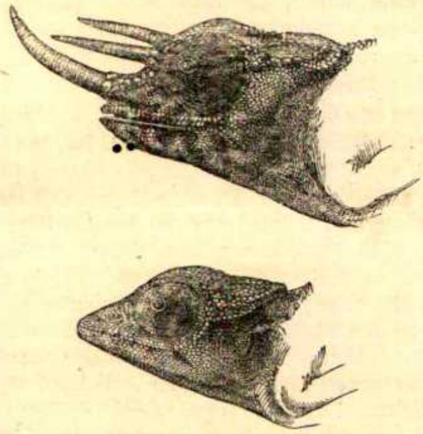

FIG. 2.—*Chamæleon Owenii.* Upper figure, male; lower figure, female.

The remainder of the first and the greater portion of second volume are occupied by a survey of sexual variations throughout the animal kingdom. Passing rapidly over the other invertebrate classes, the author devotes two chapters to the secondary sexual characters of insects. The weapons, the ornaments, and the sounds peculiar to the males of this vast group of animals are briefly described, and the remarkable analogy between insects and birds which is seen in so many other particulars is traced here also. The brilliant colours of many caterpillars, which, of course, cannot be due to sexual selection, offer one of the many difficulties which are faced, and this is explained by the aid of what the author terms Mr. Wallace's "innate genius for solving difficulties," as being due to natural selection. The bright colours warn the enemies of the caterpillars that they are unfit for food, and so benefit the latter, "on nearly the same principle that certain poisons are coloured by druggists for the good of man." Many cases are probably further complicated by mimicry, savoury caterpillars assuming the colours of distasteful ones so as to share in their immunity, in the same way that a druggist might label his bottles of sweetmeats "poison," to keep them from the shop-boy.

In the frigid classes of the lower Vertebrata one would think that sexual selection would have little play; yet Mr. Darwin gives several instances among fishes, amphibians, and reptiles in which weapons or ornaments, peculiar to the males, appear to have been acquired by this means. (See Fig. 2.) But it is in the great class of birds that the most complete series of examples is found, and our advanced knowledge of the habits of this class renders it the best possible field for the exposition of the whole theory. Again and again our author forestalls the evidence adduced in the chapters on sexual selection among birds, when tracing its first obscure operation among lower classes, and falls back on the same stronghold when explaining its less obvious working in the mammalia.

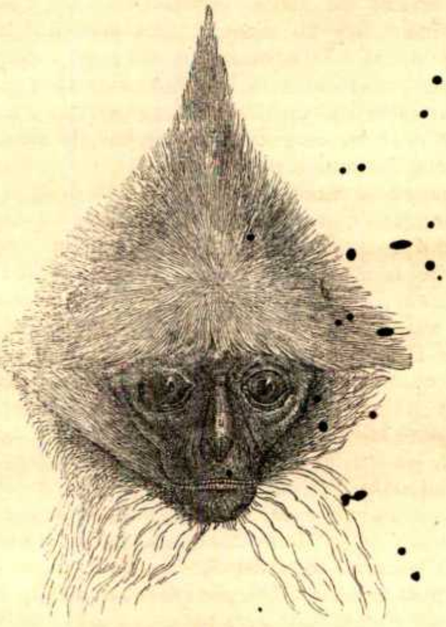

FIG. 3.—Head of *Semnopithecus rubicundus.* This figure (from Prof. Gervais) is given to show the odd arrangement and development of the hair on the head.

Among birds the rivalry of beauty has led to far more striking results than has the rivalry of strength. Foremost of these is the power of song, which, in accordance with the law of the least waste, is usually confined to birds of inconspicuous colours, while the combination of the harsh note with the magnificent plumage of the peacock is a familiar converse example. The object of the adornment of birds is conclusively proved by its being, as a rule, confined to males, and often to them only during the breeding season, as well as by the pains they take to exhibit their beauties to the hens. The difficulty is to show the precise way in which the results have been attained by gradual selection. In two remarkable instances, the wings of the Argus pheasant and the train of the peacock, Mr. Darwin succeeds in tracing the gradations in the same bird or the same family by which these wonderful and elaborate ornaments have been brought to their present perfection. The woodcuts which illustrate these gradations are unfortunately too numerous to be reproduced here; they are admirably drawn, and convey the impression of the feathers as nearly as is possible by the means employed. Indeed, we may here remark that throughout these volumes the original cuts, generally of details of structure, contrast very favourably with the figures of species taken from Brehm's "Thierleben," which are feebly drawn and ill-engraved.

Sexual selection has, of course, been continually checked and modified by the never-ceasing influence of natural selection, sometimes, as in the case of the horns of stags, being only somewhat diverted, but often directly opposed, as when it produces dangerously conspicuous colours, and dangerously cumbersome ornaments. In the case of birds, Mr. Darwin holds that the usual tendency of sexual selection being, to produce variation in males, its transmission to hen birds has been checked by natural selection. Mr. Wallace, on the other hand, believes that both tendencies have generally operated together, in opposite directions, so as to make successive generations of males more and more conspicuous than the primitive type, and those of females less so. The fact that, as a rule, young birds resemble hens in their plumage, is a strong argument for the former opinion, since most naturalists admit that early characters are the most trustworthy guide to natural alliances, *i.e.*, to true genealogy. To explain the transmission in some cases of brilliant colours (acquired probably by sexual selection, and therefore properly a male character) to both sexes indiscriminately, Mr. Wallace has framed the ingenious hypothesis, that the females have been protected from the dull uniformity threatened by natural selection, by their very general habit of building covered nests. Our author looks at the facts in a reversed way, and supposes that in most cases these hen birds, having inherited bright colours from the males, were led to the habit of building covered nests for the sake of protection.

Among mammals sexual selection has chiefly operated by increasing the size and strength of the males, and furnishing them with weapons of offence;* but besides allurements to the senses of smell and hearing, this class offers not a few instances, especially among the Quadrumana, of brilliant colouring being developed as a secondary sexual character. Here also we have the most striking instances of the production of defensive organs by the same process, as in the manes of lions, the cheekpads of some of the *Suidæ*, and possibly the upper tusks of that ancient enigma, the babirusa. Lastly, it is in the class of mammals that we meet with cases of what may be called primary-sexual ornament, as in *Cercopithecus cynosurus*, which make one wonder, with a thankful wonder, why such apparently obvious results are not more common.* We must, however, admit that such adornment is not more disgusting, nor that of which we copy a figure more ludicrous, than the personal decorations of savages. Sir Joshua Reynolds says that if a European in full dress and pigtail were to meet a Red Indian in his warpaint, the one who showed surprise or a disposition to laugh would be the barbarian.* But who could stand this test when meeting *Semnopithecus rubicundus* or *Pithecia satanas*?

We must admit, notwithstanding such anomalies, that, on the whole, birds and other animals admire the same forms and colours which we admire, and this, perhaps, may be admitted as an additional argument in favour of their kinship with us. Some of the ugliest creatures (like the hippopotamus) appear to have been quite uninfluenced by sexual selection, while the magnificent plumes of pheasants and birds of paradise are undoubtedly due to its operation. That it has occasionally led to unpleasing results in birds and monkeys of aberrant taste, is no more strange than that all savages do not carve and colour as well as the New Zealanders, or that most Englishmen admire ugly buildings and vulgar pictures. The prevailing aspect of nature is beauty, and the prevailing taste of man is for beauty also. The *means* by which natural beauty has been attained are various. Natural selection is one, by which the healthiest, and therefore the most symmetrical forms survive the rest. Protective mimicry is another, by which fishes have assumed the bright colours of a coral garden and butterflies the delicate venation of leaves. Flowers again have in many cases obtained their gay petals and fantastic shapes from the advantage thus gained for fertilisation by insects. The successive steps which have led to the graceful forms and brilliant tints of shells, to the intricate symmetry of an echinus-spine or a nummulite, these are as yet untraced even in imagination.

But that many of the most striking ornaments of the higher animals, and almost all those which are peculiar to one sex, have been developed by means of sexual selection, is a conclusion which can no longer be distrusted. There remain doubtless many exceptions to be accounted for, many modifying influences to be discovered; but the existence of a new principle has been established which has helped to guide the organic world to its present condition. Side by side with the struggle for existence has gone on a rivalry for reproduction, and the survival of the fittest has been tempered by the success of the most attractive.

P. H. PYE SMITH

HELMHOLTZ'S TONEMPFINDUNGEN

Die Lehre von den Tonempfindungen, von H. Helmholtz. (Braunschweig: F. Vieweg. London: Williams and Norgate. 3rd edition. 1870.)

THIS work traces the connection between physical and physiological acoustics, on the one hand, and the general principles and practice of music, on the other. Professor Helmholtz's qualifications for taking up this subject are unique. In each branch of science involved in the inquiry he has a reputation at least equal to that of any specialist in that branch. In the combination of eminently original mathematical power and consummate skill in physical and physiological research with the technical knowledge of a trained musician, he stands absolutely alone. It need therefore surprise no one that the volume before us, the first edition of which was published in 1862 as the fruit of eight years' work, has practically revolutionised the subject with which it deals. He begins by completely clearing up the nature of the *quality* (*timbre*) of musical sounds. He fixes his reader's attention on the *harmonics* which previous observers had recognised as accompanying a fundamental note. These, he shows, are no isolated phenomena, but invariable concomitants of nearly all musical sounds. In fact, what appears to be a simple note of any assigned instrument, is really a composite sound consisting of a number of different tones, all, however, members of a series connected together by a simple law. The *quality* of the sound depends on the relative intensities in which these *partial-tones* are present in the whole mass of sound (*Klang*) heard. Helmholtz illustrates his theory by determining the relative intensities of the audible partial-tones produced by the principal kinds of musical instruments, and also those corresponding to the different vowel-sounds of the human voice. He has also invented an apparatus by which the most important members of the complete series of partial-tones corresponding to a fundamental tone can be sounded with any assigned relative intensities, and which is capable of producing a tolerably close imitation of many sounds differing widely from each other in quality. These investigations occupy the first part of the work.

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* The very general transmission of such weapons to both sexes may, perhaps, be explained by the need females have of means to defend their young.

* Discourse delivered at the Royal Academy, December 10, 1776.

In the second part the nature of the difference between consonance and dissonance is explained, and thus a problem which has baffled natural philosophers since the time of Pythagoras finally solved. Here, again, the key to the solution is a perfectly well known phenomenon, the real significance and scope of which it was reserved for Helmholtz to recognise. Intermittent noises called *beats* had been observed whenever two notes nearly, but not quite, in unison with each other, were sounded together. Helmholtz asks what becomes of these beats when they are so rapid that the ear can no longer distinguish them as separate sounds. It had been supposed since the time of Young that they coalesced into a third musical sound, and thus formed the *combination ones* discovered as early as 1740 by a German organist named Sorge, but more generally known as *Tartini's tones*. Helmholtz proves that Young's view is erroneous. The beats never coalesce into a musical sound, but when they cease to be individually distinguishable, produce the sensation which we call discord.

This fact, taken in connection with the composite character of musical sounds, leads at once to Helmholtz's theory of consonance and dissonance. When two notes of different pitch are sounded, we have two series of partial-tones co-existing. If no member of the one series produces *beats* with any member of the other, the interval between the fundamental tones of the two sounds is an absolute consonance. If, on the other hand, beats *are* so produced, the consonance ceases to be absolute, and may be classed as a good or an imperfect consonance, or pass into a dissonance, according to the amount of discord involved in the combination. Helmholtz goes through the ordinary scale, and classifies the different intervals according to the above theory, his results tallying perfectly with those of the best writers on harmony. For the case of the comparatively unimportant class of sounds which have no upper-tones, Helmholtz employs a different method, which need not be detailed here. It is interesting to observe that his theory not only confirms some of the ordinary rules of musical composition, but is able to deduce principles which, though actually adopted by great masters, Mozart for instance, have never been explicitly stated by any theoretical writer. The third and last part of the work discusses the construction of musical scales, and the relation of each to its key-note. In this investigation æsthetical considerations necessarily assume an importance which they could not claim in the two earlier purely scientific parts of the work. As, moreover, musical technicalities of much complexity abound throughout the inquiry, it is not possible to give a popular *résumé* of the general results obtained in the third part.

The above is the most meagre outline of the subjects treated in the "Tonempfindungen." Indeed it is absolutely hopeless, within any reasonable limits, to try to convey an idea of the thoroughness, the laborious accuracy, the wonderful many-sidedness which appear on every page of it. The author, though a great mathematician, is fortunately too great an experimentalist to allow the laws of nature to figure as mere examples of the integration of differential equations, or as but affording subject-matter for new mathematical conundrums. Each acoustical law is thoroughly explained in popular language, with the most attractive richness and variety of illustration, a method of treatment infinitely refreshing to a student who has hitherto experienced only the husks of our arid examination-ridden manuals. All details of calculation are relegated to an appendix, and, though mathematics has its due honour given it, as a science absolutely indispensable for *thorough independent mastery* of any branch of physics, the most effective practical discouragement is given to the pedantic notion that no valuable knowledge can be gained without it. We may well doubt, indeed, whether the long exclusive domination of theory has made anything beyond mathematical symbols really *understood*. Cambridge honour-men will know what we mean by saying that an average wrangler, if asked what a *wave* was, would probably unhesitatingly answer—

$$\text{“}a \sin \frac{2\pi}{\lambda}(vt-x)\text{,”}$$

and refuse to produce any further explanation. We desire for works like the "Tonempfindungen" a triumph in this country over English books "adapted for writing out in examinations" as decisive as the victory of the German armies on the soil of France.

SEDLEY TAYLOR

OUR BOOK SHELF

A Monograph of the Alcedinidæ, or Family of Kingfishers. By R. B. Sharpe, F.L.S., &c. Librarian to the Zoological Society of London. 4to. (Published by the Author. 1868—1871.)

THIS work reflects the highest credit upon its author, and will establish his reputation as an Ornithologist. Very few monographs published in England are so entirely satisfactory as this one, for not only have the several parts appeared regularly during the last three years, but the concluding double number just issued contains a copious and well-written introductory chapter on classication, geographical distribution, and literature, which renders the book a model of what such a work should be. The Kingfishers, although represented in our country by only one species, are especially abundant in the Eastern Tropics, where they exhibit a great variety of form and the most exquisite beauty of plumage. A considerable number of them are inhabitants of the forests, and never frequent water, subsisting on insects, small crustacea and mollusca, and the larger species even on snakes, lizards, and other reptiles, which they capture by darting down upon them from a branch just as our own species pounces upon a fish. Mr. Sharpe has been fortunate in securing the services of a young Dutch artist, Mr. Keulemans, who has himself studied birds in the tropics, and seizes upon their various attitudes with the happiest fidelity. He also surrounds his figures with little bits of appropriate scenery, so that a considerable number of the 120 plates with which the book is illustrated are beautiful pictures, as well as admirable representations of the several species. We do not hesitate to say that many of these plates are equal to the very best that have appeared in any illustrated work of Natural History. The body of the work consists of coloured figures of every known species of kingfisher, with full synonymy, careful description, and record of whatever is known of its habits. In the introduction, the classification of the species is carefully considered, only those generic groups being retained which can be characterised by marked structural differences. The whole family is first divided into two sub-families: the Alcedinidæ, or true kingfishers, characterised by a compressed keeled bill; and the Daceloninæ, or king-hunters, which have a depressed bill rounded or furrowed above. These are subdivided into nineteen genera, in which are grouped the 125 species of kingfisher now known. The groups are all characterised by modifications of the bill, feet, or tail, and a plate exhibits these generic characters at one view. There is also a tabular key of the species in every genus and of the genera in each sub-family, and the reasons are given for rejecting numerous genera proposed by other authors on insufficient characters. The geographical distribution of the species is then discussed in the same careful manner, an exact account of the known range of every species being given, as well as tables showing at a glance the distribution of all the species of a genus or group of allied genera; after which the results of the examination are ably summed up. Kingfishers present us with some of the most curious anomalies of distribution to be found in the whole class of birds. There is no part of the world so rich in peculiar forms of bird-life as America, more especially the southern half of it, yet it is the poorest of all parts of the world in kingfishers, only eight species being found in the whole continent,—a continent with more rivers and more fish than any other! The single island of Celebes actually contains as many different kinds of kingfisher as all North and South America, while New Guinea contains more than twice as many. It is perhaps even a more extraordinary fact that there is no peculiar type of kingfisher in America, all the eight species belonging to one genus, and that genus found also in Europe, Asia, and Africa. In Africa we have three peculiar genera of kingfisher, and twenty-four peculiar species. In continental India there are only five peculiar species, and not one genus. The western Malay Islands (Indo-Malayan sub-region) have one peculiar genus, and eleven peculiar species; the Philippines, seven peculiar species; but the Australian region has no less than ten peculiar genera and fifty-nine peculiar species, or nearly half those of the whole world. The peculiarities of the island of Celebes are well shown by the kingfishers, for not only has it eight peculiar species and three peculiar genera, but one of the latter has affinities with an African genus. In discussing the general relations of this isolated group of birds to the rest of the order, and the mutual affinities of the genera, the conclusion is arrived at that they are most nearly allied to (although still very remote from) the hornbills; and their relations are expressed by a branching diagram, as well by a map of the genus on the plan of Professor Flower. A copious account of the literature of the family is also given, no less than 135 separate works being enumerated, with references to every species of kingfisher described or noticed in them. An elaborate paper on the anatomy of these birds by Dr. Murie, with a full index, completes this exceedingly valuable work, which will be equally acceptable to the naturalist for its detailed and accurate information, and to all who love nature for its beautiful and artistic illustrations.

ALFRED R. WALLACE

The Wind in his Circuits, with the Explanation of the Origin and Cause of Circular Storms and Equinoctial Gales. By Lieut R. H. Armit, R.N. (London: J. D. Potter, 1870.)

ACCORDING to Lieut. Armit "all the various phenomena which occur in Nature are accounted for by one theory forming one law, and the force which governs and regulates everything, even to imparting perpetual motion to the world, is Electricity" (p. 122). When the reader is informed of the author's opinion that the east wind is formed of compressed vapour or steam (p. 57), that lightning and thunder are caused by the Arctic current descending to fill any vacuum that may suddenly be found in the warm currents below it, the "grating" of the currents against each other causing friction and lightning, and the sudden shock of the impenetrable masses the thunder (p. 68); and that, by an attentive study of his theory, it will in future "be as easy to foretell and evade a storm and keep in a fair wind, as it is to drive over good roads and evade the bad ones, when you know the country you are driving through" (p. 126), he will understand that the book may be consulted out of curiosity, or for its psychological interest, but not for instruction in what concerns atmospherical phenomena and the laws which govern them.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Pangenesis

ON the introduction of Mr. Charles G. Leland, the author of the famous "Breitman Ballads," who was present at the reading of Mr. Galton's paper on Pangenesis before the Royal Society on the 30th ult., I have seen Mr. Lewis Ware, a young American gentleman who has been studying science in Paris since 1868.

By him I am informed that M. Leconte (I presume the physiological chemist of that name) is accustomed to mention in his lectures that he had frequently transfused the blood of one kind of animal into the veins of another; but it does not appear, in reference to those experiments, that any subsequent effects were noticed, as regards the offspring of such animals.

M. Leconte, however, further relates that once, not by way of experiment, but in order to save life, endangered, it must be supposed, from the occurrence of previous hemorrhage, he transfused into the veins of a white man blood drawn from a negro, and that the subsequent offspring of this white man by a white mother were *swarthy* in complexion.

Now, I cannot find any *published* record of M. Leconte's operation and its singular consequences, and it is impossible at the present moment to reach him by letter. I desire therefore to give publicity to what *seems* to be a conclusive proof of the theory of "pangenesis," with the view of eliciting a confirmation or refutation of the statement from some one who may chance to read this note, and who may have the necessary opportunities and leisure for further inquiry into the particulars of so very remarkable an incident. It is obvious that the number of children so affected, and the coincidence or absence of other changes in the hair, the form of skull, &c., require to be investigated, and the credibility of the parents fully authenticated.

10, Savile Row, W., April 8 JOHN MARSHALL

The Spectra of Aurora and Corona

So much attention has been drawn to the correspondence between the spectrum of the corona and that of the aurora, as to lead one to suppose that they were almost identical; or, at least, that the principal auroral line was also seen in the corona. But even this is not at all the case. As the readers of NATURE are aware, the light of the aurora is almost monochromatic, giving a spectrum of one bright line in the yellowish green (wave-length about 557), and three or four very faint bands, which are more refrangible. These last are only occasionally visible, and indeed, Angstrom, in 1869, had seen them but once, and that momentarily. It is with one of these faint bands that the 1,474 corona line (wave length 531·6) is said to coincide, and not with the bright line (wave length 557), which is entirely absent in the corona spectrum. Two more of the auroral bands are near to the F and G hydrogen lines, which are visible in the corona, but it is yet doubtful whether they coincide. It is not impossible that a faint H spectrum may be produced in the aurora by the moisture of the air, but I incline to attribute them to the low temperature air spectrum mentioned in my letter of February 7, and which has bands in nearly the same positions. From the extreme faintness of the auroral bands, it is of course impossible to measure their positions with great accuracy.

Under these circumstances it would seem rather premature to lay great weight on the supposed coincidence, and much careful work must be done both on gas spectra and on that of the aurora before we can say with any confidence that these lines are not due to gases already known to us under different circumstances of temperature and density.

I subjoin a table, giving the approximate wave lengths of the lines observed in the auroral and coronal spectra.

Lines of Corona and Prominences.		Lines of Aurora.		Observers.
Description	Wave length	Description	Wav elength	
C Hydrogen	656			
		Bright line.	630 640*	H. R Procter and others.
D Sodium	589			
D3				
		Bright line.	556 7 557 569†	Angstrom Winlock Alvan Clarke, jun.
1474 line	531 6	band	531 532	Winlock Alvan Clarke, jun.
E	527			
		band	520	Winlock
b	517			
F Hydrogen	486	band	485	Alvan Clarke, jun.
		band	464	Winlock
G Hydrogen	435	band	434	Alvan Clarke, jun.

HENRY R. PROCTER

Royal College of Chemistry, March 28

The Aurora

THE splendid aurora which was visible here last night was probably seen in many other places, and from a comparison of data, perhaps the position of the luminous arch, which formed a conspicuous part of it as seen from here, may be made out.

At 10.30 it passed through the northern part of Corona, 12 Canum Venaticorum, and the head of Leo. At the time mentioned above, the whole of the light was a vivid green, but at about 10.40 red patches appeared, and at 10.45 rays shot up to a point situated about 4° S.S.W. of 12 Canum. The colour round this point was a most wonderfully dark blood red, and in many other parts of the heavens the same colour was seen, very different from the rosy light of last October.

About 11 clouds coming up covered the whole sky, and on their partially clearing away, the aurora was much decreased in brilliancy. The lurid red light reflected from the detached clouds which preceded the main body produced an exceedingly grand effect. The light was strong enough to read type of the size in which NATURE is printed.

On first observing the green parts with a spectroscope of one bisulphide prism, the only line distinctly visible was the green one; but by watching and opening the slit there came into view two bands at the more refrangible end, more sharply defined at the more refrangible side than at the other, and there also seemed to be a considerable continuous spectrum from the green line nearly to the least refrangible of the two bands.

In the red parts the red line was most brilliant, quite equal in intensity to the green one, and then even in the green light it was distinguishable with care and long watching.

York, April 10 T. H. WALLER

LAST evening, at about 9.50, my attention was called to a magnificent display of aurora borealis. A mass of light, composed of red and bluish-white streamers or rays, moved rapidly up from about W.S.W. to E.N.E., and the whole took the form of an arch overhanging for a short space of time the western horizon, while transverse waves of light, intensifying the lustre of the blue portions, and occasionally reaching almost to the zenith, rolled across the nebulous mass at intervals of about a second. Streamers projecting eastwards kept shooting out with great brilliancy, like sheet lightning, except that they were separated by a sharp line from the dark surrounding sky. Each flash made a fresh advance eastwards, like the skirmishers preceding an army, and a few bright crimson clouds alone kept nearly the same position throughout the display,—the finest of these was nearly in the S.W. At one time, when the flashes and waves had ceased, an umbrella-like radiation of red and white rays from the zenith attained great beauty. By five minutes past ten the sky had regained a more ordinary hue, but dull red clouds still remained, and in the west a white phosphorescence like early dawn. The night was calm and rather cold; the barometer 30·00 and steady. The wind had been gusty from E. in the afternoon. I had remarked during the daytime some very rare and beautiful modifications of cirrus and wave-cloud stretching from W.S.W. to E.N.E. (a direction coinciding with that of the aurora streamers), and crossed by bars at right angles to them at a lower elevation. The arrangement of these clouds showed that they were strongly acted on by electricity, as is generally, perhaps, the case with wave-cloud. May not vapours of this kind in a peculiar state produce the apparent polarisation of the sun's rays recorded by "J. W." in the *Times* of April 8? To-day, especially, between twelve and one o'clock, detached cumuli, driving rapidly from about E.S.E. and upper cirro-cumuli from E.N.E., have behaved in a manner quite unlike anything I have observed before. The only cirro-cumulus to be seen in the morning shot out branches in advancing and melted away between the branches, leaving a sort of skeleton of spine and ribs, which in their turn were dissipated. The cumulus motions were also unusual, for portions were suddenly arrested, remaining fixed, and then rose apparently and arranged themselves in bands more or less parallel until dissolved. The last phenomena only occurred, as far as I could see, below some whitish rays, extending across the sky from W.S.W. to E.N.E., parallel to the aurora streamers of last evening, and closely resembling the beams of light which often proceed from the sun when behind a cloud on a showery day. Either an aurora must be going on to-day, or the higher atmosphere is in a peculiar electrical state. The influence of these whitish bands on the clouds at a much lower level is at any rate remarkably powerful.

Wrexham, Denbighshire, April 10 F. R.

Solar Science at the pleasure of *Secret* Referees

IN the faithfully-recording columns of NATURE for March 30, at p. 434, is a much required abstract of Mr. Stone's important paper, recently communicated by him to the Royal Society, London, on the connection between terrestrial temperature and sun-spot phenomena. By comparing the curves of mean annual temperatures during the last thirty years (as observed, ready to his hand, by his indefatigable predecessor Sir Thomas Maclear) with another curve constructed on Wolf's observations of sun-spots, Mr. Stone has been enabled to deduce, almost immediately after arriving on his new scene of labour at the Cape of Good Hope, first, that there is an approximately decennial period of such temperature, and so similar to that of the sun-spots as to indicate more than a mere coincidence; and secondly, that the sun-spots are not to be looked on as the direct ages of thent temperature variation, but that either phenomenon results from some general change of solar energy.

As Mr. Stone expressly mentions "that he had not the slightest expectation, on first laying down the curves, of any sensible agreement resulting," I presume that he is not aware that upwards of a year ago I both sought the honour and experienced the grief of communicating to the self-same Royal Society of London an almost exactly similar research, resulting in almost the same solar conclusion; my foundation matter being the mean annual rock-temperatures observed in the Royal Observatory, Edinburgh, during thirty-three years (twenty-four of them under my own supervision) with the recorded sun-spot and other solar phenomena by Schwabe, Wolf, the Kew Observatory, and other authorities, during the same period.

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* Seen on October 26 last year, but very rarely visible.

† Prof. Pickering considers this an error. My own measures give a wave length very slightly greater than those of Winlock and Angstrom.

And yet why was Mr. Stone *not* acquainted with this previous work of mine, communicated to the very same London Society, at a time too when he was still in London or very close to it?

The answer is probably, that it was the work of the *Secret* Committee of the Royal Society appointed by the Council to report on my paper. For the Committee's first proceeding was to keep the paper shut up with themselves for upwards of seven months; and their next, to condemn it before the Council on several counts, the two chief of which seemed to be—first, that I had inveighed without occasion, in a note on a certain page, *against* British units and standards of linear measure; and secondly, that I was not to be allowed to compare the Edinburgh mean annual temperatures with sun-spot observations.

Touching the first objection, I showed that there was no such note or sentiment throughout the whole paper; and with regard to the second, I felt scientifically constrained to declare, that I could not consent to be denied the right of comparing so long, accurate, and indeed hitherto unsurpassed, a series of earth-surface temperatures as those of the Edinburgh Observatory, with acknowledged solar phenomena; especially when confining myself to merely trying and exhibiting for annual means whether there was, or was not, any sensible correspondence in time and manner.

For this rebellious opposition to the despotic dictates of the *Secret* Committee, my paper was instantly extinguished by the Council of the Royal Society; and I was put to the further indignity of receiving an unpleasant letter from the secretary, on merely requesting to know the names of the gentlemen constituting the said Committee, whose identity in the flesh is therefore still a problem. But now, see how speedily the *Nemesis* of deeds of darkness has overtaken those who dabble in them; for the newly-appointed Astronomer Royal at the Cape of Good Hope, in charge of the British Observatory which stands next in importance to Greenwich; one of the Royal Society's own Fellows too, has, moved by some most mysterious impulse, made that very subject, just banned by the carefully concealed sages who speak from behind a curtain, the material of his first scientific communication from the Southern Hemisphere; and, with even a cruel degree of polite attention, he has sent that paper, filled with their own forbidden matter, to the Royal, rather than any other, Society in London, to read aloud before their members.

Yet this chief and leading society's secret Star-chamber, though exclaiming, perhaps, to the South African astronomer, "*Et tu, Brute*," has not repented, at least in any generous or human manner; for they have left me both to find out from NATURE the whole character of Mr. Stone's paper just accepted by and read before them, and to form my own unaided conclusions.

Wherefore I cannot but wonder more than ever, and even with exceeding admiration, at what *any* Scientific Societies in the present day have got to do with that accursed thing in all national history represented by Secret Committees, secret members, secret judgments, *veiled* prophets, who may, and—as would most clearly be shown if the whole correspondence in this case were to be published—who do, blunder utterly in understanding a plain sentence of simple English, who likewise enact a mistaken rule to tie down some astronomers in their own business, prove themselves totally void of Christian charity and gentlemanly feeling, and all the time require the incense of passive obedience to their partial edicts and strange behests.

Is not this then a matter just as important as any that can occur in the interests of true science and unalloyed, for the Royal Commission now sitting on Scientific Education and the Advancement of Science to take account of! For, if that Commission fitly and fully represents the general government in these times of this free and enlightened land in which we live, it would seem to be one of their holiest duties to the nation at large, to see that a base political method of a past and exploded era of our history, after being driven with ignominy out of every other branch of government, be not allowed to linger in sequestered nooks and dark corners of State-supported or State-aided Societies for scientific pursuits. C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, March 31

Ocean Currents

MR. LAUGHTON thinks (see his letter in NATURE of April 6) that the ocean surface current, which flows into the Mediterranean by the Straits of Gibraltar, is due to the preponderance of west winds over the Atlantic. Were this so there ought to be a similar current flowing into the Baltic; but on the contrary, there is a surface-current flowing out of the Baltic.

I am convinced that Dr. Carpenter is right in accounting for the currents at the entrances of both the Mediterranean and the Baltic by the differences in the degrees of saltness of different seas. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim

Sun Spots

SOME very remarkable maculæ appeared on the sun on March 28, 9^h 50^m A.M., which are of interest from the fan-like portion of the most westerly of the large spots having been much enlarged whilst under observation. The enlargement took place from the centre outwards, and occupied several seconds in its development. W. H. WALENN

74, Brecknock Road, N.

The Echoes of the Royal Albert Hall

THE following observations may assist in reconciling some of the conflicting statements respecting the above subject, and explaining their curious discrepancies:—

The shape of the building is nearly elliptical; for the sake of description I will assume that it is a true ellipse. The middle of the platform, down which Her Majesty walked on the occasion of the opening ceremony, corresponded to the major axis of this ellipse, the seats in the arena were arranged in rows at right angles to the major axis, and thus the middle row of these seats was placed across the minor axis. I sat on one of the chairs of this middle row, that against the edge of the platform, and thus was in the nearest available place to the geometrical centre of the building, and therefore well situated for one set of acoustic observations. I listened very carefully, and made the following notes:—

An invisible military band was playing for some time before the arrival of the Queen. This music was freely heard without any perceptible echo, but the outlines of the sound (if I may use such an expression) were slightly shaded, there was just that want of crispness and definition of individual sound which would be advantageous to an ill-timed band, but somewhat damaging to the display of one that was playing staccato passages with perfect unity.

When the Prince of Wales read his address I heard every word repeated with perfect distinctness, the echo was pure and single, the two voices appeared like those of a prompter and a faithfully repeating speaker. The echo was remarkably well defined, and nearly as loud as the voice of the Prince.

When the Queen replied, her words were also repeated, but far less distinctly. This was a respectful whispering echo. When Santley sang a solo, there was no distinct echo, only a slight confusion of sound; but every note of Madame Sherrington's solo was most vexatiously mocked, but not so distinctly as the words of the Prince of Wales. A slight murmur accompanied the band, but it required a strain of attention to detect any definite echo.

The above is a description only of what I heard.

A friend who was sitting at the other end of one of the middle arena seats, *i.e.* near the boxes, or circumference of the ellipse, did not hear these echoes at all, and I have since learned that this was the case with others seated in boxes on either side of the hall.

The explanation of these facts is not difficult. On both sides of the organ, which occupied the south end or vertex of the ellipse, are unbroken curved wall spaces above the choir, covered with wooden panels or lining, a material well adapted for the reflection of sound. Beyond these bare spaces the boxes begin, and extend round the circumference till they reach the bare wall-space on the opposite side. There can be no regular reflection from the large area covered by the boxes when these are filled with people, but there must be such regular reflection very distinctly effected by the small bare portion of the curvature on each side of the organ.

If the curvature of this bare wooden wall-space were a true parabola, and a sound were emitted from the focus of the curve, it would be reflected in straight lines parallel to the axis, and only extending to a short distance on each side of it. With the actual curvature a close approximation to this should occur, the echo of the sound thus emitted would travel along the platform and extend a short distance beyond it, and thus be distinctly audible to the beefeaters arranged along the platform, and to those who like myself were sitting close to it.

When the Prince read his address, he stood very nearly in the south focus of the ellipse, a little the east side of it. In facing Her Majesty his voice was projected directly towards that portion of the panelled curve which should theoretically supply the parallel waves of sound to my side (the west) of the platform. Thus the distinctness with which I heard the echo of his voice is explained, and also the absence of echo in those parts of the hall farther distant from its major axis.

Her Majesty was near the focus, but not so near as the Prince of Wales. Santley was considerably to the west, and Madame Sherrington was on the east of the focus, but not so far removed as Santley. The quality and pitch of Madame Sherrington's voice would of course assist in rendering the echo more distinct. The band and chorus being distributed between the focus and vertex of the ellipse, fulfilled their theoretical requirements by having no regular echo, nothing beyond an indefinable rumble.

That the distinct echoes I heard came from the wooden lining was evident to the sense. I have often amused myself by experimenting upon mountain echoes, and when there is no redoubling have usually been able to detect the direction of their issue, and to find the rock surface producing them. The echoes of the voices of the Prince of Wales, Her Majesty, and Madame Sherrington, were remarkably free from any degree of redoubling, and evidently came from the direction of the south wall.

The remedy for this is obvious enough. This blank wall space must be covered with some kind of drapery, or broken up by ornament. Drapery will promote absorption of sound, an irregular ornamental surface will convert the regular reflection into scattering reflection.

I listened very carefully for any indications of echo from the roof, but could detect none, either the velarium was quite effective, or the echo, formerly attributed to the roof, came from the walls, or my position prevented me from hearing the roof echo. As I was not present at the first experimental concert before the velarium was put up, I will not venture any opinion on these points.

My apology for publishing these details is that the subject of the acoustics of this Hall is one of vast importance, both in reference to the Hall itself, and the intellectual progress of the inhabitants of all great cities. If large masses are to be taught orally, we must learn how to construct buildings in which the human voice may be audible to assembled thousands. The Royal Albert Hall presenting the grandest, and, I think, we may now say, the most successful experiment ever yet made in determining the possible limits of oral teaching, every contribution to a knowledge of its acoustic properties is of great value. I offer the above simply as such a contribution of observations made from one point—perhaps the most important point, and hope that it will be followed by other observations from other parts of the building. By combining these a correct knowledge of the whole subject may be obtained.

W. MATTIEU WILLIAMS

Gum Dammar

In a valuable article in the current number of the "Quarterly Journal of Microscopical Science" the Radcliffe Travelling Fellow alludes to the general favour with which Prof. Stricker and other eminent German workers, regard Gum Dammar as a substitute for Canada Balsam as a medium for the preservation and preparation of histological specimens. Mr. Mosely further points out an unfortunate mistranslation in the Sydenham Society's English edition of Stricker's work, in which *Dammar firniss* is translated Canada Balsam, and regrets that good Dammar firniss (*Anglice* dammar varnish) cannot be obtained in England, though the gum itself is sufficiently common. It so happens that I have made use of various home-made preparations of Dammar in preference to balsam for some time past. I have found that it not only is a substitute for the latter "torment to beginners," but that it possesses many advantages over balsam, *e.g.* it is easier to use, sets more rapidly, and is above all clearer, more perfectly transparent, and more dense. Perhaps many of your microscopical readers will be glad to learn that a very good "dammar varnish" is made and sent out in a suitable form by Mr. Walter White, of Monmouth, to whom I am indebted for my knowledge of some valuable formulæ for its preparation.

Hull H. POCKLINGTON

THE IRON AND STEEL INSTITUTE

In NATURE for March 30, we called attention to the annual meeting of the above society, which had just commenced in London. The new President, Mr. Henry Bessemer, delivered his opening address on the afternoon of Tuesday, when there was a large and influential attendance of the members from Scotland, South Wales, the North of England, South Staffordshire, Yorkshire, and other districts connected with the industries represented by this Institute. There were also several gentlemen present representing Continental and American works. Various matters of considerable practical importance were introduced to the notice of those present by Mr. Bessemer, and it is understood that the attentino of the Council will shortly be directed to carrying out, as far as possible, the suggestions contained in the address. Perhaps the most important subject mentioned was the proposal for a new series of tests to ascertain accurately the relative strengths of iron and steel. Many of the data upon which engineers and manufacturers have now to depend are comparatively old, and refer to classes of iron that are not made so extensively as they were at the time the experiments were instituted. New classes of iron have risen into importance during the last few years, and steel has become a very valuable article for engineering construction, so much so that it is desirable the materials now available for use should be thoroughly tested, in order that their properties may be fully understood by engineers and ironmasters. The proceedings at the meeting on Wednesday and Thursday were of a varied and interesting character. The committee appointed to investigate the subject of distribution of iron ores in Great Britain, presented their preliminary report, in which they stated that they had received from gentlemen connected with the trade special communications upon the matter. They gave a *résumé* of the production of iron ore in the various districts, and in many cases they intimated that the mineral statistics published by the Mining Record Office did not accurately represent the quantity of material raised. Taking a general view of the production of iron ore in the United Kingdom, they remarked that attention is now being mainly directed to the development of those districts that yield the cheaper classes of ore, such as Northamptonshire, Lincolnshire, and Cleveland, and to those localities where deposits of hæmatite are obtainable. The great demand for the latter ores is stimulating research in every direction where there is a probability of this mineral being found, but the circumstances under which it was deposited are at present so imperfectly understood that it is impossible to estimate with any precision where these researches are likely to prove successful. The Committee was re-appointed, and we understand that it is their intention to proceed with the preparation of a more thoroughly comprehensive report upon the subject of their investigations, and that this will be presented at an early meeting of the Institute.

The Committee appointed to report upon the question of mechanical puddling stated that they had given considerable attention to the subject of their investigation. They had paid visits to the principal forges where furnaces for mechanical puddling were in operation, and they gave a detailed list of such works. With respect to the Continent, they reported that Mr. Menelaus had kindly undertaken to obtain from France a set of the apparatus used at La Haynge, to put the same in operation at Dowlais, and to ask the Committee to see it at work in due course. Had it not been for the Continental war this machine would long ago have been at work, but it was detained in transit, and has not yet arrived in England. The Committee intimated that they were making arrangements for obtaining the services of a practical engineer, and of a gentleman acquainted with puddling, to visit the different works, and thoroughly to investigate the merits of the various machines in existence. The Committee proposed afterwards to verify the statistics given in by this gentleman.

The subjects which these Commissions are investigating are of great interest and importance to the trade, and it is to be hoped that the gentlemen entrusted with the inquiries will be able to prosecute them to a satisfactory issue. The paper by Mr. F. Kohn on the production of alloys of iron and manganese gave rise to a discussion, in which it was asserted that the manufacture of spiegeleisen had at length become an established fact in connection with the iron trade of this country, the Ebbw Vale Company having recently succeeded in extracting that material from the spathic iron ores of the Brendon Hills. Considering that this company have devoted a long time to the solution of this important question, it is satisfactory to find that they have been fortunate in this matter. The results obtained by the Terre Noire Company in the production of a soft steel are very remarkable, as Sir W. Fairbairn testifies that in ductility this steel stands in the ratio of ·219 per unit of length to ·092 of the Bessemer steel manufactured at Barrow from hæmatite ore. The steel in question was made by the use of ferromanganese made by Mr. Henderson's process, and containing an average of 23 to 25 per cent. of metallic manganese. The pig iron is run directly from the blast furnace to the converters without mixing with other kinds of iron, and the rich ferromanganese is used instead of the ordinary spiegeleisen.

Mr. Walker read a communication on modern appliances for reversing rolling mills, which introduced a notice of the principal arrangements now adopted for securing reversing action in rolling iron and steel. This is a matter of great importance, and has engaged the attention of engineers for some time past. The principal systems are—First, by reversing motion of the ordinary locomotive type; and, secondly, by friction clutches. The latter plan was most particularly described. It was stated that the rolls can be reversed at any speed, and the mills fitted up on this method are remarkably effective. Mr. Ferrie contributed a paper upon an improved form of blast furnace recently introduced into Scotland. This differs from the furnace in ordinary use by the addition of about 20ft. at the top, which is divided into four compartments, and the raw coal charged in at the top is coked in its passage down the vertical chambers. The practical results of the working of this furnace over a considerable period has been a saving of at least one ton of coal for each ton of iron produced. In the discussion it was maintained that the advantage derived by Mr. Ferrie from the alteration of his furnace were mainly due to increased height; but it was also advanced that with raw coal for fuel it would not be practicable as a rule to make furnaces 80ft. high work satisfactorily. Mr. Ferrie appears to have demonstrated that high furnaces using raw coal can be made to give as good results as are obtained in the more modern districts. Mr. Tate read a paper giving a very clear account of the distribution, extent, and value of the iron ores of Antrim, which are now coming into notice, and will doubtless very soon be extensively employed in the west coast and elsewhere in the manufacture of hæmatite iron. Mr. D. Forbes also read a paper on iron ore, his subject being a brief description of the central mining district of Sweden. He called attention to the immense deposits of iron ores of great purity that will soon be opened up by the construction of a railway which will give direct access to the North Sea, and will thus render it feasible to import the Swedish ores into this country. The discussion on the Sherman process was interesting, but showed pretty clearly that the practical men who had taken the matter up, and had investigated it thoroughly, had not been able to arrive at any satisfactory results from it. Considering the nature of the chemical substances applied in the treatment of the iron by this process, it was only natural that such negative results should be obtained.

We must not omit to mention the lecture by Prof. Roscoe on Wednesday evening on spectrum analysis in its relation to the manufacture of iron and steel. There was not very much to state with respect to recent discoveries in the application of spectrum analysis to the production of steel, but it was important that the trade should know exactly what they might, and what they might not, expect from this delicate method of chemical investigation. Prof. Roscoe, in a very able and lucid manner, laid before the members of the Institute all that is at present known about the subject. One discovery by Mr. Snelus is likely to lead to good results. He has found that by the aid of the spectroscope he can predict the exact length of time that a "blow" will last in the Bessemer converter, and he hopes to be able eventually to stop the operation at a time when a definite amount of carbon may be left in the molten iron, and thus the quantity of speigeleisen required to produce steel of a given quality will be reduced.

The proceedings, on the whole, were of a thoroughly satisfactory character, and, considering the short time the Institute has been in existence, it seems highly probable that it will eventually occupy a position of much influence and importance.

NOTES

His Excellency the Lord Lieutenant of Ireland has been pleased to appoint Dr. R. O. Cunningham to the Professorship of Natural History in the Queen's College, Belfast. The newly-appointed Professor is well-known as an enthusiastic naturalist. The pleasant post of Professor to the most successful of the Queen's Colleges in Ireland is one that we are glad to see filled by the appointment of so estimable a man.

We rejoice to be able to announce that the following Scholarships have been established by Gonville and Caius College, Cambridge, and hope it will not be long before all the other Colleges follow so good an example:—A Scholarship in Natural Science, tenable for four years from Lady-day, 1871, and of value from 60*l.* to 20*l.*, according to the candidate's proficiency, will be awarded at this College in June next. The successful candidate will require to enter his name at this College forthwith, and to begin residence in October. There will be three distinct examinations, in any one (but in one only) of which the student may compete—namely, in (1) Chemistry and Experimental Physics; (2) Zoology, with Comparative Anatomy and Physiology; (3) Botany, with Vegetable Anatomy and Physiology. The examinations will be held on the 2nd of June. Candidates must send in their names, stating which subject they elect to be examined in, to Dr. Drosier, Gonville and Caius College, Cambridge, on or before the 15th of May, enclosing a certificate of good conduct from a graduate of one of the British Universities.

The vacancy in the curatorship of the Leeds Philosophical and Literary Society has been filled by the appointment of Mr. Louis C. Miall, curator to the Bradford Philosophical Society. Mr. Miall is already favourably known in Leeds, through the very interesting and successful lectures on Geology which he delivered at the Philosophical Hall last year, and he is at present engaged in the delivery of a course of lectures on Botany at the same place. He is the author of various papers and publications on Botany and Geology, more especially as affecting the West Riding. Mr. Miall's candidature was strongly supported by Profs. Owen, Huxley, and Rolleston; and the Leeds Philosophical Society may be congratulated on his appointment.

We are unable this week to give any report of scientific proceedings in Paris: during the civil conflict every scientific movement is paralysed. The sittings of the Academy are suspended; but should the siege be prolonged, they may probably be resumed at Versailles, although some of its members are detained in Paris by the Commune as hostages.

WE are sorry to learn that the Government of Nova Scotia, in resorting to the retrenchment system, has withdrawn the small annual grant heretofore made to the Institute of Natural Science, the only scientific society that colony possesses; and, moreover, one which for the past eight years has struggled to maintain a position creditable to itself and the country in which it is established. Surely the trouble and expense of publishing, setting aside the gratuitous mental labour of those members who have furnished the interesting papers which together form the eight annual parts, comprising two volumes of over 1,000 pages, should have been considered by the authorities of the colony before they acted in such an illiberal spirit. We trust, nevertheless, to see the Institute still progress in its career of usefulness.

WE have great pleasure in announcing that Mr. Julian Goldsmid, M.P. for Rochester, who is a Master of Arts in the University of London, has just made his University a handsome present of 1,000*l*., to be paid in annual instalments distributed over ten years, towards the formation of a good classical library in the new building. The Senate have accepted the offer, with a hearty acknowledgment of its generosity; and a committee has already been appointed to begin the agreeable task of forming a classical library. We trust Mr. Goldsmid's generosity may be infectious. The *Spectator* suggests whether it is possible to secure for the University the late Prof. De Morgan's unique Mathematical Library, which probably contains the most curious collection of books on the history of mathematics to be found in England. The value of this collection is besides greatly enhanced by Mr. De Morgan's own numerous and characteristic annotations. Whether the library is to be disposed of or not, we do not at present know; but if it could be obtained, there would be a special fitness in securing it for the University of London, which would then have a really good start towards the formation of a fine classical and scientific library.

MR. ELWES, of University College, London, and Mr. Jude, of King's College, London, have been elected to Natural Science Scholarships at Christ's College, Cambridge, of the respective values of 70*l*. and 50*l*. per annum.

WE have to record another instance of American munificence towards Science. Vassar College has received a donation of 50,000 dollars to found a natural history professorship.

WE have received the first number of the 4th volume of "Sirius," edited by Dr. R. Falb, which promises to keep its old reputation as an excellent astronomical journal. It is accompanied by an admirable map of the stars of the northern hemisphere down to the 4th magnitude, the variable and double stars being indicated at a glance.

SEVENTY years ago some domestic rabbits were introduced upon Sable Island, a small sandy islet lying about a hundred miles off the Nova Scotia coast, and being left alone and not crossed in breeding, they have entered their feral state in liveries of beautiful silver grey, with white collars, intimating some remote affinities with bygone races.

AT the Natural History Society of Ireland, held on Wednesday, April 5th, R. P. Williams in the chair, Dr. A. W. Foot read a paper on "Irish Diptera." The list contained those species taken by him during the previous summer, and embraced sixty-five species belonging to thirty-nine genera. Mr. W. Andrews read a paper on the species of the genus Hymenophyllum met with in New Zealand. Some pretty sketches of scenery in New Zealand were exhibited by the author, who also laid on a table a number of illustrative dried specimens, which had been communicated to him by friends in America.

ON December 1 there was an earthquake at Tinnevelly in the Madrâs Presidency; and on the following day a slight earthquake was felt at Darjeeling, an English town in the Himalayas.

ON the 9th February there was a very strong shock of earthquake at Illapel in Chile. On the 11th February a strong shock of earthquake was felt at Valparaiso in Chile about 4 A.M. It is worthy of note that both these earthquakes occurred simultaneously with freshets from the mountains.

ON Twelfth Day an Indian woman of Cuzco in Peru, forty-five years of age, gave birth to a triplet of boys, one of whom survived. As this happened strangely enough on the day of the Three Kings, it excited the superstitions of the natives. The surviving boy was named Gaspar, after one of the kings.

COAL of excellent quality is said to have been discovered near the rich silver mines of Caracoles in Bolivia, about twelve miles from Calama.

IN a recent number of the *Scientific American* are short descriptions and drawings of two useful inventions:—a Washing-Shield, consisting of a corrugated shield or armour which protects the arm and at the same time forms an effective surface for rubbing the clothes; and an Anti-snoring device, consisting of a leather band placed over the head and chin, which effectually closes the mouth during sleep.

FROM the Thirteenth Annual Report of the East Kent Natural History Society we learn that that body is "in much the same condition as at the end of the preceding year." The library has been largely augmented; there has been but one excursion; several lectures have been given; and fortnightly evening meetings have been established. The Committee appointed to report upon the flora of the district seems to have made but little progress. The Rev. J. Mitchinson, D.C.L., is the President; and Mr. George Gulliver, F.R.S., Secretary.

THE Malvern Naturalists' Field Club has issued in a neat volume its Transactions for 1853-70. It contains a large number of papers mainly illustrative of the natural history of the district, including catalogues of local birds, mollusca, lepidoptera, and fungi, with sketches of the geology of the Malvern Hills, and observations on the meteorology of Malvern. There is also an interesting sketch of the proceedings of the Society from its commencement in 1853, to the close of 1868, by the Rev. W. J. Symonds, F.G.S., President; and a long paper on "The Forest and Chace of Malvern: its Ancient and Present State; with Notices of the most remarkable old Trees remaining within its Confines," by Mr. Edwin Lees. This is illustrated by several well-executed engravings of some of the trees referred to; but we can commend neither the illustrations nor the matter of the same author's paper "On the Forms and Persistency of Arboreal Fungi." The volume, however, on the whole, is likely to be useful to the members of the club, and is a creditable production.

THE more general use of buffaloes for the purpose of moving timber in some of the forests of India has been recommended by the conservators of those forests which are situate on comparatively level or uneven ground as being more practicable and economical than elephants. It is found that there is great difficulty in some parts in obtaining sufficient fodder for the elephants, one of the consequences of which is that their health suffers, and this, when taken with the high price paid for them, causes considerable loss. Owing to the size and weight of many of the logs, however, which often lie in difficult positions, a few elephants are necessary; but in those forests where low trucks and carts can be used, it is said that a few pairs of buffaloes will do more work, and can be kept with less risk than elephants.

THERE is a plant in New Granada which, if our ink-makers could only grow in sufficient quantity in this country, would be a fortune to them. The plant in question (*Coriaria thymifolia*) is commonly known as the ink plant, and it is simply the juice that is used without any preparation. Its properties seem, according to a tradition in the country, to have been discovered during the Spanish administration. A number of written documents destined for the mother country were embarked in a vessel, and transmitted round the Cape, the voyage was unusually tempestuous, and the documents got wetted with salt water, those written with common ink became nearly illegible, whereas those written with "chanchi" (the name of the juice) remained unaltered. A decree was therefore issued that all government communications should in future be written with the vegetable juice. The ink is of a reddish colour when freshly written, becoming perfectly black after a few hours, and it has the recommendation of not corroding a steel pen so readily as ordinary ink.

A NEW *Wellingtonia gigantea*, or "big tree," forty feet and four inches in diameter, has been discovered lately near Visalia, in Southern California. This is thicker by seven feet than any other that has yet been found. A section of one of the "big trees" is now exhibited in Cincinnati, which is seventy-six feet in circumference and fourteen feet high; and, standing on the floor of the hall, it gives one a perfectly clear idea of the enormous size of the tree from which it was taken. The section was cut last year in the Mariposa grove, about two hundred and fifty miles south-east of San Francisco, and far up the western slope of the Sierra Nevada mountains. It was divided and hauled a hundred and forty miles to Stockton, on three waggons by seventeen yoke of cattle.

UNDER the title of "British Pharmacology" Mr. W. W. Stoddart is publishing in the *Pharmaceutical Journal* some interesting papers upon some of the British plants which are employed in medicine. Speaking of the presence of allyl in the horseradish, he says, "It is a very singular fact that the cruciferous plants produce compounds of sulphur and allyl that are so well known in the genus Allium, plants so dissimilar in habit and construction as to be in both exogenous and endogenous divisions of the vegetable kingdom. In every part of the world the garlic flavour seems to be a favourite. The Israelites of old regretted the loss of their leeks and onions. The Englishman likes the addition of a shallot, mustard, or horse-radish to his beefsteak. The Spaniard selects the onion, and the Asiatic assafœtida. Even the Brazilian has chosen the petiveria and sequieria, both of which have an alliaceous flavour. The whole of these owe their smell and taste to allyl, which in the onion tribe exists as a sulphide."

A DEPOSIT of alum of considerable magnitude has been found in the Kulhu Valley, in Madras. It was first found by shepherds. As a rule, the headmen of villages prefer even now not to disclose mineral discoveries.

MR. BLANFORD, of the Indian Geological Department, has been specially appointed to proceed to Damagoodiam in the Central Provinces to examine and report on the coal discovered by Colonel Henry.

FAVOURABLE reports of the Cinchona cultivation in the West Indies continue to be received. We learn that in the Jamaica plantations the trees are seeding plentifully, and that about 100,000 seeds of *C. calisaya* are now ripening. There are also 40,000 seedling plants of *C. succirubra* raised from Jamaica seed. One hundred acres of land over and above the hundred acres already established, have lately been prepared for planting in the coming spring, and there appears every prospect of a few hundred more acres being soon prepared to be put under similar cultivation.

FROM Asia Minor we get no scientific records of weather, but in their absence some information of a meteorological character is useful. The winter in Smyrna has been very mild, accompanied with heavy rains, but apricots had shown fruit. On the 29th the rainy season culminated in a flood of the river Metes, inundating the city of Smyrna in a way not known for more than half a century, while the great river Hermus also overflowed. Soon after the weather changed to a sharp frost, which will cause destruction among orange and fruit trees. It will be observed the weather is the reverse of ours.

THE Government of Madras has appointed four scientific gentlemen to annalyse the water of that Presidency.

DR. HERMAN CREDNER, of Leipzig, in a forthcoming number of Petermann's *Mittheilungen*, presents a valuable report upon the geology and mineralogy of the Alleghany system of the United States, accompanied by a detailed map of the region.

NEWSPAPERS do indeed bring intelligence to men of Science, but they bring error to the vulgar. We had made a note from an Indian paper of a hen which had hatched a chicken perfectly resembling a young kid, and created consternation in a whole kingdom concerned to know what misfortunes are portended. We are the more inclined to notice this valuable contribution to natural science, because now we find in a Smyrna paper, *La Réforme*, intelligence that the island of Crete is now busied with the fact, "well authenticated," that a woman in the village of Melikos has been brought to bed of two monsters having the form of serpents. Although they only lived two hours, so that the medical men of the island could not, if they had the requisite capacity, make any observations, the "fact" may cost the unlucky Turkish Government another insurrection. Cock-and-bull stories may sometimes be dismissed with contempt, but want of instruction in natural science may in this country and some others be of more serious moment; for ignorance is seldom bliss, but one of the great promoters of evil. Knowledge of natural science may prevent national discouragement in some cases, and in others stifle the spread of conspiracy and rebellion. The Aurora Borealis has in the east been connected with the Menzikoff note and been commented on by it.

A HANDSOME consignment of silkworms' eggs has arrived at Sydney with the appliances of a "magnanarie" from Japan, which may prove the foundation of a branch of Industry in New South Wales that shall vie with the production of wool and excel that of sugar. The Acclimatisation and Agricultural Societies have been directing their attention to the subject, and some of the best varieties of silkworm have been acclimatised by Mr. Charles Brady. In June last the Acclimatisation Society wrote to Sir Henry Parkes at Yokohama for specimens of the finest varieties of worms, with specimens of silk, cocoons, &c., and a complete set of sericultural implements. With kind alacrity he set to work at once to oblige the society, and so far interested the ex-Minister of the Interior, Prince Daté, on the subject as to receive from him gratuitously nearly all that was wanted.

A LONG and interesting letter by Mr. T. J. Monk, on the Breeding and Preservation of the Woodcock in East Sussex, appears in the *Field* of Feb. 25. The writer gives an account of the occurrence of this bird in seven districts of East Sussex, comprising twenty-one parishes, in all of which woodcocks have nested, and are nesting in greater or less numbers every year. On an average, Mr. Monk considers that from a hundred and fifty to two hundred nests might be found in these districts in most years; and states his opinion that, if never shot at after Feb. 1, and if the coverts were kept as quiet and undisturbed as possible during the breeding season, we might hope for a still further increase in the number of young birds.

EXAMPLES OF THE PERFORMANCE OF THE ELECTRO-MAGNETIC ENGINE*

SOME experiments and conclusions I arrived at a quarter of a century ago ~~having been~~ recently criticised, I have thought it might be useful to place the subject of work in connexion with electro-magnetism in a different and I hope clearer form than that in which I have hitherto placed it. The numbers given below are derived from recent experiments.

Suppose an electro-magnetic engine to be furnished with fixed permanent steel magnets, and a bar of iron made to revolve between the poles of the steel magnets by reversing the current in its coil of wire. Such an arrangement is perhaps the most efficient, as it is the most simple form of the apparatus. In considering it, we will first suppose the battery to consist of 5 large Daniell's cells in series, so large that their resistance may be neglected. We will also suppose that the coil of wire on the revolving bar is made of a copper wire 389 feet long, and $\frac{1}{16}$ of an inch diameter, or offering a resistance to one BA unit. Then, on connecting the terminals of this wire with the battery, and keeping the engine still, the current through the wire will be such as, with a horizontal force of earth's magnetism 3·678, would be able to deflect the small needle of a galvanometer furnished with a single circle of one foot diameter, to the angle of 54°23′. Also this current going through the above wire for one hour will evolve heat that could raise 110·66 lbs. of water 1°, a quantity equal to 85430 ft. lbs. of work. In the meantime the zinc consumed in the battery will be 535·25 grains. Hence the work due to each grain of zinc is 159·6 ft. lbs., and heat ·20674 of a unit.

8·5 = 1 BA

(Hall)

I. In the condition of the engine being kept still we have therefore, current being 1·395, as shown by a deflection of 54° 23′,

1. Heat evolved per hour by the wire 110·66 units.
2. Consumpuion of zinc per hour 535·25 grains.
3. Heat due to 535·25 grains, 110·66 units.
4. Therefore the work per hour will be (110·66 -- 110·66) 772 = 0.
5. And the work per grain of zinc will be $\frac{0}{535·25} = 0$.

54°23′

= 1·396

II. If the engine be now started and kept by a proper load to a velocity which reduces the current to $\frac{2}{3}$, or ·9307, indicated by deflection 42° 57′, we shall have

1. Heat evolved per hour by the wire $110·66 \times \left\{\frac{2}{3}\right\}^2 =$ 49·18 units.
2. Consumption of zinc per hour $535·25 \times \frac{2}{3} = 356·83$ grains.
3. Heat due to 356·83 grains, $110·66 \times \frac{2}{3} = 73·77$ units.
4. Therefore the work per hour will be (73·77 − 49·18) 772 = 18983 ft. lbs.
5. And the work per grain of zinc will be $\frac{18983}{356·83} = 53·2$ or $\frac{1}{3}$ of the maximum.

·40857

057419

III. If the load be lesssened until the current is reduced to $\frac{1}{2}$ of the original amount, or to ·698, we shall have

1. Heat evolved per hour by the wire $110·66 \times \left(\frac{1}{2}\right)^2 =$ 27·665 units.
2. Consumption of zinc per hour $535·25 \times \frac{1}{2} = 267·62$ grains.
3. Heat due to 267·62 grains $110·66 \times \frac{1}{2} = 55·33$.
4. Therefore the work per hour will be (55·33 − 27·665) 772 = 21357.
5. And the work per grain of zinc will be $\frac{21357}{267·62} = 79·8$ or $\frac{1}{2}$ of the maximum duty.

IV. If the load be still further reduced and velocity increased so as to bring down the current to $\frac{1}{3}$ of what it was when the engine was still, or to ·4653, shown by a deflection of the galvanometer of 24° 57′, we shall have

1. Heat evolved per hour by the wire $110·66 \times \left(\frac{1}{3}\right)^2 =$ 12·294 units.
2. Consumption of zinc per hour $535·25 \times \frac{1}{3} = 178·42$ grains.
3. Heat due to 178·42 grains $110·66 \times \frac{1}{3} = 36·89$ units.
4. Therefore the work per hour will be (36·89 − 12·294) 772 = 18988 ft. lbs.
5. And the work per grain of zinc will be $\frac{18988}{178·42} = 106·4$ or $\frac{2}{3}$ of the maximum duty.

V. Remove the load still further until the velocity increases so much that the current is brought down to $\frac{1}{100}$ of its quantity when the engine is still. Then we shall have

1. Heat evolved per hour by the wire $110·66 \times \left(\frac{1}{100}\right)^2$ = ·011066 of a unit.
2. Consumption of zinc per hour $535·25 \times \frac{1}{100} = 5·3525$ grains.
3. Heat due to 5·3525 grains of zinc $110·66 \times \frac{1}{100} = 1·1066$ units.
4. Therefore the work per hour will (1·1066 − ·011066) 772 = 845·73 ft. lbs.
5. And the work per grain of zinc will be $\frac{845·73}{5·352} = 158$ or $\frac{99}{100}$ of the maximum duty.

When the velocity increases so that the current vanishes the duty = 159·6.

I. Let us now improve the engine by giving it a coil of 4 times the conductivity, which will be done by using a copper wire 389 feet long and $\frac{1}{8}$ of an inch diameter, the same battery being used as before. Then when the engine is kept still, we shall have a current 1·396 × 4 = 5·584, shown by a deflection of 79° 51′. Then we shall have

1. Heat evolved per hour by the wire $110·65 \times \frac{4^2}{4} =$ 442·64 units.
2. Consumption of zinc per hour 535·25 × 4 = 2141 grains.
3. Heat due to 2141 grains 442·64 units.
4. Therefore the work per hour will be (442·64 − 442·64) 772 = 0.
5. And the work per grain of zinc will be $\frac{0}{2141} = 0$

II. Start the engine with such a load as shall reduce the current to $\frac{2}{3}$, or to 3·7227 (74° 58′), then we shall have

1. Heat evolved per hour by the wire $442·64 \times \left(\frac{2}{3}\right)^2 =$ 196·73 units.
2. Consumption of zinc per hour $2141 \times \frac{2}{3} = 1427·3$ grains.
3. Heat due to 1427·3 grains $442·64 \times \frac{2}{3} = 295·09$ units.
4. Therefore the work per hour will be (295·09 − 196·73) 772 = 75934.

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* From the Proceedings of the Manchester Literary and Philosophical Society.

= 85430

[illegible] 85430 for [illegible] current

[illegible] ·53472 [illegible] ·40857

5. And the work per grain of zinc will be $\frac{75934}{1427{\cdot}3} = 53{\cdot}2$ or $\frac{1}{3}$ of the maximum duty.

III. Lessen the load so that the velocity of the engine is increased until the current is reduced to one half its original amount, or 2·792 shown on the galvanometer by a deflection of 70° 18′. Then we shall have

1. Heat evolved per hour by the wire $442.64 \times \left(\frac{1}{2}\right)^2 =$ 110·66 units.
2. Consumption of zinc per hour $2141 \times \frac{1}{2} = 1070{\cdot}5$ grains.
3. Heat due to 1070·5 grains, $442{\cdot}64 \times \frac{1}{2} = 221{\cdot}32$ units.
4. Therefore the work per hour will be $(221{\cdot}32 - 110{\cdot}66)\ 772 = 85430$ ft. lbs.
5. And the work per grain of zinc will be $\frac{85429}{1070{\cdot}5} = 79{\cdot}8$ or $\frac{1}{2}$ the maximum duty.

IV. Let the load be further reduced until the velocity reduces the current to $\frac{1}{3}$, or to 1·8613 shown by a deflection of 61° 45′. Then we shall have

1. Heat evolved per hour by the wire $442{\cdot}64 \times \left(\frac{1}{3}\right)^2 =$ 49·182 units.
2. Consumption of zinc per hour $2141 \times \frac{1}{3} = 713{\cdot}66$ grains.
3. Heat due to 713·66 grains of zinc $442{\cdot}64 \times \frac{1}{3} = 147{\cdot}55$ units.
4. Therefore the work per hour will be $(147\ 55 - 49{\cdot}182)\ 772 = 75940$ ft. lbs.
5. And the work per grain of zinc will be $\frac{75940}{713{\cdot}66} = 106{\cdot}4$ or $\frac{2}{3}$ of the maximum duty.

V. Let the load be still further reduced until, with the increased velocity, the current becomes reduced to $\frac{1}{100}$, or to ·05584 showing a deflection of 3° 12′. Then we shall have

1. Heat evolved per hour by the wire $442{\cdot}64 \times \left(\frac{1}{100}\right)^2 = {\cdot}044264$ of a unit.
2. Consumption of zinc per hour $2141 \times \frac{1}{100} = 21{\cdot}41$ grains.
3. Heat due to 21·41 grains of zinc $442{\cdot}64 \times \frac{1}{100} = 4{\cdot}4264$ units.
4. Therefore the work per hour will be $(4{\cdot}4264 - {\cdot}04426)\ 772 = 3383$ ft. lbs.
5. And the work per grain of zinc will be $\frac{3383}{21{\cdot}41} = 158$ or $\frac{99}{100}$ of the maximum duty.

Now suppose that we still further improve our engine by making the stationary magnets twice as powerful. In this case all the figures will remain exactly the same as before, the only difference being that the engine will only require to go at half the velocity in order to reduce the current to the same fraction of its first quantity. The attraction will be doubled, but the velocity being halved no change will take place in the amount of work given out.

In all cases, the maximum amount of work per hour is obtained when the engine is going at such a velocity as reduces the current to one half of its amount when the engine is held stationary; and in this case the duty per grain of zinc is one half of the theoretical maximum.

The same principles apply equally well when, instead of employing the machine as an engine evolving work, we do work on it by forcibly reversing the direction of its motion. Suppose for instance we urge it with this reverse velocity until the quantity of current is quadrupled, or becomes 22·386 indicated by a deflection 87° 26′. Then we shall have

1. Heat evolved per hour by the wire $442{\cdot}64 \times 4^2 =$ 7082·2 units.
2. Consumption of zinc per hour $2141 \times 4 = 8564$ grains.
3. Heat due to 8564 grains of zinc $442{\cdot}64 \times 4 = 1770{\cdot}56$ units.
4. Therefore the work per hour will be $(1770{\cdot}56 - 7082{\cdot}2)\ 772 = -4100432$ ft. lbs.
5. And the work per grain of zinc will be $\frac{-4100432}{8564} = -478{\cdot}8$ or -3 times the maximum working duty.

The principal reason why there has been greater scope for the improvenmt of the steam engine than for the electro-magnetic engine arises from the circumstance that in the formula $\frac{a-b}{a}$, applied to the steam engine by Thomson, in which *a* and *b* are the highest and lowest temperatures, these values are limited by practical difficulties. For *a* cannot easily be taken above 459°+374° =833° from absolute zero, since that temperature gives 12·425 atmospheres of pressure, nor can *b* be readily taken at less than the atmospheric temperature or 449° +60°=519°. Also there is much difficulty in preventing the escape of heat; whereas the insulation of electricity presents no difficulty,

I had arrived at the theory of the electro-magnetic engine in 1840, in which year I published a paper in the 4th Vol. of Sturgeon's Annals, demonstrating that there is "no variation in economy, whatever the arrangement of the conducting metal, or whatever the size of the battery." The experiments of that paper indicate 36 ft. lbs. as the maximum duty for a grain of zinc in a Wollaston battery. Multiplying this by 4 to bring it to the intensity of a Daniell's battery, we obtain 144 foot lbs. Here, as in the experiments in the paper on Mechanical Powers of Electro-Magnetism, Steam, and Horses, the actual duty is less than the theoretic; which is owing partly to the pulsatory nature of the current, and partly also to induced currents giving out heat in the substance of the iron cores of the electro-magnets; although these last were obviated as far as possible by using annealed tubes with slits down their sides.

J. P. JOULE

OBJECT TEACHING AND SCIENCE IN AMERICA

THE following article, reprinted from the *Scientific American*, will give some idea of the spirit in which the teaching of science is being pursued in the United States:—

"The public are beginning to be awakened to the fact that technical education is *the* education they require, being in accordance with the conditions of modern civilisation; and it is admitted that such technical education must be based upon a foundation of natural knowledge. The principles of the natural sciences must then, for the future, form an essential part of popular education; the only questions are, how far and in what manner are these sciences to be introduced? Whatever is to be the amount taught, educators are agreed that the first steps in natural science, or, in other words, in systematising natural knowledge, are to be taken as early as possible. Early impressions are the deepest, and every child before its school days is already an untrained student of nature. The foundations of technical education should, therefore, be laid in the primary school; but whether commenced thus early or not, the method will always be the same. The child must be encouraged and guided in its natural habits of observing, and it must be led to systematise its observations, connecting them together by a chain of reasoning into groups of related ideas. This method is simply that known as "object teaching;" and you may as well try to fly without wings, or to teach geography without maps or globes, as to teach natural science without objects and diagrams. There is not a teacher, nowadays, but has heard of this object teaching; there are hundreds who have tried to utilise it; there are "colleges" in which it is professedly taught as a system; and yet there seems to be no method applied to the inculcation of natural science more misunderstood than this, and no teaching in our schools, at present, more utterly destitute of good results. Ninety-nine out of a hundred who talk so glibly of object teaching forget that it is merely a method—a method that has for its end to inculcate knowledge; that this knowledge to be inculcated is the essential part of the lesson; and that a *thorough acquaintance with the subject must precede any application of this mere method of instruction.* To stand up and give a lesson upon a cat, without knowing the first principles of natural history, is simply to go through a farcical parody; and authorities who have no better conception of the purposes of object teaching than this, set the cart before the horse; or, rather they never hitch on their should-be-useful animal at all, but ride off upon this hobby, leaving the load of knowledge it was meant to draw standing in the ruts—where it has been standing, as Prof. Huxley admirably puts it, ever since the days of ancient Rome.

"It has been recently advocated that every public school should be supplied with a collection of objects to illustrate the fundamental facts of natural science. By all means let it be so; but let the first use to which these are put be to instruct the teachers themselves in what they will have to teach. Let them learn what there is in each object of educational value, and what are its worthless characters; let them recognise that no object is complete in itself, but is merely a part of a vast whole, and that their office is to lead the child to recognise its most important relations to other objects. In building up the edifice of knowledge, they must not use every rough stone indiscriminately, but they must teach the little builders to chip off the useless angles of selected pieces, and so shape them that every stone shall, at its proper time, fit into its proper place. If this be not done, the most instructive objects in the world will not raise its single line of substantial structure, but will rest upon the minds of the pupils as an unarranged heap of meaningless facts—facts which will not even be long remembered; and it is as well that they should not be, because utterly useless, being unconsolidated by any cement of reason.

"We fear that no better end is attained by, or can be hoped for from, object-teaching in our public schools, until, as we have said, the teachers themselves are thoroughly educated in the principles of natural science. To accomplish this, however, the ear of these who rule the teachers must be gained; and we raise the question whether the representatives of science should not have a voice in the management of our public school system? As object-teaching is a mere handmaid of science, is of use only to give scientific habits of thought, and to convey a knowledge of scientific facts, and is worthless without science, the public should see that its introduction into our schools be carried on under the advice of scientific experts, who shall direct what is best to be taught, and advise with the adepts in teaching how such knowledge may best be imparted. As a journal having the interests of science and education at heart, desiring to see science soundly popularised, and the masses made acquainted with its technical value, we make this suggestion, and furthermore ask: Is there any man of scientific attainments in the present Board of Education? Is there any scientific authortiy upon its general staff? And how many teachers favourably known to and having the confidence of the really scientific portion of the community are engaged in giving scientific instruction in our public schools?"

TRANSMISSION THROUGH PNEUMATIC TUBES*

THE writer having been employed in designing the extension of a pneumatic despatch line in which some heavy gradients were unavoidable, and it became necessary to ascertain by calculation the steepest gradient that could be employed so as to obtain a sufficient carrying capacity in the new section of the line under given conditions of engine power and of length. Almost every text-book and paper on the velocity of gases in pipes gave a different formula, and the author therefore found it necessary to attempt to construct a convenient expression for the speeds of carriers of given weight and friction, under various conditions of pressure, gradients, and dimensions of tube. The problem of a successful pneumatic system is simply this: To make a given quantity of air expand from one pressure to another in such a way as return a fair equivalent of the work expended in compressing it. It is obviously impossible to regain the full equivalent of the work, because the compression is attended with the liberation of heat, which is dissipated and practically lost. Therefore, in designing a pneumatic system, the first thing is to contrive means of compressing the air as economically as possible; and, in the second place, to get back the available mechanical effect stored up in the compressed air, irrespectively of the work employed in compressing and examining it. The writer considers that small pneumatic tubes may be worked more profitably than large ones. The great convenience of and practical facilities for working small letter-carrying tubes have been amply proved by the extensive systems already laid down in Paris, Berlin, London, and in other towns, as adjuncts to the telegraph services. Tubes of somewhat larger diameter would undoubtedly work satisfactorily. Even still larger tubes, if of moderate lengths, might also be found useful for a variety of special applications. But the author does not believe that a pneumatic line working through a long tunnel could, for passenger traffic, ever compete in point of economy with locomotive railways. A pneumatic railway is essentially a rope-railway. Its rope is elastic, it is true, but it is not light. Every yard run of it, in a tunnel large enough to carry passengers, would weigh more than ¼ cwt. And it is a rope, too, which has to be moved against considerable friction, and in being compressed and moved wastes power by its liberation of heat. In a pneumatic tunnel, such as that proposed between England and France, in order to move a goods train of 250 tons through at the rate of twenty-five miles an hour, it would be necessary to employ simultaneously a pressure of 1½lb. per square inch at one end, and a vacuum of 1½lb. per square inch at the other. The mechanical effect obtained with these combined—pressure and vacuum—would be consumed as follows:—

In accelerating the air . .	29	millions of foot pounds.
In accelerating the train .	12	
By friction of the air . .	5721	
By friction of the train .	330	

The resistance of the air, therefore, upon the walls of the tunnel would alone amount to 93 per cent. of the total mechanical effect employable for the transmission; while the really useful work would be only about 5½ per cent. of it. And to compress and exhaust the air to supply these items of expenditure of mechanical effect, engines would have to exert over 2,000 horse power at each end during the transmission, even on the supposition that the blowing machinery returned an equivalent of mechanical effect such as has never yet been obtained. This would not be an economical way of burning coals.

ROBERT SABINE

SCIENTIFIC SERIALS

Silliman's Journal, January, 1871. The opening article in this number is by Prof. J. D. Dana, "On the Quaternary or Post-tertiary of the New Haven Region," in which he proves from numerous observations that the glacial era in this district was an era of glaciers and not of icebergs, many evidences of glacier action being visible in the form of broad furrows from eight to ten inches in depth, and extending for long distances on beds of trap and granite.—The second paper is by Prof. W. A. Norton, "On the Corona seen in Total Eclipses of the Sun." The author attributes this phenomena to a solar aurora, but the observations on the recent eclipse will probably induce him to modify his opinions; his arguments being based on the observations made during previous eclipses, when the corona was not made such a special object of investigation. He lays great stress on the long streamers as indications of auroral action, though it now appears that these streamers are not so decidedly of solar origin as was supposed.—In a letter to Dr. W. Gibbs, Mr. O. N. Rood gives a short account of some experiments to determine the duration of lightning flashes. A cardboard disc, with fifteen narrow and radial apertures, was caused to rotate very rapidly on a pin. Occasionally, during a flash, the slits in the cardboard were seen distinctly as if the disc were stationary, but more usually they were distinctly elongated. From the observations made it would seem that the duration of the flashes was about $\frac{1}{500}$ of a second. The accuracy of this result may perhaps be rendered doubtful from the fact that both Becquerel and Faraday have noticed that gases are rendered slightly phosphorescent by electrical discharges.—Dr. A. M. Mayer contributes an article "On the physical condition of a closed circuit contiguous to a permanent and constant Voltaic current; or, on the electro-tonic state." The author commences by giving extracts from Faraday's investigations, in which he uses the term electro-tonic state to indicate the condition of a wire in which an electric wave has been induced by the proximity of a conductor through which a constant current was passing. He has endeavoured to obtain some clue of the condition of such a closed circuit by passing through it another electric wave of a constant intensity, and which he ingeniously generated by slipping a flat spiral from the end of a permanent magnet, as described in the number of this journal for November last. Currents thus obtained are found, by means of a delicate reflecting galvanometer, to be practically of the same intensity; for on repeating the experiment several times this produced deflections differing from one another to an extent not greater than 20″. In this manner it was determined that a definite electric current, traversing a metallic circuit in proximity to another traversed by a powerful voltaic current, has the same intensity, whether passed in the same direction as the latter or in a direction opposed to it. The author thinks, however, that a diminution in the velocity of the current ensues, and he intends to continue his experiments in order to settle this question.—This paper is followed by an abstract of the "Programme for the Observation of Stars of the Ninth Magnitude, undertaken by the German Astronomical Society; an analysis of gahnite from Mine Hill, Franklin Furnace, New Jersey, by G. J. Brush; and an account of the Observations of the Meteors of November 13 and 14, 1870."—The next article is by Prof. J. Le Conte, "On some Phenomena of Binocular Vision," in continuation of some previous papers. For examining the effects produced on observing objects with both eyes, the author employs a white plane about twenty inches long and of any convenient width. A notch is cut at one end of the board to enable the operator to place the plane just below the level of the eyes, the notch fitting on the bridge of the nose. By sticking pins on different parts of the board and drawing lines between the pins and the eyes, the phenomena of vision can be investigated. The author points out that when things are looked at with both eyes, the eyes themselves seem to double, two of them combining to form a binocular eye in the middle which looks out between two noses, while the other two are on either side beyond the noses. This article is well worth perusal—The next paper is by Sidney I. Smith, "On a Fossil Insect from the Carboniferous Formation of Indiana," and is illustrated by a woodcut representing a wing 2·54 inches long and 0·85 broad, found in the grit quarry near Paoli, Orange County, Indiana, in cutting the stone for making whetstones.—Observations on the Earthquake of October 20th in North-eastern America have been collected by Mr. A. C. Twining. The area of disturbance extended from New Brunswick in the East to the State of Iowa in the West, and from the lakes of the River St. Lawrence in the North to Cincinnati and Richmond, Va., in the opposite direction. The shock travelled from about E 6° N. to about W. 6° S. at the rate of 160 miles in a minute.—The concluding article is by Professor A. E. Verrill, "On some imperfectly known and new Asceidians from New England."

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* Abstract of a paper read at the Liverpool Meeting of the British Association.

Silliman's Journal, February, 1871. This number opens with a paper by Dr. A. M. Mayer, "On Observations on the Variation of the Magnetic Declination in connection with the Aurora of October 14, 1870, with Remarks on the physical connection between changes in area of disturbed solar surface and magnetic perturbations" The aurora was first noticed at 6.30 P.M. on October 14, and the magnetic observations commenced at 6.35 P.M. and were continued till 10 P.M. The mean declination being represented by 0°, at 6.35 P.M. the declination was 5′·70 W. and at 6.56 P.M. 18′·37 W. The magnet then rapidly moved towards the E., and at 7.46 the declination was 21′·94 E. The motion now became westerly, and at 7.57½ P.M. the reading was 32′·19 W. There was next a rapid easterly movement till 8.5½ P.M., when the reading was 10′·42 E., deep rough streamers flashing up in the N.N.W. There was another deviation to 4′·55 W. at 8.10 P.M., after which the needle passed to the east of the neutral line, and, after several oscillations, reached the maximum easterly deviation of 22′·52 E. at 9.10 P.M. The author makes daily observations of the spots on the sun, and remarks on the connection existing between their appearance and magnetic disturbances. He points out the necessity of establishing several stations, where daily photographs of the sun may be taken.—The next communication is the first part of a series of "Notes on Granite Rocks," by T. Sterry Hurst, LL.D., F.R.S. This paper is continued in the numbers of the Journal for March, and its extraction may perhaps be more conveniently deferred until the whole of it has appeared.—Mr. E. D. Cope contributes a short note on "Siredon Metamorphoses."—This is followed by a note by Professor G. B. Andrews, "On Lower Carboniferous Limestones in Ohio," and the conclusion of Professor Verrill's "Descriptions of Ascidians from New England."—This number also contains a "Memoir of Professor Graham, by Professor J. N. Cooke, and a "Description of the Auroral belt of October 24-25, as observed at New Haven.

SOCIETIES AND ACADEMIES

LONDON

Royal Institution of Great Britain, April 3.—Mr. Warren De la Rue, F.R.S., vice-president, in the chair. Mr. William Cubitt, Mr. William Gould, Mr. Robert Hannah, Mr. John Henry Mackenzie, Rev. John Macnaught, M.A., Mr. Joseph Reynolds Masters, Mr. George Borwick Robertson, F.C.S., and Mr. George Wilson, were elected members of the Royal Institution. The special thanks of the members were returned for the following donation to "The Fund for the Promotion of Experimental Researches:"—Mr. T. Williams Helps (6th donation), 10*l*.

Entomological Society, April 3. Mr. A. R. Wallace, president, in the chair.—The Secretary exhibited drawings of Chinese Lepidoptera, beautifully executed by Mr. Holdsworth, of Shanghai.—Mr. F. Smith exhibited several examples of gynandromorphous Aculeata Hymenoptera, including *Anthophora acervorum*, *Andrena nitida*, *Nomada baccata*, and *Apis mellifica*, the latter combining the characters of male and workers. Mr. Lewis referred to the damage occasioned to books by *Lepisma saccharina*, which fed upon the paste of the bindings, and caused them to fall to pieces. Mr. Lewis also read a paper on the arrangement of British Lepidoptera, as adopted by various authors and compilers of catalogues, and he deprecated very strongly the constant changes in arrangement and nomenclature that are being made, the writers in many cases failing to give any reasons for such changes.

Anthropological Institute, April 3.—Sir John Lubbock, Bart., president, in the chair. Messrs. F. W. Rudler and Archibald Hamilton were elected members. Mr. Boyd Dawkins, read a paper "On the results obtained by the Settle Cave Exploration Committee." Victoria Cave, near Settle, in Yorkshire, is situated half-way up a cliff 200 feet high, and consists of a series of large chambers and passages, and has from time to time furnished to its explorer, Mr. Jackson, a remarkable collection of ornaments and implements of bronze, iron, and bone, along with pottery and broken remains of various animals, viz., red-deer, roebuck, pig, horse, sheep, goat, badger, fox, and dog. Fragments of Samian ware and coins of Trajan prove that the stratum in which they were discovered was accumulated subsequently to the Roman invasion. The numerous articles and bones were described in full detail. The author concluded that the cave was first inhabited by a barbarous Neolithic family, and, lastly, after a long interval, by Roman provincials, or possibly their descendants fleeing away from the arms of an invader.—A paper by Dr. Barnard Davis, F.R.S., was read, "On some recent Anatomical Writings bearing upon Anthropology, by Prof. Luigi Calori, of Bologna;" principally on the magnificent memoir of that anatomist, "Del Cervello, nei due tipi Brachicefalo e Dolicocefalo Italiani," lately published. That work is divided into four parts:—1. On the figure of the Brain in the two types. 2. The cerebral convolutions; their various aspect and their variety or anomalies. 3. On the weight of the brain in the two Italian types, brachycephalic and dolichocephalic. 4. On the extension of the cerebral superficies in the two Italian types. The second article goes into a thorough examination of the cerebral convolutions and the varieties they present.—Mr. A. L. Lewis contributes a paper "On the builders of the Megalithic Monuments of Britain." A special general meeting of the members of the Institute was held previously for the purpose of adopting the regulations proposed by the Council. Trustees of the Institute were elected, viz., Sir John Lubbock, Bart., president; J. W. Flower, treasurer; and the Rev. Dunbar J. Heath, M.A.

Zoological Society, April 4.—Mr. R. Hudson in the chair. Mr. W. Saville Kent read a paper on some new or little known Madrepores contained in the collection of the British Museum, amongst which were new species of the genera *Acanthocyathus*, *Flabellum*, *Stylaster*, and *Allopora*.—A communication was read from Surgeon Francis Day containing a series of notes on some of the rarer and less-known of the Siluroid fishes of India.—A communication was read from Mr. G. Stewardson Brady containing a review of the known *Cypridinidæ* of the European seas, together with a description of a new species of the genus *Philomedes*, proposed to be called *Ph. folinii*.—A communication was read from Dr. J. E. Gray containing additional notes on *Rhinoclemmys mexicana*, a Mexican Tortoise recently described by him in the Society's "Proceedings."—A second communication from Dr. Gray contained some additional notes on the genera *Eupleres* and *Galidia*, and a note on *Lemur ruber*, founded on specimens of these animals lately procured in Madagascar by Mr. Crossley.

Linnean Society, April 6.—Mr. G. Bentham, president, in the chair. The president read a paper on the styles and stigmas of Proteaceæ. In plants belonging to this order, the anthers mostly discharge their pollen before the expansion of the flower, as is also the case in Compositæ. In this latter order self-fertilisation is prevented by the fact that the stigmas are on the under face of the style branches which remain hermetically closed until after the opening of the flower. In Proteaceæ, on the other hand, the style is undivided, and the stigmatic surface is exposed even in the bud; and the contrivances to screen it from the action of the pollen of its own flower are very various. The observations have been made, with but few exceptions, on dried plants, and require confirming or rectifying by those who have the opportunity of examining the living plants. The anthers generally form, as in Compositæ, a closed tube surrounding the stigma, which is, however, usually if not always immature at the time of the extension of the pollen. That self-fertilisation seldom if ever takes place is shown by the fact that in no genus is the style more completely smothered with pollen than in *Banksia*, while there is none in which fecundated fruit is rarer. In *Petrophila* the stigmatic surface consists of the minute tip of the style, which is, however, completely protected while in the bud from the action of the pollen by the perianth segments. In one section of *Persoonia* the style is completely turned away from the anthers, and the stigmatic point is buried in a pouch prepared to receive it. The anther immediately over this pouch is sometimes destitute of pollen. In *Banksia* and *Grevillea* there appears to be no protection against self-fertilisation except the immaturity of the stigma. In *Conospermum* and *Synaphea* one anther in each flower has two perfect cells, separated by a connective; in the two lateral stamens there is one perfect and one abortive cell; and in the fourth there are two abortive cells, the cells are open cups; the perfect one of each lateral anther applied face to face with the adjoining cell of the double anther forms with it a closed globe in the bud, opening as the flower expands so as to let fall the pollen, the style projects far beyond with the stigmatic surface bent towards the upper perianth segment in the bud. In *Conospermum*, where the upper anther has two perfect cells, and the lowest is abortive, the style recurves elastically as the flower opens, so as to direct the stigma towards the abortive anther; in *Synaphea*, where the upper anther is abortive and the lowest perfect, the stigma is retained in its primitive position over the abortive anther by a strapshaped appendage proceeding from the rudiment of that anther and firmly attached to the lower margin of the stigmatic disc.—"On the Generic Nomenclature of Lepidoptera." By J. D. Crotch.

Chemical Society, March 30.—Prof. Williamson, F.R.S., president, in the chair. The president delivered the following address:—"Gentlemen, I feel much pleasure in congratulating you on the rapidly increasing prosperity of our society, and the enlargement which has taken place in its sphere of usefulness. For on the one hand the number of our fellows continues to show a most satisfactory increase, and on the other hand your Council has made arrangements for carrying out the system of monthly reports, which has been for some time in contemplation. It was hoped that the Chemical Society of Paris might, from the first, co-operate with us in the preparation of these monthly reports, but circumstances beyond their control have prevented the sister society from joining us in the beginning of this year. Deeming it undesirable to delay the commencement of the reports, your Council still look forward to the future co-operation of the Paris Society in their preparation. You are aware that the present available income of the society was not considered to be sufficient to defray the additional expense of writing and printing these reports, and I have the pleasure of informing you that contributions to the extent of 1,175*l.* have been promised by members of your body towards supplying the defect during the first five years of the appearance of the reports. The British Association has moreover granted us the sum of 100*l.* for this year in aid of the undertaking. We hope that in five years the funds of the society may have sufficiently increased to enable us to pay the whole expense of the reports, and that their publication will be valued by the members of our society, and promote the advancement of our science wherever the English language is read."

Edinburgh

Royal Physical Society, March 22.—Mr. W. C. Peach, President, in the chair. Note on Carbon, showing ligneous Structure in Coal (with illustrative diagrams). By Professor Duns.—Mr. A. Taylor exhibited some undescribed Fossils from the Gilmerton Coalfield.—Remarks on some Japanese Skulls. By Dr. John Kennedy.—On Successive Glacial Periods, caused by Changes in Physical Geography. By Mr. Andrew Taylor.—Mr. C. W. Peach exhibited and described a few Zoophytes and Algæ, gathered on the shores by Port Phillip, near Melbourne.—Mr. Peach had to enter on a new field, and unfortunately had no works either on Australian Zoophytes or Algæ, beyond Busk's excellent catalogue of the Zoophytes contained in the British Museum, and part of a paper of Professor Wyville Thomson on some from Australia. He therefore regretted that his paper would come before them in an imperfect state. He first noticed several species of *Catenicella*—large masses of all were exhibited, as well as small portions on black paper prepared for the microscope—showing how luxuriantly these delicate animals built up their lovely homes. This genus is found plentifully on the shores of Africa, New Zealand, and Australia, but not in British seas, evidently being confined to warmer climates. He also introduced to the members several species of *Salicornaria*, *Cellularia*, *Menipea*, *Scrupocellaria*, *Ætua*, *Bicellaria*, *Retepora*, *Flustra*, *Lepralia*, *Cresia*, *Crisidia*, *Serialaria*—altogether more than twenty species.

Glasgow

Geological Society, February 2.—Mr. J. Young, V.P., in the chair.—Mr. Robert Craig read a paper "On the Boulders found in Cuttings on the Beith Branch Railway." The line of railway referred to runs nearly south-east from Beith. The striations upon the glaciated rock-surfaces of the district have a general bearing of nearly N.E. to S.W.; the line accordingly at its western terminus crosses them almost at right angles. The cuttings run nearly parallel to the southern termination of the range of trap hills which extends from Gleniffer to Beith, and at the distance of little more than a mile from it. The Carboniferous strata crop out along the southern boundary of this trap range, and consequently about a mile to the north of the railway. In the trap range four well-marked varieties of porphyrite occur, which, with the easily-distinguished beds of the Carboniferous limestone, gave the geologist an opportunity of classifying the boulders and tracing them to their source with an exactitude not always attainable. Mr. Craig then read a table giving the percentage of the different kinds of boulders found in eight cuttings proceeding eastward from Beith, and showed that the changes which were observable in them always corresponded with the rocks to the north-east of the cuttings. This strictly local character of the boulder-clay he thought was strong proof that it was due to land-ice—that, in fact, it had been taken up and deposited as the glacial "foot-board moraine." Droppings of sea-ice would have consisted much more largely of rocks from a distance. A small percentage of travelled rocks undoubtedly occurred in the boulder-clay of the district; and this, it must be noted, in all parts of the deposit—at bottom, middle, and top. These erratics he supposed to have been dropped from time to time through fissures and crevasses of the ice during its progress. From some sections in which he had followed the direction of the ice-stream, he found there was a change in the boulder-clay every three to five miles, less or more according to the roughness or evenness of the ground. He recommended a more minute examination and comparison of the boulder-clays of different localities than had yet been effected.

March 2.—Mr. John Young, vice-president, in the chair. Mr. E. A. Wünsch read a paper on a section of the northern shore of Arran, giving an account of some transported blocks of limestones which he had observed there during the previous summer. After describing the remarkable succession of deposits which had made that part of Arran classic ground for the geologist, he referred more particularly to a characteristic bed of limestone found near the Salt Pans, on the north-eastern shore of the island. This limestone is of a deep red colour, and is full of the shells of *Productæ*—especially *Producta latissima*—together with fragments of *Encrinites* and other organisms. The bed is very regularly jointed, and breaks up into beautiful cubical masses.—Mr. James Thomson read a paper on the occurrence of *Stigmaria stellata* (Eichwald) in the lower Carboniferous series, at Wildshaw, in the Upper Ward of Lanarkshire. He described the position of the strata in which these plant-remains had been found—in beds of fire-clay and indurated sandstone, capping those thin bands of limestone which characterise the lower members of the Carboniferous series in Scotland. The section presented at Wildshaw was as under:—6 ft. calcareous shale; 3 ft. hard light greyish sandstone; 2 ft. fire-clay, containing portions of *Stigmaria*; 3 ft. highly indurated sandstone, or chertzite, into which the *Stigmaria* roots passed; 11 ft. thin banded limestone, with partings of calcareous shale interstratified. Mr. Thomson mentioned that remains of this variety of *Stigmaria* were of comparatively rare occurrence in the Scottish coal fields—he had only found them in two other localities, and always in the same geological horizon. He pointed out that *Stigmaria* were now generally admitted to be the roots of the *Sigillaria*, but in this case he found them associated and in contact with what some authorities had called *Norea taxina* (?). The specimens on the table presented very distinct stellate markings which had surrounded the rootlets at the base of attachment.

BERLIN

Royal Prussian Academy of Science, August 11, 1870.—A paper was read by Dr. Hugo Kroeker, on the law of the exhaustion of muscles.—Prof. W. Peters communicated descriptions of some new reptiles and Batrachia, including *Hemidactylus muriceus* from Guinea, *Cercosaura glabella* from Brazil, forming the type of a new subgenus *Urosaura*, *Tropidolepisma striolatum*, from N.E. Australia, *Geophis annulatus* from South America (?) *Uriechis lineatus* from Guinea, forming the type of a new subgenus *Metopophis*, *Scaphiophis albopunctatus* from Guinea, the type of a new genus allied to *Zamenis*, and *Hotlocephalus frenatus* from N.E. Australia. The Batrachia described include the types of two new genera, namely, *Entomoglossus pustulatus* and *Cophomantis punctillata*, both from Brazil; the other species are *Rana longirostris* from Guinea, *Cystignathus diplolistris*, *Hylodes Henselii*, and *H. rugulosus* from Brazil, *Arthroleptis dispar* from Prince's Island, and *Phyllobates verruculatus* from Mexico. Figures of most of the species accompany the paper. Prof. Braun presented a most elaborate memoir on the genera *Marsilia* and *Pilularia*, containing a tabular synopsis, and full synonymic and distributional revision of the species.

October 10.—The only scientific papers read were two on subjects connected with the higher mathematics, by MM. Kennmer and Schwarz.

November 3.—Prof. Gustav Rose communicated some notes on the fall of a meteorite at Murzuk, in Fezzan.

November 27.—Prof. Dove presented a paper on the annual distribution of rain in central Europe.

December 1.—M. Kummer read a memoir on a peculiarity of the unities of the complex numbers obtained from the roots of the equation $\alpha^{\lambda} = 1$; and M. Kronecker appended to this paper a further note on a part of the subject treated of by M. Kummer.

December 5.—Prof. Reichert read a continuation of his memoir on the Skeleton of the Vertebrata, relating principally to the Myxinoidæ, Leptocephalidæ, and Cartilaginous Ganoids, *Protopterus anguilliformis* and the *Chimæræ*.

December 15.—Prof. Roth read a paper on the Theory of Metamorphism and the production of the crystalline slates.—A memoir on the principal tangential curves of the Kummerian surface of the fourth degree, with sixteen nodal points, by Dr. F. Klein and Dr. S. Lie, was communicated by Prof. Kummer.

December 22.—Prof. W. Peters communicated a monographic revision of the Chiropterous genera *Nycteris* and *Atalapha*. The author reunites to the genus *Nycteris*, originally established by Geoffroy Saint Hilaire, the groups *Nycterops* and *Petalia*, which have been separated as distinct genera by Dr. Gray; he also remarks that many unnecessary species have been established in it. He describes and gives the synonymy of 9 species and figures the typical form (*N. Hispida*) and the lower dentition of all of them. Two new species are described, namely *N. angolensis* and *N. damarensis*. The species of the genus *Atalapha*, which figure in the works of various authors under the genera *Scotophilus*, *Lasiurus*, and *Nycticejus*, are very difficult to discriminate satisfactorily; Prof. Peters recognises 11, of which 3 are described as new, namely, *A. Frantzii*, *A pallescens*, and *A egregia*. All the species are American, and the type is *A novæboracensis*.

January 9 and 12, 1871.—M. Kronecker read two mathematical papers, of which no details are given.

January 19.—Prof. Ehrenberg communicated a review of the investigations made since 1847 on the abundant organic life borne invisibly by the atmosphere, as a supplement to his former memoirs on trade-wind, dust, and blood-rain. Since 1847 no less than 186 cases of the occurrence of these phenomena have been observed, and 42 samples have been submitted by the author to examination. He considers that the results of these investigations bear out his former conclusions,—In connection with this Prof. Dove communicated some observations on the "Fohn" observed at Trogen on the 13th February, 1870

VIENNA

Imperial Academy of Sciences, January 12.—Prof. Hlasiwetz communicated a memoir by Dr. E. von Sommaruga on naphthylpurpuric acid and its derivatives. This acid is produced from dinitronaphthole, by the action of cyanide of potassium; it is incapable of its being isolated from its salts. When the latter are prepared in aqueous solution, a blue compound is formed simultaneously. This was first observed by Hlasiwetz, and the author named it indophane. In alcoholic solutions it is not produced.

January 19.—Prof. L. Pfaunden presented a memoir on the elementary deduction of the fundamental equation of the dynamical gas theory; and Prof. A. Lieben communicated the result of an investigation made by himself in conjunction with M. Rossi, upon formaldhyde and its conversion into methylic-alcohol. The author found that the product of the dry distillation of formiate of lime (formaldehyde) was converted into methylic alcohol by nascent hydrogen.

February 3.—A paper on the barytes of the ferriferous Lower Silurian and Carboniferous of Bohemia, and on baryte in general, by M. R. Helmhacker, and one on the increase of curvature of an oblique section of a surface, by D. K. Exner were read.—Mr. Karl Fritsch presented a comparison of the time of the flowering of the plants of North America and Europe, from which it appears that the lines of simultaneous flowering lie 5°—10° further south in America than in Europe. Elevation seems to have comparatively little influence.—A note from M. Max Schaffner was read, describing a method of obtaining thallium on a large scale from the dust produced during the roasting of iron pyrites.—Prof. Brucke communicated a new method of separating dextrine and glycogen from animal fluids and tissues.—Prof. Stefan presented a memoir on the influence of heat on the refraction of light in solid bodies, containing a series of determinations of the refraction of rock-salt, sylvine (perchloride of potassium), alum, fluorspar, and glass, at temperatures of 12°—94° C. (53°·6—201°·2 F.). The refractive power of the first four bodies decreases uniformly, and for all parts of the spectrum, with the increase of temperature; the refractive power of glass increases with the temperature, and the increase becomes greater in passing from the red to the violet end of the spectrum. The alterations calculated for the line *D*, and an elevation of temperature of 100° C are:—

Rock-salt	−0·00373
Sylvine	−0·00345
Alum	−0·00134
Fluorspar	−0·00123
Glass	+0·00023

M. K. von Littrow communicated a memoir by M. L. Schulhof, on the determination of the orbit of the planet (108) Hecuba.—Prof. Hlasiwetz briefly communicated the results of a nearly complete investigation of Liebig's Extract of Meat made in his laboratory by Dr. J. Weidler. This extract is found to contain a previously unknown nitrogenous compound, having the formula

$C_7 H_8 N_4 O_3$, and therefore most intimately allied to theobromine and caffeine.—M. Tschermak presented a paper containing observations on a meteoric iron from the desert of Atacama in Chili. It is a large shield-shaped mass, weighing 103 pounds, and when broken not only shows the usual figures after treatment with acid, but even before the application of acid thin lamellæ of triolite are recognisable, inserted parallel to the hexahedral surfaces and interrupting the octahedral texture. A similar phenomenon is presented by a meteoric iron from Jewell Hill, Madison County, North Carolina.

February 9.—A memoir by Prof. Linnemann, entitled "A Contribution to the further knowledge of Pinakone," was read, and its author claimed the priority in the discovery that formaldehyde is produced by the dry distillation of formiate of lime, and that from it methylic alcohol and other compounds may be obtained. A memoir on the employment of an electrometer for the stroboscopic determination of the elevation of notes, by M. A. von Obermayer, was read.—A paper by Prof. A. Weiss, on the structure and nature of the Diatomaceæ, was communicated. The author stated that the silica of the Diatomacean frustule polarises light, that the Diatomaceæ are composed of innumerable, minute, but perfectly individualised, cells, and that it is to these that the markings of the silicious shells are due.—Dr. E. Klein presented a memoir on the median germ lamella, and its relation to the development of the first blood-vessels and blood corpuscles in the embryo of the fowl, and communicated a paper on the finer nerves of the vaginal mucous membrane, by Dr. A. Chrschtschonovitsch of Kasan.—Dr. A. Schrauf presented a second series of his mineralogical observations, in which he noticed certain forms of crystals of gypsum, crystals of argentite, the properties and paragenetic relations of the Azorean azorite and pyrrhite, a new mineral from Leadhills, to which he gave the name of eosite, and the characters of vanadite, dechenite, and descloizite.—Dr. S. Stern read a memoir on the theory of the resonance of solid bodies, with reference to the accompanying vibration of the air; and Prof. Reuss presented the first of a series of memoirs by Dr. Manzoni, on Mediterranean Bryozoa. In this the author notices sixteen species (one *Hippothoa*, one *Membraniporæ*, and fourteen *Lepraliæ*).

February 16.—A memoir by Dr. U. R. von Jepharovich, on diaphorite and freieslebenite was read. The author stated that two species, one monoclinic, the other rhombic, have been included under the name of freieslebenite. Their composition is identical, but they differ in density. For the rhombic species he proposes the name of diaphorite.—A memoir was also read on the theory of gases by Prof. L. Boltzmann.—Prof. Reuss communicated a memoir on some fossil star-fishes from the Rhenish grauwacke, by Dr. S. Simonowitsch. Four new species were described, namely, *Asterias acuminatus*, *Aspidosoma petaloides*, and two species forming a new genus, *Xenaster*, *X. margaritatus* and *simplex*.—Prof. von Oppolzer reported upon the calculations undertaken by him for the re-discovery of the lost planet (62) Erato.—Prof. F. Simony made some remarks on the Lacustrine erosion of shore-rocks belonging to various limestone formations.

I. R. Geological Institution, March 28.—Theodor Petersen read a paper on "Cœruleolactin." By this name he designates a new phosphatic mineral, which has been found in the mine of Rindsberg, near Katzenellenbogen (Nassau), in a layer of brown iron ore. It must be placed between Kolaite and Wawellite. The specific gravity is 2·59, the hardness 5.—Variscite. This mineral described by Breithaupt from Plauin had never been analysed. Petersen determined its sp. gr. to be 2·408.—Diabase from Nassau. Exact inquiries have proved that diabase very often contains small quantities of metallic compounds, and is probably the original source of different strata of ores. The felspar in diabase is usually oligoclase, and not as had been generally supposed labradorite.—F. Karrer and Th. Fuchs on the "Relations between the different strata of the marine deposits of the Miocene Vienna Basin." From many new sections which they obtained along the aqueduct now in construction between Vienna and Gloggnitz, the authors endeavour to show that the clay of Baden and that of Geinfahrn, the sandstones of Pœtzleinsdorf, the limestones (leithakalk), &c., are not deposits of different geological ages, as had hitherto been generally supposed, but represent different facies of the same age, and like the zones of living organisms in the seas of our day, pass into each other without any exact limit.—M. F. Posepny spoke of the penetration of Klastic masses through eruptive or sedimentary rocks. The so-called Glam in the Transylvanian mining districts is an evident example of this phenomenon, and may be compared with the dowkies in N.W. England, and the Gangthonschiefer in the mines of the Harz. An exact study of the phenomenon showed that it originated from very different causes. Sometimes the klastic masses were formed by mechanical friction, in other cases they have been successively deposited by water, sometimes they are masses of mud and pebbles, which penetrated in open veins or cavities of the rock.

BOOKS RECEIVED

English.—British Insects; their Form, Structure, and Habits: E. F. Staveley (L. Reeve and Co.)—On Intelligence. H. Paine, translated by T. D. Haye, part I. (L. Reeve and Co.)—The Bijou Gazetteer of the World: W. H. Rosser (Warne and Co.)

American and Foreign.—A Synopsis of the Family Unionidæ: Dr. Isaac Lea. New York—(Through Williams and Norgate)—Lehrbuch der Sparischen Astronomie: Dr. F. Brunnow—Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, tome XX.

DIARY

THURSDAY, April 13.

Mathematical Society, at 8.—On Diagrams of the Stresses in Warren and Lattice Girders. Prof. Crofton, F.R.S.—On Quartic Surfaces: Prof. Cayley, F.R.S.

FRIDAY, April 14.

Astronomical Society, at 8.
Quekett Microscopical Club, at 8.

SATURDAY, April 15.

Royal School of Mines, at 8.—Geology: Dr. Cobbold.

MONDAY, April 17.

Anthropological Institute, at 8.—The Position of the Australian Languages: Dr. W. H. J. Bleek.—Comparative Table of the Australian Languages: Rev. G. Taplin.—Mental Characteristics of Primitive Man as exhibited in the Aborigines of Australia: Mr. Wake.

TUESDAY, April 18.

Statistical Society, at 7.45.
Zoological Society, at 9.—On the Dodo, Part II.;—Notes on the Articulated Skeleton of the Dodo (*Didus ineptus*) in the British Museum.—On Japanese recent Brachiopoda: Mr. Thomas Davidson.
Royal Institution, at 3.—On the Geology of Devonshire, especially of the New Red Sandstone: William Pengelly, F.R.S.

WEDNESDAY, April 19.

Meteorological Society, at 7.—On Deep-sea Thermometers: Staff Commander John E. Davis, R.N.
Society of Arts, at 8.—On the Economical Construction of Workmen's Dwellings: Dr. J. H. Hallard.

THURSDAY, April 20.

Royal Society, at 8.30.
Society of Antiquaries, at 8.30.
Chemical Society, at 8.
Linnean Society, at 8.
Royal Institution, at 3.—On Sound: Prof. Tyndall.

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THURSDAY, APRIL 20, 1871

APE RESEMBLANCES TO MAN

THE Zoological Society can hardly fail to derive decided material advantage from the publication of Mr. Darwin's "Descent of Man." It has been said that already there is a perceptible increase in the visitors to the monkey-house, though an early spring has no doubt co-operated with scientific zeal in the promotion of pilgrimages to the Regent's Park, undertaken in the interest of a more than Chinese worship of ancestors. These visits would, perhaps, be considerably increased if it were very widely known that a fine specimen of a closely-related structural ally was there to be seen and heard, and one the resemblance of which to us has, I venture to think, not been generally appreciated sufficiently. I allude to the fine specimen of the Hoolock Gibbon which has been some time at the Gardens, and which appears to rejoice in good health, good temper, and good voice.

Differing so greatly and fundamentally as I do from Mr. Darwin, it is with sincere pleasure that I give my testimony to the correctness of his appreciation of the value and bearing of man's bodily structure on his zoological position. There can, I think, be no doubt that his frame is so closely related to that of the anthropoid division of the Old World apes, that to accord to it the rank of a family is to go to the extreme of maintainable distinction. Descending, however, to smaller divisions, it is generally taken for granted that the palm of resemblance to ourselves can be disputed by the Orang (Simia), or by the African genus Troglodytes (which includes both the Gorilla and Chimpanzee) alone. The third member, however, of the anthropoid Simian Graces—the genus Hylobates (long-armed apes or Gibbons)—has claims to advance for an award in its favour which I am disposed to consider not unworthy of consideration. Assuming, for argument's sake, the truth of Mr. Darwin's hypothesis that man's body was derived by natural generation from some form of ape, it may, I think, possibly be the case that we have in the existing Gibbon the representative of an ancestor more in the direct line than either the Orang or the African forms, and this in spite of the many points in which the Gibbon recedes yet further from human structure. For though it is indisputable that we can enumerate a greater number of points of resemblance between man and Simia or Troglodytes than between man and the Gibbons, while it would be easy to draw out a catalogue of details by which the last-named apes differ more from man than do Simia and Troglodytes, nevertheless there are certain points in which the Gibbon genus resembles Homo which are striking and perhaps significant. Although the enormous length of the arms disguises the resemblance, yet the proportions of the Gibbon's frame (as in some respects long ago pointed out by Professor Huxley) are singularly human. The length of the leg as compared with the trunk, and the form and proportion of the bony thorax, are points which may be mentioned. Again, a Gibbon (the Siamang) is the only ape which possesses that striking human feature—a true chin. The slight prominence of the nose too is also very remarkable, a point which has not escaped the notice of Mr. Darwin, and is to be seen in the living specimen here referred to. Again, the power, quality, and compass of the voice are qualities justly dwelt upon by the last-named author; and, finally, the gentle, yet quick and active nature of the Gibbon is eminently noteworthy.

On the other hand the Orang is a very specially organised, quite aberrant beast (as I have elsewhere endeavoured to show), and the brain in the genus Troglodytes is considered by Gratiolet to indicate altogether other relationships. Now it is not impossible, on the hypothesis assumed, that the Orang, Gorilla, and Chimpanzee may be types which have really diverged further from that anthropoid root-form which most nearly resembled man than has the Gibbon, and that adaptations to conditions may have superinduced many of those human resemblances which at present characterise them. It seems difficult, certainly, to apply this view to some details, such *e.g.* as the vaginal process of the temporal bone on the basis cranii. On the other hand, it is not in the highest but in one of the lowest of the Simiadæ that I have found an anchylosed styloid process to be occasionally present.

A very interesting fact is the great Miocene Gibbon of Europe, the Dryopithecus, which goes to confirm the view here suggested as to the dignity of Hylobates; but of course we can but speculate inconclusively till Palæontology furnishes us with the nearest extinct representatives of the Gorilla, Chimpanzee, and Orang.

To prevent misconception, I may add that fully recognising the truth of Mr. Darwin's appreciation of man's zoological position, which I have ever maintained and indeed laboured to support, I none the less completely differ from him when I include the totality of man's being. So considered, Science convinces me that a monkey and a mushroom differ less from each other than do a monkey and a man.

ST. GEORGE MIVART

THE COLLECTION OF INVERTEBRATE ANIMALS IN THE FREE PUBLIC MUSEUM, LIVERPOOL

II.

WE have mentioned in a previous article[*] the division of the series of Invertebrate Animals in the Liverpool Museum into 216 groups. The following is the plan of arrangement adopted in connection with each group.

Wherever circumstances permit the plan includes: (1) A printed schedule. (2) Exotic species. (3) British representatives. (4) The printed tablet. (5) Earliest fossils. (6) Diagrams and other illustrations. (7) Species and varieties on a more extended scale.

(1) The schedule, of which an example follows, is printed in large type, and is placed conspicuously at the head of the drawer; it is designed to show the derivation of the group, *e.g.*—

"Group 198.

SUB-KINGDOM—*Annulosa*, Skeleton external, ringed.
PROVINCE—*Arthropoda*, Limbs jointed.
CLASS—*Insecta*, Legs six.
SUB-CLASS—*Metabola*, Transformations complete.
ORDER—*Lepidoptera*, Wings with scales.

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[*] See NATURE, vol. iii. p. 202.

SUB-ORDER—*Rhopalocera*, Horns clubbed at the apex.
FAMILY—*Papilionidæ*, Middle nerve of fore-wing four-branched."

The next sub-division appears on the tablet as the distinctive title of the 198th group.

"GENUS—*Ornithoptera* and allies, Bird-winged butterflies. About twenty species known."

(2) The further portion of the drawer, to the extent of three-fifths (more or less) of the whole area, accommodates from ten to sixty exotic species; such as are most distinct being preferred. A reference to the authority accompanies many of the generic, sub-generic, and specific names. The locality, when copied from a monograph, is stated simply; but when it is known where the specimen has been collected, the word "from" is added—*e.g.* "from Madagascar."

(3) The nearer right-hand corner of the drawer is occupied by representatives of the group indigenous to Britain. Some groups have no British representatives; in others—*e.g. Noctuæ genuinæ*, a selection from the British species fills three-fourths of the drawer. Amongst these, foreign specimens of rarities are admitted, but in all such cases the words "exotic specimen" are appended to the name on the label.

(4) The nearer left-hand corner is assigned to a few fossils showing the earliest appearance of the group in the Geological record. In one or two instances, *e.g. Hippurites*, the entire group is fossil, in others, of course, fossils are absent.

(5) Between these two latter sections of the drawer is placed the printed tablet, about the size of an octavo page. It exhibits an attempt to describe some of the salient points in the life-history of the group. Here, and throughout the series, some attention has been given to ensure legibility; names and descriptions being of much less value when they cannot be read easily.

(6) The upright portion of the table case over each drawer is given to miscellaneous illustrations of the group. The series includes drawings and photographs of structure and anatomy, economic products, silk in various stages, marine and freshwater pearls, cameos, from the rough medallion cut from the shell to the finished work, polished shells, and sections showing the interior of shells, eggs, preserved larvæ and pupæ, preparations in spirits, examples of mimicry, nests of *Hymenoptera*, galls and their tenants, timber and stone pierced by molluscs, crustacea and insects, distorted growths, healed fractures, coral beads, British and exotic specimens of fungi growing on pupæ, and many other objects of interest.

(7) The blocks on which some of the table cases rest are fitted with drawers suitable for receiving an extended series of species and varieties, valuable only to the student, and intended to be seen only on application to the Curator. Very little progress has been made in carrying out this portion of the plan, which has, however, the good effect of rendering it quite needless to overcrowd the groups with insignificant species.

The difficulties attending the formation of the series of specimens have not been very great. Collections have been presented to the Museum by several friends of natural science, amongst whom may be mentioned Mr. Samuel Smith, of Liverpool, the donor of a collection of shells rich in generic forms and in costly rarities of the highest beauty. Mr. Moore has been successful in establishing friendly communications with many captains of merchant vessels sailing from the port of Liverpool, some of whom have been supplied by the committee with dredges and collecting apparatus, and have become enthusiastic naturalists. In recognition of their services several of them have been received as Associate Members of the Literary and Philosophical Society of Liverpool, a distinction which seems to be highly appreciated by them. Something has also been done in the way of exchanges; but a large proportion of the whole series has been purchased specimen by specimen. No object has been purchased simply on account of its rarity, but at the same time no reasonable expense has been spared in procuring the most beautiful and perfect examples.

A few general remarks on the subject of expense may be permitted: details will gladly be communicated to inquirers connected with museums. Few collections exhibited to the public will bear comparison with corresponding series contained in private cabinets. Why should this any longer be permitted? It may arise, *in part*, from the impression that in public museums it is unnecessary to spend much on specimens. There can be no excuse for extravagance, but economy may be pushed too far. The trouble and great risk of collecting in tropical climates must often be very inadequately represented by the apparently high prices asked for the chief desiderata, and the rest of a collector's stock may remain on his hands for years. Again, if a genus or a group is illustrated in nature by a great variety of beautiful forms, this surely is a biological fact which may claim, on scientific grounds, to be fairly and appropriately represented. Even on the most severe estimate of what is necessary for an educational series, something must be allowed simply for the sake of beauty and attractiveness; that is to say, if museums are to avoid the fate of certain parochial lending libraries which contain only such books as everybody *ought* to like to read. Naturalists of the very highest scientific standing, much more ordinary observers, are greatly under the influence of beauty of form and colour. Even Mr. Wallace himself would not have been so near syncope at the sight of a new Brachelytron as he seems to have been on his first introduction to the magnificent *O. Crœsus*. Why, moreover, should the public be taught to esteem art treasures as so much more valuable than the choicest productions of nature? One hears of a pair of vases being sold for 2,000*l.*, a sum which would provide twenty first-rate table cases, and stock them with very fair illustrations of the whole of the invertebrate groups. It is a happy circumstance that a museum of common objects may, at a trifling cost, be established in almost any village, and with judicious local influence brought to bear upon it, may prove both useful and creditable; but why should not wealthy communities, possessing endless drawing-rooms ablaze with costly decorations, exercise something of a corresponding liberality towards the museum which is the representative of their appreciation of that which is higher than the highest art?

Considerable difficulty has been found in selecting appropriate materials for the printed tablets. Many of the chief continental authorities on the Invertebrata, admirable as are their works for the purpose of identifying species, afford scarcely a line of information on the life history of the objects they so grandly figure and often so elaborately describe. Even the reports of scientific expeditions may frequently be searched in vain for this kind of information, which has to be gleaned from authorities not always trustworthy, from scattered papers, or from books of travel such as have been issued in this country on the Malay Archipelago and the River Amazon. It is mortifying to exhibit forms distinguished by extraordinary developments of structure, and to be able to say nothing on associated habits. Such strange developments were once considered to be mere freaks of nature, but no one now doubts their having a biological and even a genealogical significance. What a field is is here opened! How little of the biology of a new form has been exhausted when it has been collected, named, described, figured, and even dissected! Scientific treatises have prepared the foundation for a solid knowledge of the subject, but there would be occasion for regret if biology should ever come to be regarded by students in an aspect too exclusively histological or even physiological, if such a view operated to the prejudice of genuine out-of-door observations. The greatest advance in Natural History made in the present, or perhaps in any other generation, has been mainly accomplished by two observers who are pre-eminently life-historians.

Little need be said of the miscellaneous illustrations contained in the upright portion of the table-cases. They seem to be very successful in engaging the attention of visitors of all classes—a point which is felt to be of prominent importance where the admissions amount to about 2,000 daily. What brings them here? is a question which again and again suggests itself. Reduce the number by all the idlers and sight-seers who, no doubt, constitute a large proportion of the gathering; still, if only 100 or even 50 seek some kind of instruction, even these in the course of a year form a large and teachable class. As a firm believer in the humanising effect of an intelligent interest in Natural Science, to myself the grand museum problem seems to be, how to make such an institution most beneficial to the greatest number.

HENRY H. HIGGINS

PRE-EUCLIDIAN GEOMETRY

Die Geometrie und die Geometer vor Euklides. Von Prof. C. A. Bretschneider. (Leipzig: B. G. Teubner, 1870. London: Williams and Norgate.)

UNTIL the appearance of this book, Montucla's celebrated "History of Mathematics" contained almost all that was known about the early history of Mathematics up to the present time. Later historians, even the careful Chasles, have almost exclusively copied him, without taking the trouble of searching the Greek writings for themselves. Montucla's remarks, however, are not only meagre, they are even not always correct. For this reason Prof. Bretschneider has collected all important passages in Greek writings which refer to the state of Geometry in Greece in the time before Euclid. This author is the first of whom complete works have reached us; with him, therefore, a *History* of Geometry begins. With regard to the ante-Euclidian times we cannot advance beyond conjectures, and these will always depend more or less upon the individuality of the historian. Perfectly aware of this, Prof. Bretschneider gives in the little volume before us, of about 180 pages, not merely his conclusions, but he adds the whole material which he has collected. Instead of simply referring to an author, he quotes *in extenso* the original Greek text, and adds translations. Thus every reader is at once enabled to form his own opinion, which, we feel assured, will in most cases agree with that of our author.

In the first section Prof. Bretschneider considers the Geometry of the Ægyptians, and tries to make out how far their knowledge extended. He protests against the old opinion that they possessed only the very first notions of geometry, and that the Greeks did not obtain anything from them worthy of the name of science. He refutes equally strongly the statement of some modern writers, who maintain that the Ægyptians knew not only all that Euclid gives in his Elements, but were even acquainted with the theories of quadratic equations and of conic sections. According to him geometry originated in Ægypt, where it was cultivated for practical purposes. It was rather an *art* of mechanical drawing than a science proper. The results obtained were collected in the form of fixed rules, always ready for use, most of them probably strictly proved others perhaps resting on experience only. Those collections of rules were at an early age included in the religious canons. Any alteration, any improvement, was thus almost impossible, especially as the only cultivators of Science, the priests, would take a secondary interest only in anything not strictly connected with religion. Thus it is not to be wondered at that geometry remained for thousands of years in the same state, till the unfettered genius of the Greek nation began to cultivate it, and then the progress was a most rapid one.

It is, however, remarkable, although natural enough, that the Greeks retained to a certain extent the *form* into which Ægyptian priests had cast their propositions. For this fact there exists a testimony in a papyrus at the British Museum, formerly in the possession of the late Mr. Rhind, which contains a pretty complete treatise on Applied Mathematics, in the shape of problems which are stated in that peculiar form with which we are so well acquainted through Euclid's Elements. Dr. Birch, who has given an account of it, dates it as far back as 3400—3200 B.C. Prof. Bretschneider traces many other peculiarities in Euclid's Elements—for instance, the order of propositions—back to the same source; so that the Ægyptian priests, who lived about 6000 years ago, have, in the most direct manner, influenced the mode of teaching geometry in English schools even at the present time.

The extent of Ægyptian Geometry is estimated as follows:—the theory of angles and parallel lines; the construction of triangles, parallelograms, and trapezoids from given parts, and the determination of their areas; the elementary propositions of the circle together with the inscribed regular polygons;—this is about the sum total of Plane Geometry. In Solid Geometry their knowledge was limited to the first notions about lines perpendicular to a plane, and the theory of parallel lines and planes in space. They were acquainted with the existence of prisms, regular pyramids of four sides, of the right cone and cylinder, the sphere, and of the regular solids with the exception of the dodecahedron, which is the only one discovered by Pythagoras. Of the properties of these solids they knew little, the theory of the sphere alone appears developed to a certain degree necessary for astronomical purposes. Of proportions and similar figures they knew nothing.

The succeeding chapters relate to Geometry in Greece. We shall only mention a few of the most striking differences between Prof. Bretschneider and his predecessors. That most of the anecdotes about Pythagoras—his long captivity in Babylon, for instance,—are rejected, need scarcely be mentioned. But many readers will be surprised to hear that Plato is not considered a very eminent mathematician as he himself did almost nothing to enrich this science, and most of the theorems usually believed to be due to Plato are his pupils'. Thus the conic sections were invented by Menaichmos. Again, the *reductio ad absurdum* is considered one of the simplest methods of demonstration and used long before Euclid thought of it.

One of the most important points in the book consists in a long passage out of Eudemos' "History of Mathematics" (about 340 B.C.), which Bretschneider was fortunate enough to discover in the commentary on Aristotle by Simplikios. It contains a critique on investigations on the problem of squaring the circle. Some of the methods here employed are exceedingly interesting, as they show what means were at the disposal of the mathematician. Antiphon, a contemporary of Socrates, inscribes a regular polygon in a circle, and then proceeds in the well-known manner to double the number of sides till these at last coincide with the circumference of the circle. He then considers the circle as a regular polygon, and says that it may be converted, like any other polygon, into a square. It appears, therefore, that Antiphon was the first who introduced infinitesimals into geometry, and thus became the forerunner of Archimedes. These methods were, however, far too much in advance of his time, and Eudemos rejects them as not exact. Hippocrates, known by his quadrature of the lunulæ, makes several attempts to extend this discovery, and to obtain similar results for other lunulæ and thence for the whole circle. Some of these attempts indicate great power, although they lead to nothing tangible.

The book contains many other important results, and all those who take an interest in the history of the development of science will feel indebted to the author for its publication.

OUR BOOK SHELF

Notes on the Natural History of the Strait of Magellan and West Coast of Patagonia, made during the Voyage of H.M.S. "Nassau," in the years 1866—1869. By Robert O. Cunningham, M.D., F.Z.S., &c., Naturalist to the Expedition. (Edinburgh: Edmonston and Douglas.)

WE regret to be obliged to find fault with the work of a naturalist, but duty to our readers compels us to say that this book should never have been published. There is perhaps no part of the world which is at once so remote and inhabited by such interesting savage tribes, with the main features of whose scenery and natural productions the public is so well informed as that which forms the subject of this book. It is therefore surprising that a gentleman who has had the opportunity of studying the natural history of this region at his leisure, should have thought himself justified in printing a volume of 500 pages of his rough journal, nine-tenths of which are occupied with a bald record of the usual monotonous incidents of sea and shore excursions, and with repetitions of facts already given us by Darwin, Hooker, and a host of other less eminent writers. There are, of course, some interesting facts and some original observations in this volume, but they are so thinly scattered amid a mass of details of weather and personal incidents, with records of the gathering of every common plant and the capture of every common as well as uncommon bird or insect, as to be not worth the search after. The book too is got up with an utter disregard of the reader's convenience. The author journalises his whole voyage, and at least one third of the volume treats of other parts of the world than those indicated by the title, yet the heading throughout is "Strait of Magellan," even when Rio Janeiro, Valparaiso, or the Azores are being described. Neither is there any indication of years or months, except when a change occurs; and if we find that something was captured on the "14th," we have to go back or forward many pages to discover whether we are in May or December. The plates too are wholly without references to the letterpress; and we find a curious plant (*Philesia buxifolia*) described at page 173, and figured at page 321, with no reference from description to figure or from figure to description. The illustrations seem thrown in at random, anything the author collected being apparently deemed worthy of a plate. On no other principle can we explain the plate devoted to an indifferent figure of the cranium of so common an animal as the puma; and another to the furcula of a condor, the picking-up of which is recorded at page 113, and figured full size, *à propos* of nothing, at page 303.

It is the more to be regretted that such a book as this has been published, because there is ample room for one of a different character, and for which Dr. Cunningham must have collected or have been able to obtain ample materials. The temperate parts of South America form a well-marked district, the productions of which are exceedingly interesting and their affinities well worthy of careful study. The relations of the fauna and flora of this district to those of Tropical America, of Europe, of Australia, and of New Zealand, require a thorough and critical examination; and this could hardly fail to throw much light on the means by which organic forms have been distributed, and on the relative importance of the various zoological regions into which the globe has been divided.

The author states in his preface that he has not yet completed the examination of his materials. Why then did he rush into print before he was able to lay before the world a single generalised result of his three years' voyage?

W.

Les Houillères en 1869. Par Amédée Burat, Secrétaire du Comité des Houillères Françaises. Texte et Atlas. (Paris, 1870. London: Williams and Norgate.)

THIS is an annual publication of a semi-official character, proceeding from M. Burat, the Secretary of the very useful Association of Coal-mine Proprietors in France. We are not aware that any such committee of coal owners exists in Great Britain, although other trades or professions, such as bankers, railway companies, &c., have similar committees. The Iron and Steel Institute, which has lately been holding its meeting in London, fulfils to some extent this purpose in regard to the iron trade. Wherever such associations exist we wish that they could be persuaded to publish as complete and valuable reports on the condition of their branch of trade as we have in these annual reports on the French coal trade, now available for ten or eleven years back. The present report consists of four chapters, which treat respectively of the strikes of the coal-miners, which had greatly interfered with the trade, improvements in the machinery and modes of working coal mines, the statistical conditions of the production of coal in 1860, and the legislation which had taken place in relation to the trade. It is impossible to avoid feeling, however, that the interest of such a complete statement is much reduced by the events which have occurred since the year to which it relates. All industrial improvement in France must be greatly retarded, if not converted into retrogression, for some time to come. The remarks on the subject of strikes form perhaps the most interesting part of the volume. England is the birth-place of trades' unions, and our capitalists feel their influence severely; but it is among an unstable and disaffected population, like that of the French manufacturing and mining districts, that they produce really evil effects. M. Burat gives emphatic testimony to the high character of British workmen, wholly uneducated as they often are. He says (p. 135), "Once again, in traversing the coal fields of England, we have been struck with their superiority, due not only to the special conditions of the coal basins, but to the aptitudes and the discipline of their working population."

W. S. J.

Jahresbericht uber die Fortschritte der Chemie, und verwandter Theile anderer Wissenschaften. Herausgegeben von Adolph Strecker, für 1868. (Giessen, 1870. London: Williams and Norgate.)

THE Jahresbericht is too well known as a trustworthy book of reference, both for chemists and physicists, to need commendation; and the care and judgment with which it is compiled have made it almost indispensable to those interested in science. In these days, when the amount of work done—both useful and useless—is so great, it is no slight convenience to have the year's labour carefully sifted and preserved for reference in a single volume. The present volume, under the editorship of Herr Adolph Strecker, is arranged with all the care which has characterised previous numbers, and contains numerous papers of unusual importance. Among the most important of these may be noted Becquerel's researches on electro-chemical action, Graham's experiments on the occlusion of hydrogen by palladium, and the existence of the metal hydrogenium. Rammelsberg's researches on the periodates are given at considerable length, and also Bunsen's process for separating the platinum metals found in the residues of the Russian mint. In the organic section of the book very important papers are given on the constitution and derivation of benzol, toluol, and napthalin, and Gräbe and Liebermann's valuable researches on the preparation of alizarin from anthracene are fully described. Perhaps the success of the Jahresbericht has caused the Chemical Society to make the changes in their journal which, we understand, are in contemplation. We believe that it is intended to reproduce in the pages of the new journal all important researches made in this country and abroad, so that it will become to us what the Jahresbericht already is to Germany. If the same care is bestowed on its production that has always characterised its German contemporary, we anticipate for it a similar and deserved success.

F. J.

Notes or Jottings about Aldeburgh, Suffolk. Relating to Matters Historical, Antiquarian, Ornithological, and Entomological. By Nicholas Fenwick Hele, Surgeon. 8vo, pp. 198. (London, 1870.)

IT is no disparagement to Mr. Hele to say that he is not a Gilbert White, and the reviewers who have compared this book with the "Natural History and Antiquities of Selborne" have certainly done its author a wrong, while they have shown their own want of discrimination. Gilbert White was in the front rank of the naturalists and antiquarians of his day; as an outdoor observer he had no equal, and perhaps never will have one. Add to this the charming and delicate simplicity of his ideas, rendered all the more striking by the slight shade of pedantry which not ungracefully tinges his style; for the old pupil of Warton never forgot his scholarly breeding, and what wonder is there that "Selborne" is an English classic? If ever there was a naturalist in whom the poetic faculty was developed, if ever there was one who wittingly or unwittingly possessed the scientific use of the imagination, Gilbert White was the man. Now, there is nothing to show that Mr. Hele is a closer observer than (happily) many of his fellows, and what he has to say he says in very plain, straightforward language. It is clear that he keeps his eyes open whene'er he takes his walks abroad, but he favours us with few inferences from his own experience. Still we must particularly praise the absence of any attempt at fine writing, and the consequence is a little volume of a kind of which we should be glad to see many more. The "matters historical and antiquarian" of which he treats are, of course, beside the path of Nature, though the old boat found in company with flint-flakes deserves the attention of the Anthropological Institute; but the topographical, ornithological, and entomological notes include much that is of interest, exception being taken perhaps to a few of the statements. However, Mr. Hele's opening assertion that "Aldeburgh, as a place of resort for the naturalist, may be fairly classed as one of the most attractive localities in the east of England," is undoubtedly true, and his sketch of the neighbourhood shows that he appreciates its advantages. We have derived great pleasure from this unpretending little volume, and are sure most of our readers will do the same.

Mineralogie der Vulcane. Von G. Landgrebe. (Cassel and Leipzig, 1870. London: Williams and Norgate.)

THIS is not a very satisfactory book. It consists of an alphabetical arrangement of minerals with a brief description of each species, the species selected being those which the author regards as volcanic minerals. In spite of his title of "member of several learned societies," we take leave to doubt whether he has any clear idea of what a volcanic mineral is. A great part of his book is devoted to minerals which, like the zeolites, are not original volcanic products, but the result of subsequent changes. Any mineral which he can discover to have been ever found in an eruptive rock, he sets down in his pages as one of the "minerals of volcanoes." There is no critical faculty shown in discriminating between the primary and secondary ingredients in volcanic rocks. A good work on volcanic minerals properly so called, with a minute and exhaustive examination of their microscopic structure, and a philosophic induction therefrom as to some of the conditions under which volcanic action must take place, would be a great boon to science. But it is not to Dr. Landgrebe that we must look for such a treatise. He tells us that perhaps he might have delayed the publication of his volume until he could take advantage of the results which the new development of mineralogy through the application of the microscope promises to furnish; but as he found the delay might prove a tedious one, he decided not to wait any longer, but to present his labour of "Lust und Liebe" to the indulgent criticism of the public. Even so; such is the history of too many books in the scientific as well as in other branches of literature.

A. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Science College at Newcastle

PERMIT me to rectify a misapprehension which appears, not through any fault or oversight on your part, in your appreciative article upon the College of Physical Science, which the University of Durham, with the co-operation of the scientific men of the North of England, is engaged in founding at Newcastle-upon-Tyne.

The Executive Committee fully recognises the importance of Biology. The professorial chair, which was called at the public meeting that of Geology and Mineralogy, will be called, according to a subsequent decision of the committee, the Chair of Geology, Biology, and Mineralogy. It is true that this designation leaves it uncertain whether Biology is co-ordinated with Geology, and has its claim to be a distinct science allowed, or is subordinated and intended to be treated only as a component part of Geology. In the former case, it may be said, the Chair will be too capacious for one man to fill; in the latter, the rights of biology are but inadequately acknowledged.

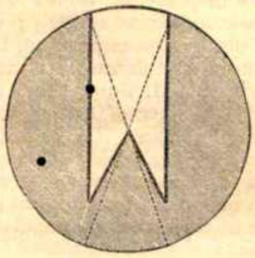
The answer to this is, that the programme of teaching is necessarily, at the present stage of affairs, initiatory. It will, no doubt, when the work of the Institution has commenced, and the scale of operations which everything seems to foreshadow has been in some measure realised, receive such expansions and modifications as are suitable to the relative value of the sciences, and the actual wants of the students.

J. WAITE

University College, Durham, April 17

The Aurora Borealis

A VERY bright display of aurora was visible here last night, illuminating the greater part of the heavens at intervals with a fitful light. At 9h. 45m., when I first noticed it, broad cirrus-like brushes of white light stretched in parallel bands across the zenith, from below Corona in the north-east, across Cor Caroli, nearly overhead, to about the altitude, and 15° or 20° south of Venus, then shining dimly through clouds in the western sky. My view of the aurora was afterwards confined to an east window; but from a short examination of a clear part of the sky towards south-west, it appeared to be almost as bright, at first, in the opposite direction as in the quarter between north and east, where I had, from that time onwards, an uninterrupted view of its progress. The streamers were white, and irregular in form, rising from no distinct arch, or definite base in any quarter, but they occasionally met and formed a bright corona overhead. A rose-tint pervaded some of them, in the north-east, at 10h. 30m., and presented itself in different parts of the sky until about 11h., when the phenomenon faded, and a faint glow only remained visible in the north. At about 11h. 40m., while streamers reappeared, forming a bright corona overhead, whose arcs and beams appeared to grow more densely luminous until 12h. 15m. It then showed a well-defined central nucleus, with rays of great brightness proceeding from it, about three-sevenths of the distance from η Ursæ Majoris to Arcturus, white streamers rising to meet it all round, with the appearance of a cupola or dome. While I watched some of the brighter stars through its dense light-cloud, it rapidly assumed a vivid fiery red colour, and a similar bright appearance breaking out at the same moment at the base of a north-east streamer, the fiery glow reflected among the clouds in that direction perfectly resembled a distant conflagration. A wide expanse of brilliant orange and crimson-red light soon joined these two regions of greatest intensity together in one splendid blaze of ruddy colours. This brilliant outburst faded away at 12h. 25m., the streamers disappearing, until 12h. 40m., when they again met overhead, and formed a corona with a sharply-defined nucleus, about 3° west of the star γ Böotis. The light of the white streamers flickered considerably, as if waves of varying brightness were driven rapidly over the whole phenomenon by the wind. Until after two o'clock, when the moon rose, and their light was weakened although not extinguished, a constant succession of bright steamers occupied the north-east sky between the horizon and the zenith; towards these I directed the slit of a Browning's Student's Spectroscope, in order to determine, if possible, the position of some of the auroral lines. A single greenish line only was so faintly visible in the spectroscope that all attempts to view it simultaneously with the cross-wires of the instrument proved unsuccessful. A simple form of pointer, substituted for the use of the wires in the dark field of the telescope enabled me, however, to identify its position with considerable accuracy. A circular card, of the figure shown in the sketch, was slipped into the forward end of the eye-piece until it reached the diaphragm upon which the cross wires are stretched. It was so disposed that the angular pointer in the middle of the card exactly filled up the lower angle of the cross-wires; and its sharp point coincides almost exactly with the place of their intersection. In a field of view almost dark it was possible to see the summit of the card-pointer with considerable distinctness; and if a great and imposing phenomenon may be compared with an object so diminutive, the appearance of its dark peak, surmounted by the narrow, flickering line of light, resembled in the dim field of view the cone of a volcano, projecting in a thin jet from its apex frequent eruptions of varying height and brightness. By obtaining a reading of the soda-line seen in the flame of a salted spirit lamp, and referred to the pointer in the same manner, I obtained as a result of three observations of the auroral line, differing less than two minutes on the graduated position-circle of the spectroscope, a place in the spectrum twenty-four minutes more refrangible than Fraunhofer's line D. Many observations of the dark lines of the solar spectrum, compared with their positions in Kirchhoff's maps, having assured me that a minute of arc on this spectroscope corresponds between Fraunhofer's lines D and *b*, with 10·44 divisions of Kirchhoff's scale, the resulting difference of refrangibility between the green auroral line and Fraunhofer's line D is 250·6 divisions of Kirchhoff's scale; and its absolute position, assuming for that of D to be 1005 Kirchhoff, was within 10 or 20 units on either side of a place at about Kirchhoff 1255. A bright line in the spectrum of the aurora was observed by M. O. Struve at Pulkowa, in April 1868, whose position he ascertained to be within 10 or 15 units of the scale at Kirchhoff 1259, a place apparently identical with that of the bright line which was principally visible in last night's aurora. The faint and uncertain brightness of its appearance, while confining my attention principally to its observation, prevented me from searching in other parts of the spectrum for accompanying bright lines Within a space of about 400 Kirchhoff on each side of that which was recorded, I could, however, perceive no traces of any co-existing lines. The position of the bright lines which are most conspicuous in different auroras is, perhaps, a subject of interesting inquiry in connection with the yet unascertained laws which govern their appearance, and with the varying characters and peculiarities of their phases. A remarkable circumstance connected with the appearance of the single line observed on this occasion, was the flickering and frequent changes with which it rose and fell in brightness, apparently even more rapidly than the swiftly travelling waves, or pulsations of light, that repeatedly passed over the streamers, near the northern horizon, towards which the spectroscope was directed.



A. S. HERSCHEL

Andersonian University, Glasgow, April 10

THERE was a brilliant display of aurora borealis visible from this place on Sunday the 9th inst. I first noticed it about 10.45 P.M.; there was then a considerable luminosity in the N.W. with a magnificent red glow and streamers springing from the W. extending to within 20° of the eastern horizon, also radiation from other parts in the N. and N.W., but less brilliant. At about 10.55 P.M., a bright streamer made its appearance near the zenith crossing the red at right angles, and standing out clearly upon it. The aurora had nearly faded away at 11.10. When it was at its brightest through a direct-vision spectroscope with the slit rather wide and directed to the N.W., where there was scarcely any colour, the red and green bands usually seen under such circumstances were clearly defined, more particularly the red band. I then directed the spectroscope to the W., at a part where the red light was most intense, to my surprise the red band was scarcely to be discerned, and looked blurred, and spread out towards the green, which was better defined, but not clearly so. I several times repeated the observation with the same result. This, if confirmed by other observations, would appear to point to some fluorescent property of the upper atmosphere.

W. J. B. Thompson

Weybridge Heath, Surrey, April 11

There was a magnificent auroral display last night (Sunday), which commenced at 10.45 P.M., and continued till 11.15. The streamers radiated from a point due N.W., there being no clouds in their vicinity, the colour was a deep red, and they extended far beyond the zenith. At 11.20 another display was seen, but at right angles to the previous one, and streamed across the zenith due N. and S. When the sun had set there was an accumulation of stratified clouds near the western horizon and quite horizontal, these gradually rose radially, having their centre situated a little north of that of the aurora; and at about 11.10 the uppermost radii passed into the field of the northernmost streamers, so that it became impossible for me to distinguish between them which was cloud and which was auroral. After the first appearance had faded away, the second was seen to proceed from these clouds upon the latter passing overhead, but did not last longer than fifteen minutes, by which time the radial clouds had become so dissipated as to lose their character, and, to me, were almost invisible. I may mention that this peculiarity in the clouds was noticed on Saturday evening (the 8th inst.) after sunset.

The old superstition of bloodshed taking place in a distant country, and the red colour being the sign, is still prevalent; it was asked of me, by an observer, "whether there was a war in Paris, or was it on fire, as the reflection of something like it was in the sky?" Another declared he could hear "distant noises of the clashing of weapons." [*] But the assertion of an old woman who was amongst my interrogators, was that it was the reflection of fire, and "showed that *mahogany* was burning." The ignorance on this subject is appalling, and when we have such absurd accounts of the aurora borealis from persons of our own time, surely the obscure mentions of this phenomenon in the old chronicles (*vide* my article on this subject in Nature for Dec. 29, 1870) by perhaps equally ignorant observers of nature, are not so difficult of interpretation as many imagine. The experience of the present is the key to the past.

John Jeremiah

43, Red Lion Street, Clerkenwell, April 10

On Saturday, the 15th inst., a remarkably fine aurora was to be seen here towards the N.N.W. It was first observed at 8.45, and continued from then shining brightly till 9 P.M., when it made its disappearance. At first it formed a complete rose-coloured arch, in which flickering rays of bright white light were occasionally to be seen. During its appearance its position was gradually shifted round to the north, where it remained till it disappeared.

J. B. F.

Marlborough College, April 17

The Comparative Aggregate Strength of the Light from the Red Hydrogen-Stratum, and of that from the rest of the Chromosphere

I was a spectator of the total eclipse on December 22, and though I had no further connection with the Government expedition than that I was favoured with a passage home in H.M.S. *Urgent*, I was kindly permitted by the Rev. Mr. Perry and his party to set up my telescope at their observatory at San Antonio, near Port St. Mary, on the harbour of Cadiz.

I made an observation during the total phase, which I ought to have published sooner, but did not, partly from the distrust which an inexperienced observer naturally feels in his own results, and partly because I did not see that it might have some value. As I now think it may be of some interest, I will state what I saw.

The instrument which I used was one of Browning's direct-vision spectroscopes with seven prisms. It was not fitted to the telescope which I had with me, simply because I had not time to get them properly adapted to one another either before leaving England, or after arriving at Gibraltar by the Peninsular and Oriental steamer on December 16.

I spent the first thirty or forty seconds of totality in taking a general observation with the telescope; after that I gave it up to the friend who was travelling with me, and had fifty seconds at least during which I used the spectroscope. At first I tried to use it with the slit moderately narrow (about ·005in.); but, as there was much cloud over the moon, though not enough to prevent the corona and a slight amount of red light from the red hydrogen-stratum being seen with the naked eye, I could get no light to pass through the prisms while the slit was narrow; accordingly, I enlarged the slit to a width which would be absurd under most circumstances (about ·025in.), and then, taking a general view of the corona, saw a spectrum in which the red and green were present, but the blue and violet totally absent, and on this spectrum one line and one only: this line was strong compared with the rest of the spectrum, red, and of course broad to correspond with the width of the slit; and from its position on the continuous red part of the spectrum was either C or near it.

After I first saw this spectrum and line I had fully thirty seconds, which I employed in directing the slit as well as I could towards all parts of the corona, and in seeking for other lines; during the whole of the time I saw the same spectrum and the same red line only.

As this spectrum and line, from the nature of my instrument, must have proceeded from the mingled light of all parts of the chromosphere, I consider my observation as a humble imitation of that made by Mr. Pye, recorded in Prof. Young's article in Nature for February 2 (p. 261). The mingled light was sufficient to give him with his delicate instrument the lines C, D, 1474; and he estimated the light of 1474 to be apparently greater than that of C, and to bear to it the ratio of 10 to 8·5; yet the light of 1474 was not sufficient to penetrate my rougher instrument, although the red line (which I can hardly doubt was C) showed conspicuously throughout the time that I turned the spectroscope upon the corona.

Whatever value my observation has it must be set in the balance against Mr. Pye's, and tends to weaken the argument by which Prof. Young attempts (in the article above-quoted) to show that the angular area of the self-luminous corona bears to that of the red hydrogen-stratum visible during totality so large a ratio as 35 or 70 to 1, and consequently extends 8' or 16' from the sun; in fact my observation tends to give a smaller extent of self-luminous corona than Mr. Pye's.

Basil E. Hammond

53, Bilton Road, Rugby, April 11

Mount Washington

Twice recently it has been mentioned in your columns that Mount Washington in New England attains the height of 10,000 ft. If it were so its summit, for nearly 2,000 ft., would be clothed with perpetual snow. The most careful measurements—those of Prof. Guyot—give the height of the mountain as 6,288 ft.

Samuel H. Scudder

Mentone, April 13

[The communications in question were received from an American correspondent.—Ed.]

The Name "Britain"

The existence in former times of other Britains than those of our own island should not be overlooked in discussing the derivation of the word. According to Dr. Karl von Spruner's maps of France and Spain for the sixth century, besides the Britannia in the north-west of France, there was a town called Britonia (the present Mondonedo) in the north-west of Spain. Unless the similarity of these names is nothing more than a strange coincidence, it does not seem probable that the name Britannia can have any connection with the tin which is found only in one of the Britannias. Carte (vol. i. p. 10, note), speaking of the Phrygians (Briges) "the first nation that entered Europe," says, "of this sort are the people known over England, Scotland, and Ireland, by the name of Brigantes; the Britanni on the sea coast of Gallia Belgica; the Brigani mentioned by Pliny as seated in the Alpes . . . . A Brigantium, now Braganza in Portugal; and another (town) of the same name, now Britanças or Corunna, called anciently Portus Brigantinus, in Gallicia." According to this the Briges would be the common ancestors of the Britons of England, France, and Spain, and the similarity of the names in these countries could be accounted for.

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[*] How old and persistent this belief is may be shown by quoting a remarkable passage from Pliny. In Book II. chap. lvi., he says—"In the time of the Cimbrian Wars we have been told that armour was heard to rustle and the trumpet to sound out of heaven, and this happened very often both before and after those wars. But in the third consulship of Marius the Amerines and Tudertes saw men in arms in the sky, rushing one against another, from the east and west, and those from the west were discomfited. That the very firmament itself should be on fire is no wonder, for often it hath been seen when clouds have caught any great deal of fire." This same appearance is very probably that which Josephus refers to in his narrative of the terrors sent by God previous to the siege of Jerusalem.

A. R. H.

Faunas of Oceanic Islands

In NATURE, of February 16, I observe the reviewer of Mr. Godman's "Natural History of the Azores," makes the following statement:—"Mr. Godman appears to be the first who has, after a personal exploration of one of these oceanic groups, endeavoured to collect all that is known of its natural productions." Now, it is not with the intention of detracting from the merits of Mr. Godman's work, but merely to refresh the memory of your reviewer, that I beg to state, that I published in 1859 a small volume entitled "The Naturalist in Bermuda," which contained all that was known of the natural productions of those islands at that date. It was by no means a complete essay, but as stated in the preface "merely a prelude to a more complete publication on the same subject," which anticipated work, the result of several visits to the group, I hope to present to public notice shortly.

J. MATTHEW JONES

Institute of Natural Science, Halifax, Nova Scotia

Influence of Barometric Pressure on Ocean Currents

IN the recent discussions on the influence of barometric pressure upon ocean currents, I have not seen any allusion to the observations that have been made upon the effect of variations of barometric pressure upon the sea-level. In a memoir by M. Ch. Aimé, "Sur les variations de niveau de la Mediterranée," in the Annales de Chimie, tome xii., 1844, it is stated that a fall in the barometer is pretty uniformly accompanied by a rise in the sea-level to about thirteen times its amount. The Report of the British Association for 1841 contains a letter from my old friend, Mr. T. G. Bunt, of Bristol, stating that his observations upon the tide-gauge under his charge led him to conclude that a fall of one inch in the mercurial column was accompanied by an average rise of about 13½ inches in the high-water level. And the same industrious and careful observer, in a recent "Discussion of Tide Observations at Bristol" in the Philosophical Transactions for 1867, gives as the mean result of twenty-one years' examination of this point, "12·772 inches of tide to one inch of mercury." I referred to Mr. Bunt's observations in a discussion at the Geological Society (March 6, 1867) on a paper by the Earl of Selkirk "On some sea-water-level marks on the coast of Sweden," pointing out that some of the discrepancies in the observations as to the sea-level of the Baltic might be attributed without improbability to variations in barometric pressure. I have since learned from Admiral Key, who served in the Baltic fleet during the Russian war, that he had been led by his own observations to a like conclusion. And I find it stated in the description of the Baltic Sea, in the English Cyclopædia, that its level is sometimes observed to rise, and to remain thus elevated for a time without any obvious cause, two or three feet, of which phenomenon the explanation is probably the same.

I am sorry to find that I have not succeeded in convincing Mr. Laughton of the existence of a regular undercurrent in the Strait of Gibraltar. If he will take the trouble of carefully perusing the detailed report which I have presented to the Royal Society, he will find that he is quite in error in stating that I rest my affirmation upon "one observation after several attempts made in vain." All our observations, when rightly interpreted, tended to the same conclusion. The *reduction* of the boat's drift *almost to nothing*, in the first set of experiments, when it lay in a surface-current running nearly three miles an hour, with a breeze setting in the same direction, was just as conclusive evidence that a reverse current must have been acting on the current-drag below, as was the *reversal* of the boat's drift in the subsequent experiment, when the surface-current was less rapid and the opposing breeze diminished its action on the boat. And our observations of the Temperature and Specific Gravity of the 250 fathoms' stratum most unmistakeably indicated on both occasions its Mediterranean derivation.

I should like to know what is the precise minimum of movement which is held by Physical Geographers to constitute a *current*. There seems to me a great deal of confusion upon this point. The existence of an underflow of polar water towards the Equator cannot now be a matter of question. Commander Chimmo has recently obtained with the Miller-Casella thermometers a temperature of 33½° at a depth of 2,306 fathoms nearly under the equator. What is the rate of this movement is a point as yet undetermined. But the rate of the northerly flow of warm surface-water between Scandinavia and Iceland, which is usually attributed to the Gulf Stream, but which I regard as the complement of the southward flow of deep polar water in a vertical oceanic circulation, is estimated by Admiral Irminger at from 1½ to 2½ miles per day. Is this, in the language of physical geography, a current?

WILLIAM B. CARPENTER

University of London, April 10

The "Times" Review of Darwin's "Descent of Man"

THE British public are deeply indebted to the *Times* Reviewer for his very comforting and reassuring remarks on Mr. Darwin's "Descent of Man," in which he has so well exposed the "utterly unsupported hypotheses," the "unsubstantial presumptions," the "cursory investigations," of that "reckless" and "unscientific" writer. It is a great satisfaction to find that Mr. Darwin's odious conclusion that the genealogy of the Talbots, and the Howards, and the Percys must be traced back beyond the Conqueror to an Anthropomorphous Ape, and beyond the ape to an Acephalous Mollusk, rests on no logical foundation whatever. The Reviewer well suggests that anything so odious in idea, so immoral in its apparent tendency, and so different from what we have been accustomed to believe, cannot possibly be true. One is so glad indeed to be free once and for ever from the mischievous influence of such "unpractical," "disintegrating speculations," that it seems worth while trying, if space can be found for the experiment, to elicit from the good-nature of the Reviewer, or of those who think with him, a little clearer explanation here and there, before the subject is finally consigned to a well-merited oblivion.

Mr. Darwin is invited in one passage, "if he wishes to corroborate his hypothesis, to commence by experimenting on some superior kind of Ascidian, and see whether, by patient selection, he can induce any of them to split themselves in half, and abandon their permanent support for a vagrant oceanic existence." Now, it is a fact that among Corals or Polypes, which are not far removed from Ascidians, these interesting experiments are actually exhibited; for the cæspitose Corals, by what is called fissiparity, *do* split themselves in half, thus forming two complete individuals where only one grew before, and the Corals of the genus Fungia are fixed when very young, but subsequently break their pedicels and become free. The whole group of Zoophytes, recent and fossil, connects together marvellously different forms by an almost infinite series of wonderfully minute links. The study of such a group is therefore no doubt dangerous, if not decidedly pernicious, as tending to gloss over "the enormous and painful improbability" of Mr. Darwin's speculations. For if upon examination it seemed likely, or almost certain, that different genera of Polypes were connected with one another by descent, some rash enthusiast might think a similar conclusion not impossible in the order Primates. Fortunately, one is estopped from suggesting that in fact some genera of Polypes *may be* connected by descent, for fear of incurring the sharp reproach to which Mr. Darwin has so frequently laid himself open, of "conjugating the potential mood." Hitherto in most departments of thought and inquiry, probable evidence has been allowed to count for something, and most men are content to believe themselves to be the sons of their reputed fathers upon a mixture of evidence and authority, which, by the very nature of the case, can never rise to absolute demonstration. The Reviewer has done good service to society by showing the untrustworthy character of the foundation on which all our genealogies are built. It would be well in future if some auxiliary verb, expressive of doubt and uncertainty, could be combined with our patronymics.

Mr. Darwin, it appears, has "a facile method of observing superficial resemblances." For instance he surprises the apprehension of the vulgar by exhibiting the curious likeness between the embryos of a man and a dog. As every one of course knows how he looked when he was still in his mother's womb and less than an inch long, that stage in a man's career when he is only too like an embryo puppy, might have been shrouded under a delicate reserve. If, in place of this absurd "superficial resemblance," Mr. Darwin could have pointed out similarities between man and the lower animals in regard to minute structures of bone and muscle, or in the organs of sense or speech, his argument might have been deemed a little more scientific.

Persons who have read his book say that he does dwell with considerable force upon these very matters, but it is easy to see from the Reviewer's tone that they are mistaken, and that such investigations have been sacrificed to a glance or two at things on the surface. This is the more to be grieved and wondered at, because in his monograph on the Fossil Cirripedes and in his work on the Fertilisation of Orchids, Mr. Darwin showed an uncommon aptitude at "a thoroughly scientific *clairvoyance.*"

The Reviewer thinks it perfectly reasonable that the hand of a man and the foot of a horse, the flipper of a seal and the wing of a bat, should have all been formed upon the same general plan, without any connection by a common ancestry. It would be extremely gratifying to an inquiring mind, if he would explain upon this reasonable plan, the vast succession of creatures unveiled by geological research. Why have innumerable species been created and then destroyed? When did the creation begin, and when did it end? What causes, or if there were no causes properly so called, what caprice brought about the extinction of the mammoth, and led to the introduction of the modern species of elephant? Has the creative power been at last exhausted, or do sudden creations still occur, only in a shy sort of way, when no one is looking on? The Reviewer very sensibly censures Mr. Darwin and his followers for not specifying the year B.C. when the process of evolution first began. It is with the less diffidence, therefore, that a question is propounded above as to the date of the creation. The solution of this point of chronology will be awaited by many with extreme impatience, as different nations give very different accounts, and the Hebrews, who have a very ancient record, are by no means at one with themselves in the Hebrew and Greek editions of it. The number of years required for the process of evolution is confessedly indefinite, and as the whole hypothesis must, therefore, be destitute of any scientific value, it is no doubt quite fair on the Reviewer's part, to represent an indefinite number of years as equivalent to "infinite time." But the steps required for the process are also an indefinite number, and on this point he is less clear than elsewhere, for, referring to the old sophism respecting Achilles and the tortoise, he tells us, from Sir Isaac Newton, that "quantities ultimately coincide which may be proved to approach each other indefinitely, *within a finite time.*" From this it would seem that, if Darwinians could be content with the boundaries of geological time, the genealogies of men and apes might ultimately coincide. To avoid this miserable and preposterous conclusion, we are told that the solution of the sophism by Diogenes, "is the only true one," *solvitur ambulando.* We are further obligingly informed that this solution is *identical* with Newton's. And as Mr. Darwin cannot transform one species into another under our eyes, the eminently unpractical character of his speculations is triumphantly exhibited. It will be very impertinent if any one suggests that the instantaneous creation of a species has never yet been witnessed, and that those who believe in such occurrences ought, on the *solvitur ambulando* principle, to favour the world with, at least, one such exhibition Captious persons may find fault with the Reviewer's opinion that the poetic faculty has received no development since Homer, and the religious sentiment none since the book of Genesis. They may call to mind that Moses and Socrates, and St. Paul and Luther, were guilty, like Mr. Darwin, of laying before popular audiences dangerous and "disintegrating" speculations; they may fancy that truth is worth discovering, even when it seems to involve some contradiction to our pride and some loss of comfort to our finer feelings, but such persons must be very captious, and the Reviewer will, doubtless, know how to deal with them.

Torquay, April 15 THOMAS R. R. STEBBING

Sexual Selection

IN the first volume of "The Descent of Man," at page 396, Mr. Darwin says, referring to butterflies, that "the lower surface (of the wings) generally affords to entomologists the most useful character for detecting the affinities of the various species." I think, also, that this lower surface might afford another link in the chain of argument by which Mr. Darwin supports his theory of Sexual Selection. Thus, for example, to speak of British species only, in the cabbage butterflies, the under surface of the wings is alike in both sexes of *Pieris Brassicæ*. The black spots, however, which appear on both surfaces of the fore wing of the female vanish from the upper surface of that of the male, probably because the female has some dislike to them. There is no difference in food-plant, habit, or need of protection here; the only explanation seems to be a whim of the female or a whim of nature, and we have lately discarded all thought of nature being freakish. In *P. Rapæ* and *P. Napi* a similar difference prevails, though less constant and in a degree less marked. In the allied *Anthocharis Cardamines* the under surface of both sexes is alike, notwithstanding the vast difference of their upper surfaces. When these butterflies alight and close their wings, the under surfaces of the *hind wings* are alone visible, and these are, apparently, the parts of the insect modified for the sake of protection. The simple yellow in *Brassicæ* and *Rapæ*, the green-veined yellow in *Napi*, the green marbling in *Cardamines*, of the under sides of the hind wings, are well fitted to conceal those insects as they settle on the wild flowers which they prefer.

Again in *Hipparchia Janira* the light brown patch so conspicuous on the upper surface of the fore wing of the female vanishes from that of the male; and in *H. Tithonus* and *H. hyperanthus* a tendency to decrease the quantity of light colour on the upper surface of the male butterfly prevails. So is it also with one of the Hair Streaks, *Thecla Betulæ*, the under surface still remaining alike in both sexes of these different species. In this case the female butterflies would seem to wish their partners to be of a duskier hue than it is granted to themselves to be. The differences mentioned above are so slight that Mr. Darwin says at page 317, "With those (butterflies) which are plain-coloured, as the meadow-browns (*Hipparchiæ*) the sexes are alike." But it will be admitted that though these differences *are* slight they are yet important, as showing a tendency, more or less marked, to follow the rule which Mr. Darwin has laid down; and every sign of such a tendency strengthens his case.

In *Apatura Iris* the under surface of both sexes is alike, though the male has his upper surface glorified with purple for the delight of his plain brown wife. In the blues, *Polyommatus Alexis*, *P. Corydon*, *P. Adonis*, and *P. Ægon*, the under surface of both sexes is also alike, though in the males the blue and in the females the brown of the upper surface forms the background of the spotty design. The blue blood is very strong in these butterflies, and will show itself sometimes even in the females; who, if powerless over their own decoration, have at least succeeded in bringing out the innate splendour of their handsome husbands. With the blues, as with the cabbage butterflies, the under surface of the hind-wings seems specially adapted for protective purposes; every butterfly-hunter knows how difficult it is to distinguish the common blue when it is sitting, shut up, on a scabious flower. It is the same with the small copper butterfly, which has its under surface dotted very similarly. But burnished copper and dazzling blue are not colours for protection, surely. We may give the under surface to Mr. Wallace, but we must yield the upper surface to Mr. Darwin.

At page 399, speaking of the ghost moth (*Hepialus humuli*) and others of the moth kind, Mr. Darwin says, "It is difficult to conjecture what the meaning can be of these differences between the sexes of darkness or lightness; but we can hardly suppose that they are the results of mere variability, with sexually-limited inheritance, independently of any benefit thus derived." The female ghost moth follows Mr. Darwin's rule, that females are most conservative of the features of kinship. In her colouring she closely resembles the other *Hepialidæ*. And the male, notwithstanding his shining shroud, keeps to the same sober under-colouring as his mate. Now *H. humuli* is more nocturnal in its habits than any of the other species in the genus *Hepialus*; I have caught *H. hectus* and *H. lupulinus* flying in bright sunshine, but I have never seen the ghost moth until dusk was far advanced. May it not be that sexual selection has come into play here by the female preferring the *whitest* male, he being the most distinguished when all colour has faded into dimness? She could not decide between differing patterns of gold and amber at that hour, but a snow-white surface would then be quite visible. The fact mentioned at page 402, that "in the Shetland Islands males (of *H. humuli*) are frequently found which closely resemble the females" (I have seen similarly varied males in Peterhead collections), would seem to confirm this theory; for the twilight of the north, at the season when the ghost-moth abounds, is so bereft of dusk that whiteness would not be needed to render the males visible.

It is possible that those acquainted with the habits of the other moths, of which Mr. Darwin speaks, may be able to reconcile their appearance with the rules of Sexual Selection which he has laid down so clearly and illustrated so fully in his last great work. GEORGE FRASER

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SCIENCE IN PARIS DURING THE SIEGE

IN a somewhat striking passage in the *De Augmentis Scientiarum* Francis Bacon contrasts the endurance of monuments of learning with that of those raised by the hand of man. The verses of Homer, he reminds us, have endured for more than twenty centuries, during which time numberless palaces, temples, and cities have disappeared from the face of the earth. Some such reflection as this may have induced the members of the *Académie des Sciences* of Paris to continue their weekly meetings with perfect regularity during the bombardment. While everything else was turned upside down, while a dynasty was passing away, while sons and brothers were perishing around them, an enemy at their gates, want within their walls, and missiles of war threatening themselves and their household gods, these men continued their usual studies. We are reminded of Archimedes at the siege of Syracuse, save that there we have but one man, while here it is a large body of the intellectual flower of the country. Some of the more active of the members are men who have attained that philosophic calm, which not even the terrors of war can dispel, nothing diverts them from the even tenour of their way—

Si fractus illabatur orbis,
Impavidum ferient ruinæ.

We frequently meet with the names of Dumas, Elie de Beaumont, and Chevreul in the *Comptes Rendus* published during the siege. The youngest of these men was born in the last year of the last century; they have seen every phase of Parisian life, are men of infinite experience and learning, the very soul of the Academy; they have held office under various Governments, have seen more than one revolution; barricades and street fighting, and the Parthian cap are no novelty to them; but with all their experience they had never beheld a bombardment of Paris; yet, all honour to them, they did not abate one jot of their Academic work. Perhaps the members may have felt it a relief to have to deal with immutable and indestructible facts, while everything around them was so mutable and perishing. Perhaps they remembered a saying of one of their countrymen:—" L'homme n'a pas de self-critérium. L'indestructibilité du fait est le critérium unique, infaillible, absolu, multiple, un, présent dans tous les ordres des connaissances."*

The papers read before the Academy during the period of the siege relate for the most part to matters connected with war and to the food resources of a besieged city. M. Payen writes on hippophagy, M. Frémy on the use of osseine as food, M. Riche on the use of black puddings of ox-blood. In the *Revue des Cours Scientifiques* we also find some important papers by M. Bouchardat on the food supply of Paris, and a paper by the same author on the sanitary condition of the city during the siege, and during the same months of the preceding year. M. Berthelot has contributed some important papers on the force of various explosive substances, both solid, liquid, and gaseous. These papers are well worthy the attention of our war authorities. There is also a paper on dynamite, by M. Champion, and on the ignition of gunpowder at a distance by means of electricity. The subject of balloons and ballooning, of course, engages a good deal of discussion. M. Marey contributes several important papers on the motions of birds during flight, accompanied by graphic representations of them, registered somewhat after the manner of the vibrations of a tuning fork, and shown by sinuous lines. The diagram representing the vertical oscillations of a wild duck during flight is very striking. Beyond these papers there is nothing of much importance.

Here, for example, are the principal papers of one number (December 5th) taken at random:—M. Milne-Edwards discusses the nutritive value of organic substances contained in bones, and the proper rations for sustaining the human body in a perfect state of health; M. Chevreul makes observations upon M. Frémy's paper on the use of osseine as food; M. Gazeau details various experiments on the nutritive properties of cocoa leaves; M. Montier treats of the specific heat of gases under constant volume; M. Riche of the preparation of osseine and gelatine; and M. Castelhaz of the refining of crude tallow.

The future historian of science will wonder when he reads in the *Comptes Rendus* for January 9, 1871:—

M. Chevreul donne lecture à l'Académie de la déclaration suivante :

"Le jardin des plantes médicinales, fondé à Paris par édit du Roi Louis XIII. la date du mois de janvier, 1626.

"Devenu le Muséum d'Histoire naturelle par décret de la Convention du 10 de juin 1793.

"Fut bombardé,

"Sous le règne de Guillaume I^er roi de Prusse, Comte de Bismark chancelier,

"Par l'armée Prussienne, dans la nuit du 8 au 9 de janvier, 1871.

"Jusque là, il avait été respecté de tous les partis et de tous les pouvoirs nationaux et étrangers.

"E. CHEVREUL, *Directeur*."

He will grieve when he reads " M. Le Président annonce à l'Académie la douloureuse nouvelle, malheureusement très-probable, de la mort du . . . ." occurring too often in what should be only a record of the living and of their work. So we grieve: and yet more when we see the intellectual resources and energies of a great country paralysed, and the whole current of its active thought diverted no man knows whither; while its schools and colleges are empty, and many of those who should be in them have been killed untimely to satisfy the necessities of war.

G. F. RODWELL

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* *Philosophie Méthodique*, par J. de Strada, a work too little known in this country.

AMERICAN NOTES

WE learn from *Harper's Weekly* that the fourth volume of the "Report of the Geological Survey of Illinois," which has been in progress for several years past, under the direction of Prof. A. H. Worthen, has just been published. Like its predecessors, it is a handsome book, well illustrated, and containing much important matter relating to the geology and physical features of the State. This volume is occupied by a detailed account from assistants in the survey in regard to particular counties in the State, followed by systematic papers—one upon the fossil fishes by Prof. J. S. Newberry and Prof. Worthen, and the other upon fossil plants by Prof. Lesquereaux, illustrated by a number of well engraved steel plates. The economical value of such a survey to the State, conducted with the spirit and efficiency which have marked Prof. Worthen's administration from the beginning, is exceedingly great, and cannot be estimated in dollars and cents. Not merely does it furnish a fund of information to the residents of Illinois, but it supplies an official guarantee to others in regard to the resources of the State which could not be obtained in any other way.—We have, in a previous article, referred to some interesting speculations by Prof. Shaler, of Cambridge, upon the formation of the New England coast; and we find in the proceedings of the Boston Society of Natural History for February last some additional remarks by him on the same subject. He considers that the Chesapeake and Delaware bays, like many of the deep gorges in Switzerland and elsewhere, were formed by the action of ice, and that the existence of Cape Hatteras is due to the uplifting of the rocks on which Richmond is situated. The sand-bars on the coast he believes to have been formed by the material dug out of the Delaware and Chesapeake bays by this ice action, and worked southward by the united force of the floods and currents. He finds that, after we pass these bars, south of Weldon, the sea-bottom is totally distinct in character, being purely submarine, and formed by the action of the sea. He points out the existence of a rise and fall of the coast at different portions of its extent; this, in the most recent geological period, amounting at Charleston, South Carolina, to from 50 to 60 feet, in Maine to 200 feet, and to a still greater extent on the coast of Labrador. As a general rule, he thought there was evidence to prove that, taking a line from the centre of the continent to the centre of the sea, the sea-floor was coming up and the high elevations were coming down.—Mr. Hyatt states that observations made by the Coast Survey showed that the coast of Long Island Sound, and southward to New Jersey, has been sinking, while the Florida Keys are rising; and Mr. Niles remarked that, from the earliest times, in the Adirondacks and different points southerly, there had been peninsulas corresponding in position to Florida, and that this is simply the most southern and latest of a succession from north to south.—At a meeting of the New York Lyceum of Natural History, held during last autumn, Prof. Newberry, the President, exhibited the anterior portion of the cranium of a walrus which had been found during the summer at Long Branch by a gentleman whose foot struck against it while bathing. It was strongly silicified, but exhibited no appreciable difference from modern specimens. The precise age of this fossil could not, of course, be ascertained, although it is well known that its range was formerly much south of its present habitat. It is not unfrequently brought down on floating ice off the coast of Newfoundland; and although Labrador is at present the southern limit of its residence, it was once very abundant in the Gulf of St. Lawrence, and its remains have been found in the shell heaps of the Bay of Fundy. It is probable that the specimen exhibited by Prof. Newberry is a relic of the glacial period, although it was suggested that it might have been of the tertiary age, which probably cannot be verified. Other specimens of similar character are recorded as having been found on Martha's Vineyard; in Monmouth Co., New Jersey; and in Accomac Co, Virginia.—We have already referred to the detection of an ancient bone cave near Phœnixville, Pennsylvania, and about twenty-five miles north-west of Philadelphia, and to the interest which has attached to this discovery. Since our last account Prof. Cope has been actively engaged in the investigation of the collection, and already reports the existence of about thirty species of vertebrates, together with numerous plants and insects. All of these, so far as known, are probably of extinct species, although their precise relationships have not yet been fully worked out. Among the reptiles were tortoises and serpents, and of birds there was a turkey and a snipe. The mammals, as Prof. Cope anticipated, were most numerous, these including two carnivorous animals of large size, one of them a cat, and the other a bear, previously described by Dr. Leidy, of a remarkable type, and totally distinct from the cave bear, or any living species of either Europe or America. At least three species of sloths were discovered, mostly of gigantic size, one of them a species of *Megalonyx*, and two of *Mylodon*. Besides these, there were some ruminating animals, tapirs, and a small horse. With the other remains were the teeth and tusks of the mastodon. The fissure in which the bones were found was forty feet deep and fifteen feet wide; the length as yet has not been determined. Above the deposit of bones the cave was filled with washings of the Triassic age from the neighbouring hills.

SCIENCE AT OXFORD

THE following courses of lectures in Science are announced for the ensuing term:—The Regius Professor of Medicine (Dr. Acland) will continue his clinical instruction at the Infirmary, on Tuesdays and Saturdays, beginning on Tuesday, May 2, at 11 A.M. The Linacre Professor of Anatomy and Physiology (Dr. Rolleston) proposes to form classes for practical instruction in anatomy and physiology, as in former terms. Persons are invited to come to the anatomical department in the New Museum on two mornings of the week for study and demonstration, and to lectures on Saturdays at 1 P.M., and at such other times as may be hereafter arranged. The Sedleian Professor of Natural Philosophy (the Rev. Bartholomew Price) will give a course of lectures on the Solution of Problems in Applied Mathematics. The course will begin at 1 P.M. on Thursday, April 20, in the lecture-room, upper corridor south, Museum. The Savilian Professor of Astronomy (the Rev. C. Pritchard) will give a lecture early in the present term on the recent solar eclipse. He will also be ready to assist members of the University in their astronomical studies, "*sine ullâ solennitate.*" The Savilian Professor of Geometry (Mr. Henry Smith) will continue his lectures on the Anharmonic Properties of Figures. He will also give a course of lectures on Geometry of Three Dimensions. The Professor of Geology (Mr. J. Phillips) will begin a course of lectures on Monday, April 24, at 12 o'clock, and continue them at the same hour on succeeding Wednesdays and Mondays, in the New Museum. The lectures are arranged to present a systematic view of the more remarkable groups of organic remains, especially in the extinct groups. The Professor of Experimental Philosophy (Mr. R. B. Clifton) has given notice that the Physical Laboratory of the University will be open daily for instruction in practical physics, from 10 A.M. to 4 P.M., on and after Wednesday, April 19. The fee for working three days a week during the term is 3*l*. The Professor of Chemistry (Sir B. Brodie) has given notice that the Demonstrator in Chemistry will deliver a course of lectures on Chemistry on Tuesdays and Saturdays, at 11 A.M., commencing Tuesday, April 25. These lectures will be in continuation of the course of the Professor of Chemistry last term, and will commence with the element phosphorus and its combinations. The Professor of Botany (Mr. Lawson) will give a course of lectures on Structural and Physiological Botany. The Hope Professor of Zoology (Mr. Westwood) proposes to give a short course of lectures on the Articulated Animals.

SETTLE CAVE EXPLORATION

AT the last meeting of the Settle Cave Exploration Committee, the report of the excavation of the Victoria Cave, up to December 31st, 1870, by Mr. Boyd Dawkins, was read.

The results of the exploration are full of interest, and bid fair to throw light on the social condition of the Romano-Celtic inhabitants of Ribblesdale after the withdrawal of the Roman legions. The committee placed the superintendence of their exploration in the charge of Mr. Jackson, the discoverer of the cave, and began work with the kind permission of the owner (Mr. Stackhouse), by clearing away a mass of debris, which very nearly blocked up the lower entrance and formed a horizontal plateau extending some thirty feet from the solid rock. On the surface there was a stratum of angular stones which had fallen from the weathered face of the rock above, and passed into the cave at the bottom of the Attermire ravine. Below was the layer which furnished traces of man. Mixed with charcoal there were large quantities of the bones and teeth of the Celtic short-horn (*Bos longifrons*), goat, and horse, and a few remains of red deer and roe deer, which were evidently the refuse of human food. There were also fragments of pottery, bone pins, and various nondescript articles in antler and bone, stone pot-boilers, and two perforated discs of stone which had been used as spindle-whorls. As this layer passed into the cave, it rose to the surface and continued to furnish the same class of remains as those found outside. The personal ornaments are the most worthy of note. Besides bronze harp-shaped brooches of the common late Roman type, there were two in gilt-bronze, of a sigmoid shape, and adorned with a singularly beautiful pattern in blue, yellow, red, and green enamel. They are undoubtedly of a style purely Celtic. A harp-shaped brooch, Roman in design, is adorned with a most delicate pattern in blue and red enamel. Among the other brooches, one small oblong enamel is of a form hitherto unknown in brooches of this date, while a second consists of a small disc of bronze, with a plate soldered to it bearing a flamboyant ornament of Celtic design. There were also bronze gilt armlets. The whole evidence furnished by the personal ornaments in a word points to their Romano-Celtic origin, and it is not improbable that the principal seat of the art of enamelling was Yorkshire, the few enamels of that particular kind which have been found occurring, with one or two exceptions, in that county. The date of the occupation is shown by the coins, which range from Trajan down to the barbarous imitations of the coins of Tetricus. The latter were in circulation in the fourth and fifth centuries, and probably continued to be used in that portion of the Romano-Celtic kingdom of Strathclyde down to its final conquest by Eadberht in 756 A.D. The whole group of remains is precisely of the same character as those found around the ruins of Roman villas in Britain, and has probably been introduced by Romano-Celtic inhabitants who fled from their luxurious homes to take refuge from the ravages of the Picts or Scots, or of the Northumbrian Angles, who were pressing on that portion of the frontier of Strathclyde during the 5th, 6th, and 7th centuries. To suppose that people using articles of luxury such as those found in the cave would have chosen such an inclement abode, except under the pressure of necessity, is unreasonable.

At the entrance below the Romano-Celtic strata a layer of angular debris fallen from the cliff above, six feet thick, rested on a thick deposit of gray clay. At their point of junction a curious bone harpoon, a bone bead, the remains of red deer, horse and Celtic short-horn, and of bear, were found, which testified to the occupation of the cave by man long before the Romano-Celts used it as a refuge. The two flint flakes and two lumps of red ruddle found were probably obtained from this lower horizon, which, as the talus died away at the entrance of the cave, became confused inside with the Romano-Celtic stratum immediately above. It is probably of Neolithic age.

The grey clay underneath was homogeneous and very tenacious, and as the layer dipped away from the entrance, it must have been introduced by water flowing from the ravine into the cave. It was resolved to give up the attempt to fathom this bed of clay, after sinking a shaft twenty-five feet deep without any results.

The committee are desirous of exploring others of the many caves in the neighbourhood, if they can obtain support necessary to carry on a work which is of almost equal interest to the archæologist and to the historian.

NEW SPECIES OF MADREPORE

MR. W. SAVILLE KENT read a paper at a recent meeting of the Zoological Society on various new species of Madrepores, or Stony Corals, met with by himself while engaged upon arranging, naming, and cataloguing the fine series contained in the British Museum. Among the more interesting of these, commencing with the family of Turbinolidæ, Mr. Kent drew attention to a fine species of *Acanthocyathus* from Japan, more closely allied to a Maltese Miocene form (*A. Hastingsæ*) than to any known existing one; and also to a *Flabellum* allied to *F. Anthophyllites*, whose most remarkable feature rests in the phenomena connected with its reproduction by the process of gemmation, which invariably results in the destruction of the parent; the reproductive bud always originating within the margin of the parent calyx, which, in the course of its development, it splits to pieces. For this aberrant form Mr. Kent proposes the appropriate name of *Flabellum matricidum*. In the family of the Oculinidæ, which comprises the majority of the species introduced by Mr. Kent, are three new forms of *Allopora*, and numerous ones of *Stylaster*, *Distichopora* and *Amphihelia*, the first-named genus in particular containing a magnificent arborescent species, upwards of a foot in height, of a delicate rose colour, having a stem of such thickness and of such dense consistence that Mr. Kent is of the opinion that, if procurable in any quantity, it may eventually prove of high economic value, and even replace to some extent the well-known *Corallium rubrum*. The examination of these new varieties has enabled Mr. Kent to define more precisely the characters of *Allopora*, and its true distinctions from *Stylaster*, *Distichopora*, and other allied genera. In all, Mr. Kent introduces some twenty species as new to science.

SUBTERRANEAN ELECTRICAL DISTURBANCES

A FEW minutes before and after the earthquakes of the 17th March last powerful positive electrical currents were rushing towards England through the two Anglo-American telegraph cables, which are broken near Trinity Bay, Newfoundland. Mr. C. F. Varley, C.E., who informed us of the fact, broaches the novel speculation that some earthquakes may be due to subterranean lightning. He imagines that as the hot centre of the earth is approached, a layer of hot dried rock may be found which is an insulator, while the red hot mass lower down is a conductor. If this conjecture be true—and there is plausibility in it—then the world itself is an enormous Leyden jar, which only requires charging to a very moderate degree to be equal to the production of terrific explosive discharges.

The French Atlantic cable was disturbed at the same time, and so were many of the English land-lines, but the only observations as to the direction of the current were made by means of the Anglo-American telegraph cables.

A number of Mr. Varley's charts about earth-currents were published in the Government Blue Book of 1859-60, showing that the direction of these currents across England was in a very notable degree determined by the contour of the coast, and that the same auroral discharges would often produce currents at right angles to each other in direction, in different parts of Britain.

NOTES

A PROPOSAL has been made that certain Medical Schools on the north and south sides of the river should be amalgamated, in order that, by concentration of power, the teaching shall be made more efficient than it is at present, the teachers being able to devote themselves more unreservedly to their duties than they possibly can do under existing arrangements. The absolute necessity of some such arrangement as this is obvious.

MR. RUTHERFORD and M. Janssen, to whose labours cosmical physics owes so much, are both now in this country, the former, we regret to learn, in consequence of a peremptory order to cease work for a time. At the last meeting of the Royal Astronomical Society, Mr. Rutherford exhibited his exquisite photograph of the Pleiades, which represents the last important outcome of celestial photography. It appears that M. Janssen's observatory for solar research, which had been erected in one of the pavilions of the Palace of St. Cloud, at the cost of the Emperor, was one of the first buildings to be entirely destroyed by the German fire.

THE Royal Commission on Scientific Instruction and the Advancement of Science will resume its sittings on the 25th instant.

THE Marine Aquarium at the Crystal Palace is beginning to take form, thanks to the energy of Mr. Lloyd. About one-third of the 700 tons (= 150,000 gallons) of the sea water required is already in the tanks, but the steam engines and pumps are not yet regularly at work. When they are, and some of the marine animals of which at present the aquarium is quite destitute shall have been received, we hope to give a description of an enterprise of which great scientific use can certainly be made.

WE learn from the *Chemical News* that the Council of the Federal Swiss Polytechnic School at Zurich has appointed Dr. Emile Kopp as Professor of Technology in the room of the late lamented Dr. Bolley. Professor Kopp's merits, during the time he has held the Professorship at the Institute Superiore, at Turin, have been gracefully acknowledged by the Italian Government, it having pleased H.M. King Victor Emmanuel to grant to Dr. Kopp the dignity of Commandeur de l'Ordre Equestre de la Couronne d'Italie. We sincerely congratulate Dr. Kopp, and no less so the Government of the Helvetian Republic, on having secured the services of a man so eminently well suited as Dr. Kopp to the important Professorship vacant by the demise of Dr. Bolley.

THE French Academy of Sciences held a short sitting on April 3, M. Faye, who usually occupies the chair, being, however, absent. The principal subject of interest was a sharp passage of arms between M. Delaunay and M. St. Claire Deville respecting the Montsouris Observatory. The Commune having ordered the arrest of M. Henri St. Claire Deville, the Professor of Chemistry in the University, as a hostage, he has been compelled to make his escape to Versailles, together with his brother, the meteorologist. M. Leverrier is also now at Versailles. Dr. Bersigny, who conducted the observations during the Prussian occupation, will be the director of the meteorological observations conducted by the Government.

THE second *soirée* of the Royal Society takes place on Saturday evening next, and that of the Linnean Society on Wednesday the 26th inst.

WE understand that there is likely to be a contest for the seat n the Senate of the University of London, vacant by the death of Prof. Miller. The nomination would, in accordance with previous practice, be left this time to the Faculties of Science and Medicine, and their representative is Dr. Parkes, F.R.S., of Netley, who is also supported by many graduates in Arts.

THE course of lectures during the Easter term at the Gresham College, by Dr. E. Symes Thompson, Gresham Professor of Medicine, will be on April 21, 22, and 24, the subjects being, "On the Small-pox Epidemic," "On the Organs of Respiration," and "On the Organs of Circulation." These will be followed by two lectures on May 24 and 25, "On the Water-supply of London," and "On the Heart and Lungs in Health and Disease. As usual the lectures are free to the public.

IT is rumoured that a College for Women is to be established t Cambridge, in order to give the students the advantage of ttending the lectures, &c., of the University Professors.

THE following are places of Comet I., 1871, discovered y Dr. Winnecke, for the dates named:—1871, April 7, $^{h}\,50^{m}\,31^{s}$, M. T. Karlsruhe; AR app. ☄ = $2^{h}\,27^{m}\,14^{s}{\cdot}59$ Decl. app. ☄ = + 53° 53′ 8″·1. April 8, $9^{h}\,39^{m}\,43^{s}$, M. T. Altona; AR app. = $2^{h}\,32^{m}\,8^{s}{\cdot}21$; Decl. app. = +53° 24′ 41″·1. April 9, $9^{h}\,56^{m}\,25^{s}$, M. T. Altona; AR app. = $2^{h}\,37^{m}\,2^{s}{\cdot}50$ Decl. app. = +52°, 55′ 31″·0.

IN a recent number of Poggendorff's *Annalen*, Dr. Weinhold states that the black absorption line of sodium can be easily obtained by a simple process. The usual method has been to interpose a flame, coloured with chloride of sodium, between a strong light, such as the electric light, and the slit of a spectroscope. The source of light now proposed by M. Weinhold is an ordinary petroleum lamp; the light is allowed to pass through a slit directly on to a prism, and a spirit lamp flame, intensely coloured with chloride of sodium, interposed between the prism and the eye, so as to cover the entire spectrum;—the black absorption line will then be seen distinctly. If the flame coloured with sodium is placed in front of the slit, the bright yellow line will be seen as usual. M. Weinhold has not been successful in using this method with an ordinary spectroscope fitted with telescopes, on account of various practical difficulties.

M. GABRIEL MORTELLET, the sub-conservator of the St. Germain Museum, wishes us to correct a statement which seemed to imply that it was open during the Prussian siege. This was not the case, although the works were not interrupted.

THE temperature of February and March shows a remarkable contrast to that of the two preceding months. Mr. Glaisher's tables, published in the *Gardener's Chronicle*, show that at Blackheath on forty-seven days during these two months the temperature was above the average, while it was below the average on only twelve days, the mean being 3°·55 above that of the same period during the last fifty years. The disturbance of equilibrium caused by the low temperature of December and January has thus almost been restored. During Feburary the thermometer only fell to the freezing-point on five nights, and during March east winds prevailed to only a very moderate extent, showing a very marked contrast to the same month last year. The depression of temperature which commenced on March 28 continued to April 11, when there was a sudden rise from 4°·1 below the average to 11°·4 above the mean on April 12.

THE publication is announced of a new series of the "Messenger of Mathematics." It will be edited by Messrs. W. Allen Whitworth, C. Taylor, W. J. Lewis, R. Pendlebury, and J. W. L. Glaisher, and the first number will be published by Messrs. Macmillan on May 1st. The editors consider that the "Messenger" has, during the five years it has already existed, amply fulfilled its object of encouraging original research in the three Universities among junior graduates and others, although no inconsiderable portion of its contents has been supplied by writers of established reputation, who rank amongst the foremost mathematicians of the age; and it is this fact in particular which now induces them to appeal directly to the mathematical world at large, and to remove from their title-page any words which might be supposed to limit the sphere of usefulness of the "Messenger." They are therefore prepared to receive communications from every available source, and have already to announce papers, forwarded or promised, by Prof. Cayley, Chief Justice Cockle, Mr. Routh, Mr. Esson, and others. Besides original papers, it is intended to insert brief notices of select articles or treatises on mathematical subjects, as well as short accounts of the proceedings of societies at home or abroad.

THE second series of "Science Lectures for the People," delivered in the Hulme Town Hall, the first of which we have already had occasion to notice in terms of commendation, contains reports of lectures by Professor W. C. Williamson on "The Natural History of Paving-Stones," Dr. W. B. Carpenter on "The Temperature and Animal Life of the Deep Sea,' Mr. A. H. Green "More about Coal," and Mr. Norman Lockyer "On the Sun." It is published by Mr. John Heywood, Deansgate, Manchester, and Messrs. Simpkin, Marshall and Co., London.

THE second part of the fifth volume of the "Proceedings of the Bristol Naturalists' Society," from May to December 1870, does not contain much original matter. The longest paper, by Messrs. Sorby and Butler, "On the Structure of Rubies, Sapphires, Diamonds, and other Minerals," has already appeared in the "Proceedings of the Royal Society." The paper "On the Natural History of Filey and its Neighbourhood," however excellent in itself, hardly appears to us in place among the proceedings of a local society on the other side of England. Mr. C. F. Ravis contributes a useful paper, "On Denudation, especially as exhibited in the Valley of the Avon," and there are some good local geological and entomological notes. The zoological and botanical sections present no report.

COLONEL HAIG, R.E., of the Godavery Navigation Works, has found coal near Dumagudlom, in the Madras Presidency, 130 miles from Kokonada. It is not so good as English coal for locomotive purposes, but is good enough for welding in the Government workshops.

MR. MALLET, of the Indian Geological Survey, has been examining Aden with a view to water supply. He considers there is no hope from Artesian wells, but recommends water to be brought from wells near Mahillá at the Sheikh Othman aqueduct.

THE culture of bamboo for paper making and other purposes is being promoted by the Government of Central India. The Indian Government is buying land in the hill district of the Neilgheries for a spice plantation. Col. Boddam has proposed the cultivation of the sunflower in Mysore. It is very successful in France. Government has sent out six more Scotch gardeners for experimental cotton growing. These men have answered very well.

THE Broom (*Sarothamnus scoparius*) is extremely abundant in Madeira, but is supposed to have been originally introduced to the island. It is now sown extensively on the mountains for the purpose of being cut down for firing, or burnt on the spot every five to seven years to fertilise the ground. The twigs and more slender branches are also used commonly as withs for binding bundles of faggots, brushwood, fern, &c.; and numbers of country people, especially young girls and children, residing within reach of Funchal, gain a livelihood by bringing daily into the town bundles of broom for use in heating ovens, &c. The fine and delicate basket-work peculiar to Madeira is manufactured from the slender peeled twigs of this plant. Mr. Lowe speaks of a variety with pure white flowers which occurs on this island.

THE Apocynaceæ are noted for their poisonous properties, and one of the most interesting plants of the order is *Cerbera odollam*. It is a tree about 20ft. high, growing about Malabar and Travancore, and produces a drupe-like fruit as large as a fine mango. The inner shell of the fruit, after removing the green rind, is composed of a mass of reticulated woody fibres, and when dry is not much unlike a ball of coarse, dirty string. These fruits are said to be used by the natives in Travancore to poison dogs, for this purpose they are taken when green and covered with sugar or some sweet substance, by which the dogs are tempted to eat them. The effect of the poison is to cause the teeth of the animals to become loose or to fall out before death ensues.

A NEW kind of stuffing for mattresses appears to be coming in vogue, for we learn from the *Journal of Applied Chemistry*, "that sponge, dipped in glycerine and well pressed, remains elastic, and can be used for mattresses, cushions, and general upholstery," and we further learn that "sponge mattresses prepared in this way, are now finding great favour."

THE *Pharmaceutical Journal* draws attention to an article which originally appeared in the *American Journal of Microscopy*, on the value of the microscope to the pharmacist in the detection of parasitic animalculæ or fungi in drugs of vegetable origin. We all know that leaves, roots, and seeds deteriorate very much by being kept any length of time, therefore the remarks of the author are of the utmost importance to mankind generally. He says "that it is notorious that the most carefully prepared tinctures and extracts of certain drugs are sometimes devoid of medicinal power. It has been supposed that certain volatile constituents escape from the substances from which such tinctures are prepared, but of this we have no certain proof. Why is it that the leaves of belladonna may, in some instances, be kept for years, and at the end of that period be capable of yielding a reliable preparation, while other specimens, when kept only a few months, are worthless? It must be because of some destructive process going on in the substance, which cannot be discovered with the naked eye." The hints which follow are even worth the attention of those upon whom we depend for our medicinal preparations. "The pharmacist should first learn to recognise the natural healthy appearance, under the microscope, of all the vegetable substances he works upon; then he should subject a specimen of every substance he prepares to a careful examination, and if he discovers the presence of vegetable or animal parasites, such substances should be rejected. The world is flooded with inert medicinal preparations. Doubtless many such preparations are made worthless by improper methods of manufacture; but it is my opinion that in many instances their worthlessness is due to the fact that the substances used have been injured by certain agencies which could have been discovered by the intelligent use of the microscope."

THE *Eucalypti*, or Gum-trees of Australia, are well-known for their hard wood as well as for the oils and gums yielded by many of them. Some of the species have been introduced and successfully grown in different parts of Europe, and their products may become, ere long, recognised articles of import. At the present time large quantities of sticks of a species of *Eucalyptus* are imported into England from Algeria, and are made into walking sticks. During the Great Exhibition in Paris in 1867, the leaves of *Eucalyptus globulus* were made into cigars, and recommended as being very efficient in aiding digestion. We now learn from the *Gardeners' Chronicle* that the leaves of a species of *Eucalyptus* have been recently used on the Continent in place of lint, the leaves being merely laid on the wounds. "Their balsamic nature not only cures, but, after a few hours, all unpleasant odour ceases."

THE prickly poppy (*Argemone mexicana*), originally from the New World, has become naturalised throughout the tropics and sub-tropics of both hemispheres. In the West Indies, where it is very abundant, it is called *Fico del inferno*, the reason for this name being, according to Gerarde, "because of his fruit, which doth much resemble a figge in shape and bignesse, but so full of sharpe and venemous prickles that whosoever had one of them in his throte doubtless it would send him packing either to heaven or to hell." Barham, however, gives, as an explanation, a statement that the seeds, "being much stronger than opium," are "enough to send any that should take them wilfully to *inferno*; but this is much exaggerated. In India it is now abundantly naturalised in the eastern part of the Punjab, and is spreading over fresh districts year by year. It is not altogether a useless addition to the Indian flora, as near Delhi an oil is extracted from the seeds which is used for burning, as well as in the treatment of chronic sores and eruptions.

ECONOMIC ENTOMOLOGY

THE Royal Horticultural Society has just offered the following prizes for collections of Economic Entomology:—

1. A prize of 10*l*. for the best collection of British insects injurious to any one plant, as the oak, pine, cabbage, wheat, &c. (the choice of the plant to be left to the competitor). The insects to be shown as much as possible in their various stages of development—eggs, larva, chrysalis, and perfect insect. In judging, a preference will be given to those collections which most successfully illustrate the life history of the insect, and exhibit the mischief done, whether shown by specimens, drawings, models, or other means. Examples of the application of drawings, models, and specimens to this purpose may be seen in the Society's collection in the South Kensington Museum.

2. A second prize of 3*l*. for the second best collection.

3. A prize of 5*l*. for the best miscellaneous collection of any branch of British Economic Entomology, similarly illustrated.

4. A second prize of 2*l*. for the second best collection.

The collections to be sent to Mr. James Richards, Assistant Secretary, Royal Horticultural Society, on or before the 1st of May, 1872, each collection bearing a motto, and a separate sealed envelope, with the motto on the outside, and the name of the competitor inside.

The Society is to be entitled to take from any of the collections sent in, whether successful or not, whatever specimens or illustrations they may choose, at a price to be fixed by the judges.

The judges to have power to refrain from awarding the prizes, should the collections seem not worthy.

SCIENCE TEACHING IN ORDINARY SCHOOLS

A PLAN for teaching the Natural Sciences in ordinary schools has been submitted to the School Board for London by Mr. J. C. Morris. The following are stated by the *Journal of the Society of Arts* to be the principal points of the proposed system:—Subjects—chemistry, heat, light, sound, electricity, magnetism, telegraphy, mechanics, hydrostatics, steam engine, &c.; geology, metallurgy, botany, zoology, animal physiology, health, &c. A committee should be formed to select, revise, and compile a complete set of suitable text books, which should bear their sanction, and be then published in the cheapest possible form. There should be a depôt to provide apparatus at a cheap rate, a complete set of which, sufficient to illustrate the sciences mentioned, would not cost more than 100*l*., and should be divided into ten cases of 10*l*. each, a case to be complete for one or two subjects. The teacher should be a visting one. He could attend from two to three schools per day, and give from one to two hours' instruction in each, during two days in the week. The instruction to be given in a separate apartment, if there be one; or, if not, at such a time as would not interfere with ordinary school business. A single school teacher could thus attend from six to nine schools weekly, if sufficiently near each other, and get through at least three or four subjects annually, so that in two or three years he would have completed the full course in each school.

There should be an institution where teachers would have an opportunity of acquiring a practical knowledge of their profession, and affording a means of testing their qualifications. A more economical way, however, would be for each teacher to have an assistant, by which method a nucleus of teachers would soon multiply into a goodly number. In conclusion, Mr. Morris advocates periodical examinations, with a regular system of rewards; and, in reference to funds, he thinks that the teachers and inspectors might be supported either by Government or subscription, the apparatus to be supplied by Government at reduced rates. Schools should fix a small fee for attending the class, which would add to its importance, and help to defray expenses. Examination fees in like manner. Evening classes for adults could be managed under somewhat similar conditions.

THE INFLUENCE OF AQUEOUS VAPOUR ON METEOROLOGY

THE remarks on Meteorology contained in your summary of the scientific advances during the last year, encourage me to offer a few observations on the subject. Where so little is determined, speculations even by unknown contributors may receive some consideration. I shall begin with the subject of aqueous vapour, to which, I think, too much importance has in some respects been attached as a meteorological agent.

I shall commence by offering a determination of the specific heat of aqueous vapour at constant volume, which has not, to my knowledge, been hitherto given. It is evident that if a given weight of water be evaporated at 0° C., and then raised to 100° C., without change of volume, the total heat absorbed by it is the same as if it had been raised to 100° C. in the condition of water, and then evaporated so as to fill the same volume. Now if this amount be one kilogramme, and c be the specific heat of vapour at constant volume, the total heat absorbed in the first case is (adopting Regnault's formula for latent heat—viz., $L = 606^\circ{\cdot}5 - 0{\cdot}695\,t$)—

$$606^\circ{\cdot}5 + 100\,c.$$

And in the second case the equivalent amount is

$$537^\circ + 100{\cdot}8 = 637{\cdot}8,$$

the total amount of heat required to raise one kilo. of water from 0° to 100° C. being 100·8, according to the best determinations.

From this equation we obtain for the mean value of c between 0° C. and 100° C.

$$c = 0{\cdot}313$$

But the specific heat of aqueous vapour at constant pressure seems to be about 0·475. Hence for each kilo. of aqueous vapour raised through 1° C. at constant pressure, we have 0·313 heat units expended on internal work, and 0·162 on external work. The proportion is 1 : 0·517. In dry air the proportion in question, according to the latest determinations, seems to be 1 : 0·421. If the same amount of heat therefore be applied to produce expansion in vapour and in dry air, it produces a greater expansion in the former case. But the difference is not very material. In round numbers 30 per cent. of the absorbed heat is employed in producing expansion in one case, and 35 per cent. in the other. Apart from the different absorptive powers of air and vapour, this difference would be hardly perceptible. For equal quantities, heat absorbed by vapour has little (if any) greater effect in producing air currents or barometric depressions than heat absorbed by air. If the heat is absorbed in *producing* evaporation, the effect is still less. It is well known that less than one-eighth of the heat so absorbed goes to produce external work—a much smaller proportion than if it had been absorbed by dry air. I may observe that the result here arrived at for aqueous vapour supposes that its co-efficient of expansion between 0° C. and 100° C. is greater than that for air in the proportion of nearly 7 to 6. This would make the mean co-efficient about 0·00427 for 1° C. between these limits.

The next thing to ascertain is the quantity of aqueous vapour suspended in the air at any given time. This, of course, cannot be ascertained exactly, but it seems to me to be wholly erroneous to measure it by the tension of vapour at the earth's surface relatively to the pressure of dry air. It is generally supposed that the upper strata of the atmosphere are relatively drier than the lower; but even if we suppose them to have an equal relative humidity, the actual vapour-tension will become less in proportion to the tension of dry air at every step of the ascent. According to Sir J. Herschel, the law by which the temperature varies with the pressure of the air at any elevation is given (in Fahrenheit degrees) by the law

$$t = -87^\circ + 9{\cdot}0667\,p - 0{\cdot}1333\,p^2$$

Assuming this law, and supposing the relative humidity constant, we can calculate the temperature corresponding to any given pressure, and then find the corresponding vapour-tension by reference to the table. By trying this for each separate inch of pressure from 30 inches down to 0, and calculating the vapour-tension in each inch, I find (on a rough approximation) that the average proportion of the vapour-tension to the dry-air pressure will not exceed one-half of that which we find at the earth's surface. Thus, when the vapour-tension at the earth's surface is half an inch in 30 inches, or $\frac{1}{60}$, the average vapour-tension throughout the aerial column does not exceed $\frac{1}{120}$ of the whole; and when we calculate the weight of the superincumbent vapour, we must further allow for its smaller specific gravity. Making this correction, I believe that when the tension of vapour at sea-level is half an inch, the real weight of the superincumbent vapour column seldom exceeds that of one-sixth of an inch of mercury. The proportions of course are not fixed. Those which I take from Herschel answer best for a temperature at the earth's surface of about 65° F., or 18° C.

I intend to apply these observations chiefly to the explanation of the annual and diurnal variations of the barometer by the greater or less amount of aqueous vapour in the air. It is supposed, for example, that when the vapour-tension at the earth's surface is an inch, about $\frac{1}{30}$ of the whole air-column consists of vapour. This displaces an equal bulk of air, and thus the column is lighter than a dry-air column of the same height and temperature by the difference in weight of the air and vapour occupying this space, *i.e.* about $\frac{3}{8}$ of an inch, or $\frac{1}{80}$ of the whole. But if my computation is correct, the diminution of weight owing to this cause would only be $\frac{3}{16}$ of an inch, or $\frac{1}{160}$ of the whole. Taking the standard pressure at 30 inches, this would only account for a diminution of 0·187 of an inch when the air from being absolutely dry changed to one inch of vapour-tension. In this country we never experience absolute dryness, and we seldom if ever experience so much as an inch of vapour-tension at the surface. Yet the annual variations of the barometer exceed 0·187 of an inch. Looking, for example, at the table for Greenwich Observatory at the end of Herschel's work, I find the mean monthly pressure varying between 29·923 and 29·602 inches, a difference of 0·321 inches; while the monthly means of vapour-tension vary between 0·466 and 0·195, a difference of 0·271; which, as I have endeavoured to prove, would only account for a change of 0·051 in the mean pressure. The diurnal maxima and minima at the same place exhibit a difference of 0·018, the vapour-tension giving 0·042, which only accounts for about 0·008.

The same thing is more evident in other places. At Madrid for example, the monthly barometric averages vary between 28·003 and 27·701, a difference of 0·302, while the vapour-tension varies between 0·236 and 0·076, a difference of 0·160. At Longwood, St. Helena, the diurnal variation of the barometer gives a mean of 0·067, while that of the vapour-tension is only 0·030. This would only account for a change of 0·006, or less than $\frac{1}{10}$ of the actual change. At Bombay the diurnal variation of the barometer amounts to 0·102 inches, while the corresponding variation of vapour-tension is only 0·004. This would not account for one hundredth part of the change.

The same result is confirmed by taking another view. It is pretty evident that in a country of any considerable extent the diurnal oscillation of the barometer (which is often nearly double the diurnal variation), if produced by changes of vapour-tension, must always be less than the mean diurnal rainfall and dew-fall, since rain often falls at hours when the barometer is on its diurnal descent. Now in this country the mean diurnal rainfall does not exceed $\frac{1}{15}$ of an inch, corresponding in weight to $\frac{1}{200}$ of an inch of mercury. Supposing all this to fall during the hours when the mean barometric pressure is increasing, the subtraction of that amount of aqueous vapour from the column, and the replacing of it by air, would not nearly account for the observed diurnal oscillations.

I therefore conclude that the annual and diurnal variations of the barometer are not due to changes in the amount of vapour present in the aerial column. Indeed it does not seem certain that the vapour displaces air at all. It may simply permeate that column without displacing any of it, and thus add to the weight of it. Again, if it displaced air, condensation or the formation of dense clouds ought always to be attended with a rise in the barometer, since air would rush in to fill up the space which the vapour vacated on condensing. This does not seem to be the case.

W. H. S. MONCK

THE ROYAL SOCIETY OF VICTORIA.

WE owe an apology to our scientific friends at the Antipodes for having allowed the president's address, delivered last July, to have remained so long unnoticed. Mr. Ellery, after noticing the most important papers that had been read during the past two sessions (for no address was delivered in 1870 in consequence of alterations being made in the Society's buildings), including eight on physical science, seven on geology, mineralogy, and palæontology, one on natural history, three on medical science, one on social science, and four on arts and manufactures, expresses his regret that the state of their finances has for a time caused a stoppage in the printing of their Transactions which were commenced in 1868. He then proceeds to notice the present state of the chief scientific establishments in Victoria. "Botanical knowledge," he observes, "is largely indebted to the labours of our member, Dr. Von Muller, the head of the botanical department of Victoria. One of the prominent results of Dr. Müller's investigations is the publication of the *Universal Flora of Australia* (under the editorship of Mr. Bentham), to which Dr. Muller is the principal contributor; the fifth volume has, by this time, passed the press in London. This work, when completed, will be the only one extant that deals universally with the flora of a large division of the earth's surface. It will form a permanent basis of all future research with respect to the adaptability of Australian plants to medicine, the arts, or other useful purposes. You will be glad to learn that Dr. Müller is about to establish a permanent phytological collection in our new industrial museum, which will comprise objects illustrative of our natural resources in the vegetable kingdom, and of materials used in the industries obtained from plants in this country as well as other parts of the globe. Such a collection properly arranged and accessible to the public will undoubtedly prove a valuable and instructive addition to the industrial museum, more especially if at the same time Dr. Müller fulfils a project he has in contemplation of publishing in a popular form a volume on the culture of utilitarian plants in the colony not indigenous to it, as well as of plants likely to add to the resources of countries lying under similar latitudes to our own. The preservation and perpetuation of our more extensive forests has already become a question of serious import. A few years ago we thought our forests inexhaustible; but already the bad effects of the indiscriminate stripping of our mountain ranges are becoming visible. The immense and increasing draft on our forests for fuel and other purposes has already denuded the land in the vicinity of towns and other centres of population of its former covering of timber. This, unless replaced by artificial planting, will eventually leave our hills bare, and in all probability the climate will suffer in proportion. Dr. Müller, in introducing and rearing very large numbers of forest trees that will be useful in themselves for the wood and bark, has exercised a wise forethought, of which the colony will reap the fruit in years to come, when the corks, oaks, hickories, red cedars, and firs, shall have in part replaced our eucalypti, mimosæ, and other far less useful trees."

"Our observatory," he adds, "has been engaged with its usual work in astronomy, meteorology, terrestrial magnetism, and general physics. Considerable progress has been made in the Melbourne portion of the survey of the southern heavens; the sky lying between the 60th and 52nd parallels of declination has been carefully surveyed, and the positions of 38,305 stars established and catalogued, of which 29,633 have been reduced to the epoch agreed upon, namely, 1875, and their positions at that date computed. Our staff of self-registering meteorological instruments may now be considered complete, and consist of three magnetographs (that is for declination, dip, and horizontal intensity), a thermograph, a barograph, electrograph, and anemograph. With these instruments a continuous and unceasing record is obtained by the aid of photography of all the variations in the force and direction of the earth's magnetism, of the temperature of the air, and of evaporation, of the state and variations of the pressure of the air, atmospheric electricity, as well as of the direction, changes, and force of the wind."

The great Melbourne telescope, which, when the address was delivered, had been fairly at work for ten months, is then considered, and Mr. Ellery observes that while the Society is disappointed in not getting, as it was hoped, the best telescope in the world, the members may feel satisfied that they have obtained an instrument that, "if it does not exceed, quite equals every other of its sort that has been yet made."

The progress of the survey is then noticed at considerable length. "The coast line from the boundary of South Australia to Lake Howe has been carefully measured, and, with the exception of the north-west portion of the colony, nearly every district has been emmeshed by the geodetic surveyors. The most important operation of late has been the determination of the termini of the boundary between New South Wales and Victoria. It is much to be regretted that the late retrenchments in the public expenditure have materially interfered with the progress of the survey.

After a few remarks on the commercial importance of local industries, especially the preservation of meat, the president referred to our vastly increased knowledge of the sun since the date of the eclipse of May 1869, to the nature of the sun's spots, and to the connection of the latter with the occurrence of magnificent auroras and magnetic storms, and to the spectrum of auroral light. "During the most brilliant display in April last, I was able," he observes, "to obtain a very bright spectrum of the light with a micro-spectroscope. Unfortunately the dispersion was small, but the light was so intense as to admit of a very narrow slit. The spectrum obtained from the red streamers consisted of a strong red band or line (which I estimated was rather more refrangible than C line), and bands in the green, which I believe to be the same as described by Angström. The spectrum of the green light which formed the lower arch of the aurora, however, contained no red band, and the appearance of it, as the spectroscope was passed up and down, so as to receive the light from the streamers or green arch, was very marked indeed. I am not aware of this red band or line having been noticed by any previous observers; and had it not been so clear and prominent, far brighter than the green ones—and had I not proved that it belonged to the red streamers, and not to any other, of the auroral light, by the method referred to—I might have been doubtful as to the real existence of a line not hitherto noted in the spectra of aurora." The address concludes with a few observations regarding the possibility of our ever being able to ascertain the laws which govern the weather so as to predict with certainty the atmospheric condition of to-morrow. On this point Mr. Ellery does not express himself very hopefully, but he thinks that the greater climatal events, such as dry or wet, hot or cold, seasons may be traced to varying conditions in the sun itself, and will be found to be extraneous to our globe.

G. E. D.

SCIENTIFIC SERIALS

THE *Journal of the Royal Geological Society of Ireland*, vol. xii. Part 3 (vol. ii. Part 3, new series), containing the Proceedings of the Society for the session 1869-70, has just been published. It contains among other memoirs, Prof. Traquair on *Griffithides mucronatus* (M'Coy) Plate xvi., and on *Calamoichthys calabaricus*. Rev. J. D. La Touche on Spheroidal Structure in Silurian Rocks, Plates xvii.-xx. Rev. M. Close on some Corries and their Rock Basins in Kerry, Plate xxi. Edward Hull on the Geological Age of the Ballycastle Coal-field, and its Relations to the Carboniferous Rocks of the West of Scotland, Plate xxiii.—W. H. Baily on the Fossils of the Ballycastle Coal-field, Co. Antrim.

Zeitschrift für Ethnologie, 1870, Hefts 3, 4.—A paper by Bastian on the legend of the Amazons, is full of valuable information, but is written with less skill than learning. The footnotes make more than three-fourths of the whole, and the parentheses nearly half of the rest.—Hensel contributes a description of two skulls of Coroado Indians (Brazil) with figures. He agrees with many of our best ethnologists that the dimensions of the cranium afford us no safe ground for making racial or specific distinctions. On the other hand, he regards the structure of the facial bones as of great importance from this point of view.—R. Hartmann continues his studies on domestic animals by an account of the reindeer in its present condition, followed by an interesting discussion on the evidence of its domestication in prehistoric times. This number also contains a short archæological account of the Uglei See (one of the numerous lakes in the east of Holstein, situated in an enclave belonging to Oldenburg), by E. Friedel.—The last number of the same journal (1870, 4) is almost entirely devoted to American Ethnology. Prof. Strobel concludes his contributions to comparative ethnology by an account of the weapons and food of the South American Indians; Dr. Fonck has a paper on the Indians of Southern Chili; Ernst of Carácas one on the Natives of the Peninsula of Goajiro, which forms the western boundary of the entrance to the gulf and bay of Maracaybo, in Columbia; and Erman contributes an account (with a map) of the various races inhabiting what was until lately Russian America, the Aleutian Isles, and the opposite coast of N.E. Asia; he divides them into two great groups according to their system of numeration.

IN the *Journal of the Ethnological Society of London* (October 1870) is an interesting paper by Mr. David Forbes "On the Aymara Indians of the Peruvian highlands." Very full information as to their physical structure is given, together with *exact measurements*. Beside their short stature and capacious thorax (which seems to be constantly fixed in the condition of inspiration) Mr. Forbes's statistics show that the thigh is shorter than the leg, and that the heel is as much shorter than a European's as a Negro's is longer. The half-castes between these Indians and the white population are not believed by the author to be prolific, so that, as in the case of mulattos, the intermediate race would soon die out if not continually recruited by new accessions. Among many interesting details on the food of the Aymaras—especially their method of preparing potato so as to keep it from rotting—on their disposition and habits, their implements, and their language, perhaps the most remarkable is an account of a silver statuette (figured in pl. xx.) of a man in a strange headdress, who holds in one hand a mask, which he has apparently taken off in order to look through an instrument like a telescope. This tube he holds to his left eye (without shutting the other) and directs it upwards. Mr. Forbes believes this to be a unique specimen.

THE last part (Band vii. Heft 1) of the *Zeitschrift für Biologie* contains: 1. The results of an elaborate series of experiments by Gustave Meyer of Oldenburg on the effects of feeding dogs and man on bread alone, and bread mingled with meat and other articles of diet. He shows what indeed has long been known, that to feed either animals or man on bread alone is a great waste of material, and that immense quantities must be given in order that the body should lose no flesh, whilst on the other hand the addition of some, even though a small quantity, of meat is economical. He demonstrates that the tissues of the body become more watery with insufficient food, which renders the whole organism less capable of resisting injurious influences. In his experiments on man he endeavoured to ascertain which of the several kinds of bread in ordinary use (white bread, rye bread, black bread) was absorbed in greatest amount during its passage through the alimentary canal, and found that white wheaten bread occupies the first place, then leavened rye bread, then the bread (rye) prepared by the Horsford-Liebig process, and lastly the Pumpernickel (North German black bread). Nevertheless, the first is not so satisfying to the feeling of hunger as the three latter, and is more expensive in every point of view. He denies the great nutritious value often attributed to bran, since the nitrogenous compounds it contains are mingled with much non-assimilable matter, but admits that if these could be extracted and were then returned to the flour, the best results would be obtained, as the meal already contains abundance of salts. 2. A paper by MM. Ernst Schulze and Max Märcker on the determination of Nitrogen in the Urine of the Ruminants. 3. A paper by Dr. J. Bauer on the Metamorphosis of tissue in poisoning with Phosphorus; and lastly a short paper by Max von Pettenkofer on Typhus and Cholera as connected with the basal water line in Zurich.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, April 6.—Prof. Frankland, F.R.S., president, in the chair. The president, occupying the chair the first time since his election, returned his thanks to the Society for the honour conferred upon him, and expressed his readiness to discharge the duties of his office to the best of his abilities. The following gentlemen were elected fellows:—F. Coles, C. E. Groves, E. W. T. Jones, L. T. MacEwan, and J. L. Shuter. The following papers were read: "On Burnt Iron and Burnt Steel," by W. Mattieu Williams. Iron, which has been damaged by reheating, or excessively heated and exposed after balling in the puddling furnace, is designated "burnt iron" by the workmen. It is remarkable that no amount of heat applied to the iron in the blast furnace or in the early stages of the puddling process produces burnt iron. Burnt iron is brittle, its fracture is short and what is called crystalline, it has lost the fibrous character of good iron. If steel is raised to a bright red heat and suddenly cooled, it is rendered hard and brittle, but these conditions may be modified by the process of tempering; if, however, the steel be raised to a yellow or white heat, and then be suddenly cooled, it is no longer capable of being tempered by mere reheating. It is worthless for ordinary uses of steel unless it is again raised to a welding heat and rolled or hammered while hot, and then allowed to cool gradually. The fracture of burnt steel presents a coarse grain and a crystalline appearance. Careful investigation, however, shows something more, viz., that the facets of the aggregated granules have a more or less conchoidal form. The name of "toad's eyes" has been given by practical men to these concavities. Mr. Williams found that a piece of burnt iron contained oxide of iron dispersed through its mass. A sample of burnt steel, however, investigated in the same manner as the iron, showed no indications of the presence of oxide. This, of course, was to be expected, as the carbon of the steel must, more or less completely, protect the metal from oxidation. That iron, when unprotected by combined carbon, should oxidise not merely on its surface, but through its whole substance, when exposed at a sufficiently high temperature and for a sufficient length of time to the action of the atmospheric oxygen, is not difficult to conceive, since the researches of Deville, Troost, and Graham have shown red-hot iron to be permeable by certain gases. In the case of steel, as Mr. Williams states, the burning is limited to the oxidation and consequent removal of the carbon, which takes place even at a low red heat. The permeability of red-hot steel by oxygen and carbonic oxide enables us to understand the process of the internal oxidation of the carbon. The "toad's eyes" or conchoidal facets of the so-called crystals, Mr. Williams explains by supposing a piece of steel at the temperature most favourable to the rapidest endosmosis of oxygen and the exosmosis of carbonic oxide to be suddenly cooled, and the possible occlusion of the carbonic oxide to be arrested. The results would be a certain molecular disintegration and porosity of the steel presenting those conchoidal spots. This view is further supported by the fact that burnt steel may be cured by reheating and hammering, or rolling at a welding heat.—"On the Formation of Sulpho-acids," by Dr. Armstrong. Occupied with an investigation into the constitution of sulphuric acid, the author turned his attention to chlorhydric sulphate, a body discovered some years ago by Prof. Williamson. When that substance, SO_2 HO Cl, is made to react on benzol, the chief product of the reaction is sulphobenzid; sulphobenzolic chloride and sulphobenzolic acid being also formed, but in relatively small quantity. This led Dr. Armstrong to commence a series of experiments to determine, if possible, the conditions under which the one or the other of the above reactions took place, and to arrive at a general expression for the action of chlorhydric sulphate on organic bodies. The bodies he had until now acted upon with SO_2 HO Cl are brombenzol, nitrobenzol, nitrophenol (both modifications, the volatile and the non-volatile), and naphthalin. The results of his experiments lead the author to conclude that the normal action, so to speak, of SO_2 HO Cl is to form a sulpho-acid, the Cl of the chloride removing H from the body acted upon, and replacing it by the group HSO_3; it is only under certain conditions that both Cl and HO are removed from the chloride, and a sulphobenzid-analogous compound formed. What these conditions are Dr. Armstrong hopes to establish by further experiments.—"On a Water from the Coal Measures at Westville, N.S.," by Prof. How. The contents of this paper bear upon the relation of the constitution of a water, and the nature of the geological stratum from which it takes its origin. The water above-mentioned comes from what Dr. Dawson terms the Middle Coal Formation of Nova Scotia, which includes the productive beds of coal, and which, according to the same authority, are destitute of properly marine limestone. The analysis of the water seems to bear out the latter assertion, since the water is very poor in chlorides.

Geologists' Association, April 4.—The Rev. T. Wiltshire, M.A., F.G.S., president, in the chair. A paper by Messrs. Alfred and R. Bell was read, "On the English Crags, considered in reference to the Stratigraphical Divisions indicated by their Invertebrate Fauna." In this paper the authors object to the present division of the crag series, and especially to all the beds which have hitherto been included under the term Red Crag being associated together. From palæontological and other evidence they conclude that the upper portions of the Red Crag ought to be associated with the Mammaliferous Crag, the Chillesford beds, &c., for the whole of which deposits the name Upper Crag is proposed. The Red Crag proper should then be called Middle Crag, and for the term Coralline Crag the name Lower Crag should be substituted. The authors having paid great attention to the organic remains of the crag, were able to give with their paper lists in which were enumerated a larger number of species from the Red Crag than had previously been published. Mr. Henry Woodward, F.G.S., while commending the labours of Messrs. Bell in the Red Crag, urged the systematic exploration of the Norwich Crag, which would, he thought, yield interesting and valuable results. Communications which had been received from Mr. G. Charlsworth and the Rev. Osmond Fisher on the subject of the paper were read, and after some remarks by Mr. Lobley on the nomenclature at present in use, Prof. Morris, in an interesting speech, referred to the labours of Mr. Charlsworth and others, and advised the postponement of any alteration of the crag nomenclature until the recent researches of Mr. Prestwich have been published. Several other members took part in the discussion, after which Mr. A. Bell briefly replied.—A paper on South African Diamonds was then read by Prof. Tennant, who exhibited a fine collection of specimens of these brilliants, as well as models of the largest which have been found. Amongst the interesting facts stated it was pointed out that in four years six diamonds, each weighing upwards of fifty carats, had been found in South Africa, while in the same period only one of a similar weight had been obtained from Brazil. The president expressed his belief that diamonds would ultimately be produced artificially. Prof. Morris inclined to the opinion that diamonds were of vegetable origin, and thought they might have been produced from decomposed resins. The geological formation from which diamonds are derived is very doubtful, as the stones are found in gravels and sands brought down by streams. Mr. Rabone, who has lately returned from the diamond-fields, gave a very interesting account of the discovery of diamonds in the colony, and of the operations now going on there. It appears that no less than 150,000*l.* worth of diamonds have been passed for duty, and this amount, there is reason to believe, is not more than half of the entire value of the stones found during the past four years. The diamond country is, perhaps, 20,000 square miles in extent, and there are now 13,000 persons engaged in searching for the gems. Mr. Rabone expressed his conviction that after two or three months' labour success on the part of a searcher was certain, and corroborated Prof. Tennant's statement as to the large proportion of heavy diamonds found. He considered the discovery of diamonds in South Africa was intended to further the spread of the human race, reminding the meeting that the colony of South Africa is larger than France, while the population is not greater than that of the city of Glasgow.—At the next meeting of the association on the 5th May a paper will be read by Mr. Henry Woodward, F.G.S., F.Z.S., "On the Fauna of the Carboniferous Epoch."

NORWICH

Norfolk and Norwich Naturalists' Society, March 31.—Mr. H. Stevenson was elected the President for the ensuing year, and the Rev. Joseph Crompton, who has filled the chair since the formation of the society, was elected a Vice-President. Mr. Thomas Southwell was elected secretary, and Mr. C. G. Barrett treasurer. The retiring President then read an interesting address, setting forth what had been done by the society during the past year, and what should be its objects in the future, contrasting favourably the present state of scientific inquiry and the spirit in which the search after truth is received, with that which prevailed in years that are passed, concluding with some remarks upon Darwin's last book, the "Descent of Man," which he said should rather have been called the "Ascent of Man."—After the President's address, a letter was read from Mr. Stevenson, who was unable to be present, strongly reprobating the practice of killing woodcocks in spring, when returning northwards to their accustomed breeding haunts. He deprecated the act as inexcusable, inasmuch as a March woodcock is useless for the table, and the shooting season being over, they are mostly killed by gamekeepers, whereas, if the birds were allowed to remain unmolested, many would breed in this country, every season affording fresh instances of their inclination to do so.

KILKENNY

Royal Historical and Archæological Association of Ireland, April 5.—Mr. P. Walters in the chair. The following new Associates were elected:—Rev. F. E. Hamilton, Messrs. R. O'Brien, R. W. Banks, W. F. Skene, J. H. Browne, J.

J. Cramsie, W. E. B. Wyse, T. Atkinson, J. O'Neill, and J. Martin, M.D. The following members were admitted as Fellows: — Hon. B. E. B. Fitzpatrick, Lieut.-Col. E. Cooper, Captain Langton, Messrs. E. Shine, R. R. Brash, J. Watson, N. Emmis, J. Digges, F. Coney, J. Hill, J. E. Mayler, and W. R. Molloy. An application from Mr. Justin McCarthy Brown, Hobart Town, Tasmania, "that the Journal of the Association might be given as a free grant to the Tasmania Library, Hobart Town," was considered and granted. The Secretary, Rev. James Graves, reported on the progress made with the restoration of St. Francis Abbey, Kilkenny, and pointed out the necessity for further subscriptions to preserve the beautiful old tower. A report on the present state of the ruins at Monasterboice, Co. Louth, by J. Bell, C.E., was read, and the following subscriptions, to commence forming a preservation fund, announced:— E. Fort, Bart., D. Dunlop, R. M. Bellew, C. Fortescue, M.P., and M. O'Reilly-Dease, M.P., 10*l.* each; M. Branagan, 5*l*; Revs. Harpur and Campbell offered not only to subscribe but also to collect subscriptions. The Chairman exhibited and described some more of the ancient record of the Corporation of Kilkenny.—Papers read:—"On the exploration of Cranoges," by G. H. Kinahan, M.R.I.A., "On some iron tools and other antiquities found in the Cranoge of Cornagall," by W. F. Wakeman, M.R.I.A.

CALCUTTA

Asiatic Society of Bengal, January 4.—The president, the Hon. T. B. Phear, exhibited some diagrams, showing the diurnal oscillations of the barometer at Dalhousie during part of October 1870. He remarked upon these curves, and called attention to the part which the pressure of vapour in the atmosphere was supposed to have in effecting the barometric oscillations. Colonel Strachy stated that the opinion that the presence of vapour in the atmosphere had any important influence on the oscillations of the barometer was totally unfounded, and indicated the results of his own observations at various stations.—Mr. T. W. H. Tolbort communicated a paper on the history, archæology, and natural productions of the district of Dera Ismail Khan, which will be published in the Journal of the Society.—Bábu Rájendrálala Mitra read a memoir on the antiquity of Indian architecture, in which he maintained the indigenousness of the art.—Mr. Wood Mason exhibited and described a very curious instance of polydactylism in a horse from Bagdad. This horse had on each fore-foot a supernumerary digit, furnished with an asymmetrical hoof, articulated to the rudimentary metacarpals of the fourth toes; these digits consited of the usual number of phalanges. Figures of this curious malformation are given.

PHILADELPHIA

Academy of Natural Sciences, December 6, 1870.—Dr. Ruschenberger, president, in the chair.—Professor Cope made some observations on a number of species of reptiles from the Cretaceous beds of Kansas, which he had recently studied. He tated that the specimens included parts of *Elasmosaurus Latyurus* Cope, *Polycotylus latipinnis* Cope, *Liodon proriger* Cope, and two new Liodons, which he named *L. ictericus* and *pl. mudgei*. A third new Mosasauroid of the size of the *L. Mudgei* was described under the name of *Clidastes cineriorium*. It was stated to be much the largest species of the genus, and to differ from the three now known in having the plane of the articular extremities at right angles to the long axis of the centra, and not oblique to it. He described a third new Liodon, of gigantic size, stating it to exceed by very much the Maestricht reptile, and even the *Mosasaurus brumbzi* Gibbes, which was till now the largest known species. He pointed out the characters of the vertebræ, which were very much depressed as to the centrum, which measured 5¾ inches in diameter. It was allied to the *M. brumbzi*, but differed in having a strong emargination of the articular faces to accommodate the neural canal. He named it *Liodon dyspelor*. Prof. Cope also exhibited the humeri and femora of *Polycotylus*, which were like those of *Plesiosaurus*, and measured eighteen inches in length.—Mr. Thomas Meehan exhibited several specimens of the *Maclura aurantiaca*, the common osage orange, in which the plants were inarched together in pairs in a remarkable way. He said the osage orange was extensively grown as a hedge plant, and in digging up the one-year plants these united twins were usually found in the proportion of about one score in ten thousand. Double kernels were common occurrences in many seeds. There were double peaches, almonds, and double yolks in eggs. But these all had their separate seed coverings or membranes, and the yolks their own albuminous envelopes, consequently the separate embryos produced distinct plants. But these indicated that there had been two separate embryos under one seminal covering, and that the radicular portions of this double embryo, having no membrane to separate them, had inarched themselves together while passing to the ground. If this was the true explanation, he thought there was no such case recorded. That it was true seemed probable, from the fact that all the specimens were united in exactly the same manner, showing that time, place, and the circumstances of the union were uniformly the same. The scars showed that there were four cotyledons and two germs, and that the place of union was midway between the pairs of cotyledons. From the base of the cotyledons extending the whole length of the radicle, the union existed. The length of this united part was from half an inch to one inch, according to the vigour of the plant. Another lesson he thought was afforded by these specimens. Dr. Asa Gray had recently remarked, in *Silliman's Journal*, that European botanists still believed what American botanists had learned to doubt, that the radicle was a true root, rather than a morphologised joint of stem. Here was, he believed, an illustration of the American view. These radicles, which had evidently united together under the seed coat, had elongated after protrusion, just as a young shoot with all its parts formed in the bud elongates after the bursting of the bud scales. They comprised the half inch, or inch united portions before referred to. If these radicular portions of the seed were of the nature of root rather than of stem, we might expect to see lateral fibres push from them, as we do see from the true roots, which start out below the union. But these parts are as free from rootlets as any portion of the true stems above the cotyledon points, indicating, as had been suggested, that their properties were rather of stem than of root.

December 20.—Mr. Vaux, vice-president, in the chair.—Prof. Leidy directed attention to a preparation of the trunk of an adult male subject, from the dissecting room of the University, in which all the viscera were reversed in the order of their usual position. The heart is reversed in position with its apex directed to the right. The aorta descends on the right side; and the cavæ are placed on the left of the vertebral column. The liver is placed in the left, the spleen in the right side. The stomach is reversed, and the large intestine commencing in the left iliac region terminates in the rectum from the right side.

December 27.—Dr. Ruschenberger, president, in the chair.—Prof. Leidy called attention to an interesting geological phenomenon in the vicinity of Wayne station on the Germantown Railroad, about three miles from Philadelphia. At the point where Wayne Street cuts through a fold in the micaceous schists of this district, there occur huge imbedded boulders of very hard compact hornblende rock. The matrix of mica schist has the appearance of an altered argillaceous slate, and rapidly decays on exposure. The hornblende rocks are thus left protruding above the soil, and would be difficult to account for if attention had not previously been called to them in place. As occurring in the schist, they are rounded upon their corners and edges, and smooth upon the sides. It does not appear an improbable conjecture to suppose that they constituted a part of a primitive surface formation—perhaps the original earth crust—which was broken up before the deposition of the metamorphic rocks which make up the azoic rocks of undetermined geological age, overlying the south-eastern angle of Pennsylvania; and that by steam and current actions, perhaps in part by glacial, they were brought into the shape of boulders at a time anterior to the deposition of the sedimentary mica schists. And it is a fact of interest in this connection that the highly garnetiferous mica schists of this district, are charged with dodecahedral garnets, which have probably belonged to preexistent rocks, inasmuch as their angles and edges are rounded off, and the crystals reduced to an almost globular form. This is true of the garnets while still firmly imbedded in the mica schists, and applies to the garnetiferous mica schists extending over a wide area.

American Philosophical Society, February 17.—Dr. Emerson read a paper on the Lunar Influence in its supposed relation to meteorological phenomena, combating views favourable to the existence of such influence.

March 3.—Prof. Cope read a paper "On the occurrence of fossil Cobitidæ in Idaho."—A paper by Thomas Bland was read

"On the geology and physical geography of the West Indies, with reference to the distribution of mollusca." He stated that the land shell faunas of Porto Rico and the Virgin Islands, Sombrero, Anguilla, St. Martin, and St. Bartholomew, are closely allied, and may be called distinct from that of Haiti on the west and the islands to the south. He came to this conclusion from the facts of distribution, and now finds corroborative evidence from the depth of water. An elevation of the bank on which Porto Rico and the Virgin Islands stand (to and including Anegada) of less than 40 fathoms would make one island of the whole. Anguilla, St. Martin and St. Bartholomew stand on one bank, and a similar elevation would unite them; there is deep waters around Sombrero. The fauna of the group is Mexican and Central American, with peculiar genera not represented in the islands south of the Anguilla bank. There are genera represented in Cuba and the Bahamas, Haiti, Porto Rico, and the islands on the Virgin and Anguilla banks, but not in the islands south. One Strophia fossil in Sombrero and in St. Croix, recent in the others. The depth of water between St. Thomas and St. Croix is 2,000 fathoms, telling of long separation. These facts point to a large island or continent, which embraced the Anguilla bank in its southern limit. Barbuda and Antigua stand on one bank, St. Eustatius, St. Kitts, and Nevis on another, with land shell fauna alliance with Guadaloupe, Dominica, Martinique, and Barbados—that group by their fauna connected (not a few species in common) with Guiana—water over 1,000 fathoms deep between Dominica and Martinique, and the latter and St. Lucia and St. Vincent. Now Trinidad and Tobago (both on soundings) Grenada (300 to 400 fathoms between it and Trinidad), the Grenadines (all on one bank with Grenada), and St. Vincent (1,300 fathoms between it and St Lucia) have peculiarly the fauna of Venezuela. Bulimus proper (South American) is only found on those islands and St. Lucia. The greatest depth between St. Vincent and the Barbados is 1,218 fathoms, and between the latter and Tobago 1,060. These facts point to an extension of the South American continent, say from north of the Amazon River to a point west of Trinidad, and northerly to Barbuda, the west side (now Trinidad, Tobago, Grenada, Grenadines, St. Vincent, and St. Lucia), having the Venezuelean fauna, and the east side (now Barbados, Martinique, Dominica, Guadaloupe, Antigua, &c.) having the Guiana fauna.—Prof. Cope read a paper entitled "Supplement to the Synopsis of Extinct Batrachia and Reptilia, &c.," in which several extinct reptiles were described. Portions of the jaws and teeth of one of these from New Jersey were exhibited. It was named *Liodon sectorius*, and was characterised by a greater amount of compression of the teeth than in any certainly known Mosasauron, the crown resembling those of some sharks.—Prof. Cope read a paper "On extinct forms of fishes of the neotropical region." Two new genera, *Prymnates* (Clupeidæ) and *Anædopogon* (? Characinidæ) were determined. He also exhibited some fossil Batrachia from the Carboniferous of Linton, Ohio, obtained by the Geological Survey under Prof. Newberry. One a specimen of *Sauropleura remex* Cope presented a well-developed hind limb. *Oestocephalus amphiuminus* was branchiferous, and probably limbless. Another fossil, representing a new genus, was referred to as *Conchiocephalus pisculus* Cope. It had two operculum-like bones on the sides of the cranium, the teeth obtuse and in brushes; the size of *Protonopsis*.—Mr. Pliny Earle Chase read a paper on American and European rain-falls, showing an opposition at different seasons of the year, analogous to that which he had pointed out at different periods of the lunar month. Comparing the quarterly rains at Lisbon and at Philadelphia for the sixteen years, 1855 to 1870 inclusive, he found that the half years which were the most rainy at one station were the least so at the other. He also found that, in ten years out of the sixteen, an annual rainfall above the average at one station was accompanied by one below the average at the other. Mr. Chase also communicated some of the results which he had obtained by a discussion of the meteorological observations of the Signal Service Bureau, United States War Department. Perhaps the most important of his deductions were the following:—(1) The greater importance of the barometric gradients than of the isobars, in making American forecasts; (2) the great frequency of anti-cyclonic storms in the United States; (3) the probable origin of a large proportion of our storms in the blending of the polar and equatorial currents, near the latitudes at which the general tendency of the winds changes its direction; (4) the greater severity and briefer duration of cyclonic commotions than of those which are primarily anti-cyclonic.

BOOKS RECEIVED

ENGLISH.—Fragments of Science for Unscientific People: J. Tyndall (Longmans).—Classical and Prehistoric Influences upon British History, pt i.: S Bannister (Longmans).—British Rainfall for 1870: G. J. Symons (Stanford).—Symons' Meteorological Magazine for 1870 (Stanford).—The Beginning: its When and its How: M Ponton (Longmans).—The Poor Artist: R. H Horne (Van Voorst).—Half-crown Saturday afternoon Rambles round London: H. Walker (Hodder, Stoughton, and Co).—A Sketch: Romance of Motion: A Lee (Longmans).—What is Industrial and Technical Education? two Orations by Dr John Mill (Simpkin and Co).

FOREIGN.—(Through Williams and Norgate)—Die Elektromagnetische Telegraph: Dr Schellen (2 vols).

DIARY

THURSDAY, APRIL 20.

ROYAL SOCIETY, at 8.30.—Note on the Circumstances of the Transits of Venus over the Sun's Disc in the years 2004 and 2012: J. R Hind, F.R S.—On the Existence and Formation of Salts of Nitrous Oxide: Dr. E. Divers.—Research on a new group of Colloid Bodies containing Mercury, and certain members of the series of Fatty Ketones: Dr J. E. Reynolds

SOCIETY OF ANTIQUARIES, at 8.30.—On the Original Purport and Use of the Galilee of Durham Cathedral W. White, F.S A.

LINNEAN SOCIETY, at 8.—Notes on Mr Murray's paper on the Geographical Relations of the chief Coleopterous Faunæ: Roland Trimen, F.L.S.

CHEMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall.

FRIDAY, APRIL 21.

ROYAL INSTITUTION, at 9.—On the pre-Socratic Philosophy: Prof. Blackie, F.R.S.E.

SATURDAY, APRIL 22.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.

ROYAL INSTITUTION, at 3.—On the Instruments Used in Modern Astronomy: Mr Lockyer.

MONDAY, APRIL 24.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

INSTITUTE OF ACTUARIES, at 7.—On Industrial Assurance: H. Harben.

LONDON INSTITUTION, at 4.—On Astronomy: R. A. Procter, F.R.A.S. (Educational Course)

SOCIETY OF ANTIQUARIES, at 2.—Anniversary Meeting.

TUESDAY, APRIL 25

ROYAL INSTITUTION, at 3.—On the Geology of Devonshire, especially of the New Red Sandstone System: William Pengelly, F.R.S.

WEDNESDAY, APRIL 26.

GEOLOGICAL SOCIETY, at 8.—On a new species of Coral from the Red Crag of Waldringfield: Prof. P. Martin Duncan, F R S., F G S.—Notes on the Minerals of Strontian, Argyllshire: R. H Scott, F.R.S, F.G.S.—On the probable origin of Deposits of "Loess" in North China and Eastern Asia: T W. Kingsmill, of Shanghai

SOCIETY OF ARTS, at 8.—Photography in the Printing Press, being a Description of the Working of the Heliotype Process: Ernest Edwards.

ROYAL SOCIETY OF LITERATURE, at 8.30.—On the Classical Names of Rivers: Hyde Clarke.—On Shakespeare's Birthday: C. M. Ingleby, LL.D.

LONDON INSTITUTION, at 12.—Annual Meeting of Proprietors.

THURSDAY, APRIL 27.

ROYAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 7.30.—On Economic Botany: Prof. Bentley.

ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall.

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THURSDAY, APRIL 27, 1871

THE HOPE OF FRANCE

A PAPER which M. Henri Sainte-Claire Deville has recently laid before the Paris Academy of Sciences is in our opinion of such high importance that we make no apology for placing it in original before our readers. It runs as follows :—

"La science a joué un grand et terrible rôle dans les défaites que nous venons de subir. Les découvertes d'Ampère, les travaux de nos mécaniciens militaires ont été cruellement utilisés contre nous. Enfin, l'organisation libérale des universités allemandes a été mise au service de passions haineuses dirigées contre notre pays. Aussi dit-on de tous côtés et avec raison que c'est par la science que nous avons été vaincus. La cause en est dans la régime qui nous écrase depuis quatre-vingts ans, régime qui subordonne les hommes de la science aux hommes de la politique et de l'administration, régime qui fait traiter les affaires de la science, leur propagation, leur enseignement, et leur application par des corps ou des bureaux où manque la compétence et par suite l'amour du progrès.

"Aujourd'hui, messieurs, il est temps d'agiter publiquement les grandes questions. La réserve modeste pratiquée trop souvent par un trop grand nombre des membres de cette Académie serait une faute grave en ce moment, une faute sans excuse.

"Dans des temps calmes beaucoup d'entre nous avaient pu se ménager dans leurs cabinets ou leurs laboratoires cette vie studieuse rendue si douce et si facile par l'éloignement des hommes et de leurs débats intéressés. Il est de notre devoir aujourd'hui d'intervenir tous activement et directement dans les affaires du pays, et de contribuer de toutes nos forces à une régénération par le savoir dont la France exprime partout la nécessité.

"Dans les temps difficiles, le pays a trouvé chez les membres de cette Académie, et dans l'Académie tout entière, le dévouement absolu sur lequel il avait le droit de compter. Nos séances, si bien remplies pendant la durée du siége, en seront un témoignage mémorable. Ces services mêmes, l'autorité morale que nous devons à notre origine qui est l'élection de chaque membre par ses pairs, tout, messieurs, nous oblige de contribuer à cette régénération du pays par l'initiative de chacun, par l'action de la compagnie tout entière.

"J'ai donc l'honneur de proposer à l'Académie d'admettre à l'ordre du jour de ses séances les grandes questions du développement et de l'enseignement de la science en France et toutes les questions d'intérêt général qui concernent la science et les savants.

"Par exemple, la France possède de grands et glorieux corps scientifiques dont quelques membres ont constamment siégé dans cette Académie. Quel service nous rendrions, si nous pouvions faire dépouiller ces grands corps de l'enveloppe politique, administrative ou fiscale qui les étouffe, qui met en péril le recrutement de la science parmi eux et dans les écoles célèbres qui leur servent de pépinières.

"Je le répète; je demande à mes confrères d'élargir le cercle de ses communications et d'y faire entrer toutes les questions d'intérêt scientifique, de quelque ordre et de quelle que nature qu'elles soient, de quelque part qu'elles viennent.

"Des commissions choisies dans nos sections et quelquefois dans les autres classes de l'Institut, devraient préparer, résumer et rédiger au besoin comme des vœux ou des décisions académiques les délibérations de la compagnie.

"Sous cette forme nouvelle, qui exclut toute intervention dans les affaires du gouvernement (car les affaires d'instruction publique ne sauraient plus être politiques), nous ferons arriver les conseils de l'expérience et du savoir, et, j'espère, toutes les vérités utiles à la connaissance directe du pays tout entier."

It is not our purpose to inquire how true may be M. Deville's idea that the success of the German arms has been due to the more vigorous pursuit of science in Germany; but we may remark that it is certain that it is the distinct and emphatic belief of the most eminent scientific men of France: it is our clear duty, however, to inquire whether we are in a better condition in this matter than France was a year ago.

First. Are our men of science subordinated to place-seekers or politicians who, according to M. Deville, lack competence, and therefore the love of progress? It is a question even whether we have even reached this stage! In England there is absolutely no scientific organisation whatever, no minister whose duty it is to care one jot for science, no one man in office to whom either scientific instruction or the advancement of science is of the least interest—unless, perhaps, we except Mr. Lowe, and we all know in what point of view he from time to time turns from his more important avocations to deal with such questions.

So that on this point there is no comparison between the two countries. The French have an organisation, the working of which, according to their own showing, has been entrusted to incompetent politicians. We have no organisation whatever—which, perhaps, is a blessing, if we should be compelled to imitate the French plan—but we very often have to pay very dear for blessings, and this certainly is one which the sooner we get rid of the better.

Let us now pass from the disease to the remedy. M. Deville proposes that the French Academy should at once take up all large questions connected with the advancement and teaching of science (and we are glad to see that M. Deville does not put the cart before the horse, as is too much the fashion here) in order to liberate it from the political, administrative, and fiscal fetters which now paralyse it and to enable the country to make the most of every scientific idea or effort.

As we understand this proposal, M. Deville wishes that the Academy of Sciences should interest itself not only in abstract Science, and in the teaching of abstract Science, but in all the scientific inquiries or departments of all branches of the administration. To what extent interference is proposed in this latter case we have as yet no means of knowing, but it is easy to see that the further this goes the better it will be for the nation. Two matters which have recently occurred in England, to which we may refer parenthetically, will, we venture to think, strengthen our assertion. We shall not be contradicted when we state that if the Scientific Committee recently appointed by the Admiralty—a Committee which contains among its members Profs. Sir Wm. Thomson and Rankine—had been in existence when the *Captain* was built, instead of being appointed after that costly ship had simply turned bottom upwards because it was top heavy, we should now be the richer of a noble ship and five hundred men. And yet—and yet—we believe this Committee is only a temporary one. Take another case: A Commission was recently appointed to inquire into the Education of the Army. Was there a man of Science upon it? Certainly not. And what was one of the first things that came out of that Commission's report? This, namely, that most of the teachers of Science in the Army Schools received notice to quit. England, on the high authority of Lord Northbrook, did not want a Scientific Army.

All this by the way. We have referred to these instances, in order to show that the various departments of the Administration want scientific control here as is in France—that M. Deville's suggestion is of value here as there.

Now, assuming that the suggestion is a vital one, or even that it is an important one, and that it is good for England as for France, and we shall gladly open our columns to a discussion on these points; the question arises—is it possible to adopt it here?

We are met at once by the different conditions of the French Academy of Sciences, and our own Royal Society. The Academy is a large paid body; our Royal Society is a small unpaid body, and the work, which M. Deville considers so necessary for the regeneration of France, and which many consider necessary for the salvation of this country, is no temporary or small affair. The labour would be great, enormously great at first, and, moreover. would be a never ending one. To impose such a labour as this on a private body, which was constituted for entirely different purposes, would simply be to destroy that private body altogether, and it would be both unwise and unjust for such a body to undertake it, unless each member had ample means and no occupation. so that all his time and energy might be devoted to the task.

We think, then, that for these and for other reasons, not far to seek, it is impossible for our Royal Society to play permanently the *rôle* here which M. Deville has suggested to the Paris Academy.

But here, at length, is a grain of comfort. We have in England, at the present moment, a body at work, which if the general ideas of the power entrusted to it be correct, may perform those very services for England which M. Deville so loudly calls for—a call which all men of science *d'outre manche* re-echo—in the case of France. We refer to the Royal Commission on Scientific Instruction and the Advancement of Science, on which body, we take it, has devolved just such a general overhauling of all matters scientific within these realms as M. Deville has proposed—a herculean task, but a noble one if done well, and a task which will not be well done unless it is indicated how England can be put in a position second to no other nation so far as Science is concerned, a position that she certainly does not occupy at present.

But supposing all this done, we must have something more. We must have some permanent machinery, and having this we must have the scientific men mindful, above all other things, of the interests of science, and then our politicians will hear no uncertain sound as to the merits or demerits of State aid to the higher education. A nation, as a distinguished foreign *savan* has recently said, must endow science until that nation stands first (1) in abstract Science, (2) in the applications of Science generally, and (3) in the amount of knowledge possessed by State servants of all classes. When she has achieved this point the question of continuing State aid may properly be discussed—not till then. To this let us add that apart from the question of State-aided Science that nation will stand highest which, in addition to the above conditions, calls into her councils her men of Science, and becomes a Science-aided State.

EDITOR

PANGENESIS

IN a paper, read March 30, 1871, before the Royal Society, and just published in the Proceedings, Mr. Galton gives the results of his interesting experiments on the inter-transfusion of the blood of distinct varieties of rabbits. These experiments were undertaken to test whether there was any truth in my provisional hypothesis of Pangenesis. Mr. Galton, in recapitulating "the cardinal points," says that the gemmules are supposed "to swarm in the blood." He enlarges on this head, and remarks, "Under Mr. Darwin's theory, the gemmules in each individual must, therefore, be looked upon as entozoa of his blood," &c. Now, in the chapter on Pangenesis in my "Variation of Animals and Plants under Domestication," I have not said one word about the blood, or about any fluid proper to any circulating system. It is, indeed, obvious that the presence of gemmules in the blood can form no necessary part of my hypothesis; for I refer in illustration of it to the lowest animals, such as the Protozoa, which do not possess blood or any vessels; and I refer to plants in which the fluid, when present in the vessels, cannot be considered as true blood. The fundamental laws of growth, reproduction, inheritance, &c., are so closely similar throughout the whole organic kingdom, that the means by which the gemmules (assuming for the moment their existence) are diffused through the body, would probably be the same in all beings; therefore the means can hardly be diffusion through the blood. Nevertheless, when I first heard of Mr. Galton's experiments, I did not sufficiently reflect on the subject, and saw not the difficulty of believing in the presence of gemmules in the blood. I have said (Variation, &c., vol. ii., p. 379) that "the gemmules in each organism must be thoroughly diffused; nor does this seem improbable, considering their minuteness, and the steady circulation of fluids throughout the body." But when I used these latter words and other similar ones, I presume that I was thinking of the diffusion of the gemmules through the tissues, or from cell to cell, independently of the presence of vessels,—as in the remarkable experiments by Dr. Bence Jones, in which chemical elements absorbed by the stomach were detected in the course of some minutes in the crystalline lens of the eye; or again as in the repeated loss of colour and its recovery after a few days by the hair, in the singular case of a neuralgic lady recorded by Mr. Paget. Nor can it be objected that the gemmules could not pass through tissues or cell-walls, for the contents of each pollen-grain have to pass through the coats, both of the pollen-tube and embryonic sack. I may add, with respect to the passage of fluids through membrane, that they pass from cell to cell in the absorbing hairs of the roots of living plants at a rate, as I have myself observed under the microscope, which is truly surprising.

When, therefore, Mr. Galton concludes from the fact that rabbits of one variety, with a large proportion of the blood of another variety in their veins, do not produce mongrelised offspring, that the hypothesis of Pangenesis is false, it seems to me that his conclusion is a little hasty. His words are, "I have now made experiments of transfusion and cross circulation on a large scale in rabbits, and have arrived at definite results, negativing, in my opinion, beyond all doubt the truth of the doctrine of Pangenesis." If Mr. Galton could have proved that the reproductive elements were contained in the blood of the higher animals, and were merely separated or collected by the reproductive glands, he would have made a most important physiological discovery. As it is, I think every one will admit that his experiments are extremely curious, and that he deserves the highest credit for his ingenuity and perseverance. But it does not appear to me that Pangenesis has, as yet, received its death blow; though, from presenting so many vulnerable points, its life is always in jeopardy; and this is my excuse for having said a few words in its defence. CHARLES DARWIN

THE NEW HOSPITAL OF ST. THOMAS

II.

THE large wards of the Hospital contained in the several flats of the Blocks 2, 3, 4, 6, and 7 * are rooms of noble dimensions. In the second, third, and fourth floors, each ward is more than 100ft. long, 38ft. wide, and 15ft. high; and as this space is designed for the accommodation of twenty-eight patients, each patient will have more than 2,000 cubic feet of air to his own share, irrespective of change by ventilation. But the arrangements for warming and ventilation are also very complete and admirable. The entire building is, in the first instance, warmed to a certain extent by pipes which receive supplies of hot water from large boilers fixed in the basements of each block of building. These heating pipes are expanded into broad radiating coils here and there where immediate increase of warmth is desired. There are two of these radiating coils to each ward. But in addition to these, there are also in each three *open fire-places* situated in the central line of the floor, and sending circular iron chimneys or flues up through the ceiling. These columnar iron chimneys are, however, double. Each has an inner central pipe, and an outer investing sheath. The inner pipe carries up the smoke of the burning fuel; the outer case collects all the effete and used-up air of the chamber, and discharges it with the smoke at the outer orifice above the roof, the central heated pipe being an efficient cause of a steady up-cast. The final outflow of both smoke and impure air is by the square turrets, which are seen from the outside as a part of the ornamental finish of the roof. The fresh air is brought from the outer wall beneath the floors, and is discharged into the wards *through the heated casings* of the fire stoves and radiating coils. This double plan of warming, partly by radiating hot pipes, and partly by open fire places, is the very perfection of efficiency and comfort. Private residences in England are almost always uncomfortable in very cold weather, however liberal may be the consumption of fuel, because the larger and brisker the fires, the more intolerable are the indrafts of cold air. The cylindrical smoke pipes run straight up from basement to roof through the entire series of floors, so that when the flues require cleansing, a kind of plug is removed from the bottom of the pipe, and the entire accumulation of soot is brought down at once into one of the cellars of the basement, without causing any interference with the comfort or cleanliness of the several wards above.

There are nurses' chambers on either side of the entrance of each large ward; and at each side of the farther end corresponding turrets, or corner rooms, containing lavatories and baths on one side, and closets on the other with convenient little shoots, which are to convey the dust of sweepings and the soiled linen of the patients down at once to the offices in the basements. Near the nurses' chambers there is also a large square lift, worked by hydraulic power, to be used in conveying patients and supplies of all kinds, up and down between the projecting corners, or turrets (at *b b* on the plan). At the further extremity there is a most delicious open-air balcony looking over the cheerful river, with ready access to it from the windows of the wards.

Block No. 9, being designed for the reception of infectious and contagious diseases, is differently planned. There are smaller wards on each side opening from a central stair-case and landing. Between the Blocks 2, 3, and 4, and between 6, 7, and 8 (at *a*, *a*, on the plan) are low buildings rising in broken and ornamental form from the general line of the connecting corridor, which will be used for the residence of officers of the establishment. Connected with the upper part of these, there is a fine surgeons' operating theatre at each side of the building, one for males and the other for females. These are entered from the light and airy glazed corridor of the second floor, and have retiring-rooms for patient and surgeon, and a direct way to a pleasant open-air flat roof looking out over the river.

In communication with the great connecting corridor there is a perfect maze of offices and conveniences, approached by an accident-receiving porch abutting on the Lambeth Road. There are receiving-rooms for out-patients and for surgical cases and accidents, dispensaries, and a long range of small private rooms for the medical and surgical officers, clerks, and dressers. The Administrative Block, No. 1, is entered from the Westminster Bridge Road by two flights of steps, one leading to the private residence of the Treasurer of the Hospital, and the other to a large Council hall looking out by a balcony upon the river, and to Committee rooms and other offices, which are to be connected with the other departments of the establishment by lines of electric telegraph. The general entrance of the Hospital is from the Lambeth Road, leading to a spacious hall in the central block, No. 5, above which is the Chapel of the Hospital, a vaulted building of fine proportions and very chaste design. This block will be finished towards the river front, where it is set back or recessed from the line of the other blocks, by an ornamented face which looks out on an enclosed space or central court. From this court the prospect to the river is between the pillars of an open colonnade, bearing in the centre a group of sculptured figures, of which the chief will be the statue of Edward the Sixth, the royal founder of the Hospital.

Block No. 9 has much more the appearance of a church, or chapel, than the central building. It is of low elevation, compared with the other blocks, and has ornamental arched windows of large size; and at the corner there is a square tower, half steeple, half pagoda, which is altogether suggestive of a campanile. It is only when the eye is carried along the general range of the buildings, and it is noted that this tower is simply the architect's contrivance for raising the chimney turret in the case of this lower erection to the same height as the chimney turrets of similar aspect on the higher blocks, that the idea dawns upon the observer that this building may have a very different purpose to perform than affording accommodation for religious worship and service. It is, indeed, a temple for the dead rather than for the living. It is properly the Museum and Medical Schools of the Hospital; and the lofty tower will be employed to waft the vapours of effete corporeal mortality well up into the purifying atmosphere. Its interior will be warmed by hot-water warmed air to feed the combustion of the open fires. This evil is entirely obviated by this plan of having half pipes and radiating coils, to cause a strong up-cast; and will gather the air from brick channels of escape which permeate all portions of the building. The most surprising thing about this terminal block of the Hospital is the vast amount of accommodation that has been got out of it by dint of good package and clever arrangement. It looks like a tolerable-sized chapel, and might really be no more, yet it really contains a very capacious Museum, four large Lecture Theatres, a Chemical Laboratory, a Dissecting-room, mortuary chambers, and students' halls. It is, in fact, one of the most commodious and best ordered Medical Schools in the Metropolis.

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* See plan in NATURE No. 63, p. 202.

There is a long tunnelled way under ground, running to the chambers for the dead in this building, from the several Hospital blocks, from one part of which a branch passage leads to the wash-house and laundry. One of the mortuary rooms is to be arranged as a sort of Morgue, or reception room for friends who come to pay a last visit to the dead; and a pair of large gates opening upon the Lambeth Road will admit the hearse to this portion of the Hospital, when it comes to claim those portions of the remnants of vitality that have not found another mode of escape through the campanile tower. This "old mortality" end of the Hospital nestles curiously close under spiritual over-shadowing. It is only separated from the battlements of the Archbishop's palace at Lambeth by the stables and coach-house of the Treasurer of the Hospital.

The new Hospital of St. Thomas promises to be one of the most complete and efficient of the hospitals and medical schools of the metropolis. R. J. M.

ZOOLOGICAL TEXT-BOOKS

General Outline of the Organisation of the Animal Kingdom and Manual of Comparative Anatomy. By Prof. Rymer Jones, F.R.S. (Van Voorst.) Fourth Edition. 1871.

A Manual of Zoology for Students. By Dr. Henry Alleyne Nicholson. (W. Blackwood and Sons) 1870.

THE mass of information which is continually and rapidly accumulating in every department of Natural Science, renders it increasingly desirable that every manual writer should zealously aim at combining terseness with accuracy, and, by a well-chosen selection of the most important facts, exhibit the results of the more recent acquisitions of science. In the ponderous volume of 886 pages, which now replaces the long-known "Comparative Anatomy" of Prof. Rymer Jones, the very opposite characters are painfully conspicuous, and we sincerely pity the student who has recourse to it for his instruction in zootomy. Not but what the book is both highly instructive and interesting, and exhibits conspicuously the learning, patience, and zeal of its accomplished author. For all this, however, the youth who gets up his zoology and comparative anatomy from it, will find himself out of joint with and wanting, as regards the zootomy of the present day, while he will have wasted time over comparatively useless and antiquated details.

The Rotifera are, indeed, strangely located, being described in one chapter with all the Crustacea except the Cirripeds, which latter are placed apart in a separate chapter. A still more important defect, however, and one almost incredible, is the complete omission of all reference to the Rhizocephala. After this it is not surprising that no notice is taken of those recent discoveries as to larval Ascidians, which seem to indicate a genetic affinity between them and the Vertebrata, and which are now made familiar to all by Mr. Darwin's "Descent of Man." This is the more remarkable, as at p. 666 the author speaks of the Amphioxus as in some respects resembling Ascidians, and being thus connected with the Mollusca. Another singular and conspicuous blot is the location of the Brachiopoda between the Lamellibranchiata and the Gasteropoda.

When we come to the great Vertebrate division of the animal kingdom, we are again painfully impressed by defects and shortcomings, which sometimes must lead to downright error on the part of the unlearned reader.

The Batrachians are lumped together with the true Reptiles in one class, and the student could hardly gather from Prof. Rymer Jones's pages that the true affinities of the former are with fishes, while the latter are closely related to birds. In the general index we read "Comparison between Birds and Reptiles, p. 760, sec. 2,032." We turn to the page and section indicated, expecting to find a succinct statement of the results arrived at by Profs. Huxley and Cope. In reality we find but a meagre statement of the obvious physiological contrasts between the two classes.

Owen's hypotheses as to the essential nature of the vertebrate endoskeleton and its several parts, are given without criticism or discrimination, as if they were views universally received and recognised. But there are positive errors which it is difficult to stigmatise too strongly. Thus, in spite of Prof. Huxley's papers, it is gravely asserted of the terminal caudal vertebræ of homocercal fishes, that they "are commonly blended together, and shortened by absorption, whilst both neural and hæmal arches remain with increased vertical extent, and indicate the number of the metamorphosed and obliterated centrums!"

Of Prof. Owen's hæmal cranial arches, it is said that his labours "have satisfactorily revealed their real nature, and established *beyond a doubt* (the italics are ours) the alliances which exist between the elaborate structures in question and the arches which exist under singular conditions appended to the vertebral segments of the trunk."

At the other end of the skeleton all the valuable researches of Mr. Parker, Prof. Huxley, and antecedent Continental authors are ignored, and the essential affinity between the embryonic mammalian skull and its permanent condition in Batrachians and cartilaginous fishes is passed over in silence.

Similar incompleteness is to be found in treating of single organs in single classes. Thus, in speaking of the swimming bladder, its homology with the lung is only faintly alluded to in terms hardly of approval, and nothing is said of its interesting condition in Polypterus.

As to Birds, the inquirer who consults this manual only will fail to acquire any really adequate knowledge of the class, from the absence of all description of its two very distinct existing types—the struthious and the carinate birds. As regards the omission of any notice of the Archæopteryx, it may be pleaded that it is a fossil form; still a manual of zoology must be reckoned as singularly incomplete which fails to call attention to a form so importantly aberrant.

In the highest class of Vertebrates we miss any adequate statement of the very great gap which exists between the Monotremes and the higher Mammals.

Prof. Flower's careful labours regarding the corpus callosum are utterly ignored. Thus we read, "In those Marsupial tribes that form the connecting links between the oviparous and placental Vertebrata, the brain still exhibits a conformation nearly allied to that of the bird, and the great commissures required in the more perfect encephalon are even yet deficient; but in the simplest brain of a Placental Mammifer the characteristic differences are at once apparent." The student is left entirely in the dark as regards the large anterior commissure of the lower forms which compensates, as it were, for the reduction of the corpus callosum, while presenting such a contrast to the brains of birds and reptiles.

The facts here mentioned will enable the zootomically instructed reader to appreciate the defects which unhappily characterise this last production of Prof. Rymer Jones. As we have said, they do not prevent the volume being replete with both interest and instruction, and a large number of readers may study it with advantage, though it is not calculated for young students who wish to be *au courant* with the latest views and those received by the most esteemed biologists. The prevailing character of the book may be summed up by saying that we have in it almost a maximum of physiological anatomy with a minimum of morphology.

Turning to the much smaller work of Dr. Nicholson, we feel refreshed by coming in contact with a body of more modern views and less-known facts put forth concisely and in a form really useful to the student. There is a copious glossary, the utility of which will far more than compensate for some inaccuracies. There is also (what no book of the kind should lack) an alphabetical index, the want of which will be sadly felt by those who use Prof. Rymer Jones's volume. Nevertheless, Dr. Nicholson's manual, though serving as a stop-gap to supply an urgent need, is not, by any means, all that could be desired. Some of the very glaring omissions we have noticed in the large work are also defects in the smaller one. Thus we have again the absence of any reference to the Rhizocephala, but that larval Ascidian structure is noticed "which has been paralleled with the chorda dorsalis of Vertebrates."

The Brachiopoda are relegated to the vicinity of the Polyzoa, and removed from the Mollusca proper. The Batrachia are associated with the fishes, the Reptila with the Birds. Man is put back into the order Bimana, which appears to us a mistake when he is considered from the zoological point of view only. Investigations and discussions of recent date have abundantly demonstrated that in bodily structure he differs far less from the higher "so-called" Quadrumana than do these latter from the lowest members of that order. In connection with recent investigations it must be remarked that Dr. Nicholson does not sufficiently acknowledge how largely his work reposes on the labours and teachings of Prof. Huxley. That Professor's system and arrangements are almost entirely adopted, even to the location of the class Echinodermata in the subkingdom Annuloida.

It is much to be regretted that the last-named eminent naturalist has not ere this given to the world the results of his labours in his own clear and terse language, and published a model handbook for the use of students. In the absence of such a desideratum we feel sure that Dr. Nicholson's work will, for a time, be deservedly popular and widely used. More than this we cannot, however, anticipate for it without careful and copious additions and emendations.

OUR BOOK SHELF

A History of the Birds of Europe, including all the Species inhabiting the Western Palæarctic Region. By R. B. Sharpe, F.L.S., &c., and H. E. Dresser, F.Z.S., &c. Part I. (London: published by the Authors.)

THIS work will supply a great want, since it will give in a convenient form and at a moderate price, a really good coloured figure with a full and accurate description and history of every European bird. The talent of Mr. Sharpe for publications of this nature has been sufficiently shown by his beautiful work on the kingfishers, which we have already noticed, while his colleague, Mr. Dresser, is an enthusiastic student of European birds. In M. Keulemans they have secured an artist who bids fair to rival Wolf in the delineation of bird character; and if the work maintains the standard of its first part (and we have every reason to believe it will do so), its subscribers will have cause to be satisfied. We find in the part now issued eight species of birds beautifully figured, and about forty-six pages of letterpress, crowded with information from every available source on the habits and distribution of the species. The series of charming pictures of the most graceful of all living things which this work will give, must render it a general favourite; and it will assuredly help to extend the growing taste for natural history, by rendering it comparatively easy for the traveller or resident on the Continent to determine the species and refer to an outline of what is known about any bird he may meet with during his rambles in the country or in the markets. To the home naturalist, also, it will prove far more interesting than a work on British birds alone; for he will here find how far over the globe his feathered friends are accustomed to range, and will make the acquaintance of many members of their several families who, although they live permanently abroad, yet retain a strong likeness to their English relations. We heartily wish Messrs. Sharpe and Dresser success in their bold and laborious undertaking.

A. R. W.

A Treatise on the Action of Vis Inertiæ in the Ocean. By W. Leighton Jerdan, F.R.G.S. (London: Longmans.)

THIS book is a lamentable instance of misconception and error. It is founded on a denial of the first law of motion that a body, if moving, will continue to move in a straight line with uniform velocity, provided no forces act on it. There is through the whole book the most hopeless confusion as to what is meant by no forces acting. The author's theory of the cause of the moon's motion will illustrate the character of the book. It is briefly this :—If the moon were pulled only by the earth's gravitation, it must fall to the earth. It must therefore be pulled in the opposite direction with an exactly equal force. This is called by the author the force of astral gravitation. Since these two exactly balance one another, the question arises, What can cause the moon to revolve round the earth? "A cause," says the author, "for the onward motion of the moon according to the action of well-known laws, is, however, indicated by various well-known tidal phenomena. For the moon tends to raise a mass of water or tide on the earth's surface beneath it; and as the earth's surface rotates eastward, it tends to carry that mass of water or tide with it; and therefore as the moon tends to hold the tide beneath it, the rotation of the earth eastwards must just as certainly tend to carry the moon eastwards as to carry the tide eastwards." Really it is quite irritating that such nonsense should be printed in large type, on good paper, and in a well-bound book.

It is one of the great merits of error that it is inconsistent. That the author enjoys the full satisfaction of this merit is evident from the following :—"If the moon and earth were at rest, then astral gravitation would tend to carry the moon directly from the earth, not at a tangent to any part of its orbit; and when in motion, astral gravitation tends to carry the moon off at a tangent from its orbit certainly, but *in the opposite direction to that of its motion* at the moment of its being released from the grasp of the centripetal force." The italics are the author's—not ours. It appears from this that the author denies the second law of motion, as well as the first, which he elsewhere denies. He here assumes that the effect of a force on a body at rest can be at right angles to its effect on that body when in motion. It is much to be desired that those who undertake to write books would first learn the first principles of that which they write about.

Catalogo Poliglotto delle Piante. Compilato dalla Contessa di San Giorgio. 8vo., pp. i., 747. (Firenze, 1870.)

WE wish to call our readers' attention to this interesting little volume. Its authoress will be better known in the country as Lady Harly of Oxford; and she has spent many years in the compilation of this volume, which, we think, may prove useful to travellers on the Continent, and indeed, might even be made the means of instruction in our public schools. As an example, we select the *Bellis perennis*, which, a native of Europe, we find is, in English Daisy; French, Paquerette, *Marguerite vivace*, Fleur de Pâques; Italian, Pratolina, Margheritina de Prati; Spanish, Maya, la Margarita; German, Masslieben, Gänse Blümchen, Angerblume, Osterblumchen. But we find not only the European names of a large number of plants given, but, even in some instances, the Sanskrit, Bengal, Hind and Tamul names are also appended.

W.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Variability and Natural Selection

I ASK your permission to address to your readers some obser- [illegible] pport of the argument which Mr. St. George Mivart [illegible] his work on "The Genesis of Species," that [illegible] is attributed too exclusive an effect in the origination of new species to the influence of his law of Natural Selection.

1. There are two facts on which Mr. Darwin's theory reposes—the one the variability of animal and vegetable forms; the other, the accumulation of a useful variation by force of the struggle for existence, or Natural Selection. Mr. Darwin assumes Variability to be accidental, not because he or any other philosopher supposes that there is such a thing as accident, but because its law is unknown. He then finds the law of Natural Selection, and by this and this alone, explains the Genesis of Species.

Now, inasmuch as Natural Selection operates on Variability as a pre-existing fact, it follows that the genesis of a new species must result from the operation of two laws—namely, the law of Variability and the law of Natural Selection. But Mr. Darwin attributes it exclusively to the latter; so that, according to his view, there is a law (that of variability) which has no part in producing a result which yet depends for its existence on the fact regulated by that law—namely, variation. This is enormously improbable. It is as if the resultant from two factors were attributed exclusively to one of these factors.

The fact that Mr. Darwin assumes variation to be accidental is, of itself, sufficient to make us expect some residual phenomenon not accounted for by the law of selection: but Mr Darwin's argument does not allow the existence of such a residuum.

2. Mr. Mivart, following the *North British Review*, has shown the great advantage in the struggle for existence which the numerical superiority in individuals of the original form over the new variation may give to the original form. I want to call your attention to the fact, that the useful variety has to overcome not only this advantage to its antagonist resulting from numbers, but a quite distinct difficulty, namely, the tendency to reversion; or, that in other words, the force which Mr. Darwin has at his disposal is not the tendency to accumulate a variation, but only the balance of this tendency over the tendency to reversion.

It is no doubt difficult to estimate the extent to which one tendency is controlled by the other; but to gain some notion of it, let me assume the existence of three forms, with a male and female of each, viz.,

male	female
a	*a'* the original form
b	*b'* the first variety
c	*c'* the second variety

Let me further indicate the existence of an accumulated variation by the double letter *bb*, or *cc*. Let me then assume an equal chance of sexual intercourse between each of the six individuals and of progeny from such intercourse, and we shall see what are the chances of the origin of an accumulated form. For the union of

a	and	*a'*	produces	*a*
a	,,	*b'*	,,	*a*
a	,,	*c'*	,,	*a*
b	,,	*a'*	,,	*a*
b	,,	*b'*	,,	*bb*
b	,,	*c'*	,,	*a*
c	,,	*a'*	,,	*a*
c	,,	*b'*	,,	*a*
c	,,	*c'*	,,	*cc*

So that in the first generation the chance against any given accumulated variation being produced are 8 to 1, and the chances against any accumulated variety at all being produced are 7 to 1; and these odds will go on increasing, because the next generation will retain the tendency to revert to the original simple parent form *a*, not to the original forms of the last preceding generation only. If I assume that each marriage in the first generation produced two males and two females, the result will be, that in the second generation the chance against any given accumulated variety being produced will be 320 to 4 or 160 to 2; and the chance against any accumulated variety at all being produced will be 316 to 8 or 79 to 2.

These chances express the force of the tendency against which Natural Selection has to operate, assuming that the numbers of each variation at starting were equal to those of the original form; but this is, on the assumption that variation is accidental, infinitely removed from the truth; and if the difference between the numbers of the original form and the variation be introduced into the case, the odds are indefinitely increased against the accumulation of a casual variety. None of these observations goes to show that Natural Selection does not exist (I have no doubt that it does), but they show that it has a most uphill game to play, and one in which it is improbable for it to win without help from some other principle.

It may be objected to what I have alleged that the result of the marriages between the individuals of the original form *a* and the varieties *b* and *c*, will not be of the pure original form, but forms half way between that original form and the variety. This will often be the case, but then the improper form of *a* will have in future generations an increasing tendency to revert to the pure original.

3. Mr. Mivart has adduced the co-existence of closely similar structures of diverse origin as evidence that there is a law of Variability over and above the principle of Natural Selection.

The argument appears to me worthy of great consideration. If variability be a force operating in every direction equally, it is very improbable that the course of any two variations should run parellel; if on the other hand the tendency to variation operates along particular lines, then it is likely that the lines which represent the course of separate varieties should run parallel. How do the facts stand? The placental and marsupial quadrupeds form two distinct groups which branched off from one another in very remote times, and yet the two groups have developed into classes and sub-groups so correspondent that their courses may best be described as two parallel zigzag lines. Such a fact almost necessarily involves the conception of one and the same force controlling each group, so as to make it pass along its respective course, just as the likeness of two oak trees implies a like inherent force in each acorn.

But this is not the only instance which Mr. Mivart adduces; he produces the fact that there are two parallel bridges which span the space between birds and reptiles; and I desire to add this further fact not noticed by Mr. Mivart, that there are in like manner two parallel bridges between fishes and reptiles. As is well known, there still exists a small class of animals, half fish and half reptile, often known as the Amphibia. They possess lungs like reptiles, but like fish they have gills and the consequent modification of the hyoidal arch, and their fore limbs have what may be called a degraded and fishlike form. The Archegosauri are extinct creatures of the carboniferous strata—like the existing Amphibia, half reptiles, half fish; like them they carried both gills and lungs, and like them they had the fore limbs in a degraded condition. So far there is nothing to the point; but to the foregoing must be added that the existing Amphibians are a bridge between reptiles and the hard-boned fishes, which are the predominant modern class; whereas the Archegosauri were a bridge between reptiles and the ganoid fishes, to which class they were related by the character of their teeth, the imperfect ossification of the internal skeleton, and the excess of ossification in the external skeleton (Owen's Palæontology, p. 193, *et seq.*). This observation will remain true, even if it should be shown, as some naturalists expect, that the Archegosauri were a tadpole form of a more perfect reptile; for it can hardly be doubted that the immature form expresses the history of the perfect one, and shows by its likeness to the ganoid fishes the original relation to that class.

It has often appeared to me, that there are striking parallelisms in the three great groups of the Quadrumana. For instance, the vocal organs of the Hylobates or Gibbons of the Indian Archipelago present a close relationship to those of the Mycetes or Howlers of the South American forests. The nocturnal and insectivorous habits of some of the American genera recall the like habits of most of the Lemurs: and again, the Baboons are related to the Anthropoid Apes in a way which suggests to the mind that the similarity of their forms is greater than the nearness of their kinship.

There is no doubt that similar parallelisms may be observed in very many groups; and I think that many of those phenomena of likeness which Mr. Darwin would attribute to atavism may as well be explained by the retention in each group of the force which was inherent in the original family from which both groups have proceeded.

This sort of parallelism is well illustrated by a very common accident: a tire gets loose from a wheel; but both it and the wheel pursue nearly parallel courses, except so far as they are operated upon by different external forces.

EDWARD FRY

Protective Resemblances

HAVING read the various papers by Messrs. Bennett, Murray, Wallace, &c., in NATURE, and thinking that my observations made on plants and animals in various parts of Southern Africa may be of some interest to your readers, I am rather hastily putting together a few facts, which it had been my intention to have worked up into an illustrated paper, but which may perhaps prove of most interest at present, while the discussion is still warm.

I must own that I prefer the use of the general term "protective resemblance" to the special term "mimicry," as the latter seems to imply a certain amount of *intelligent volition*, which in the instances cited by Messrs. Wallace, Bates, and Trimen, I believe does not apply, whereas there are, I believe, cases where instinct does come into play, not acting physiologically as Mr. Bennett would seem to assert, but in the construction of disguises.

Mr. Bennett's argument appears to me as fully adverse to his own theory as to Mr. Bates's, and if Mr. Murray's theory is correct then it ought equally to apply to animals so widely different as ants and spiders.

Protective resemblances appear to me to be capable of being roughly classed as general and special, though both run into each other. Of general resemblances there are so many that I hardly think it worth while to enumerate instances, I shall therefore confine myself to some examples of special protective resemblances which I have noted.

1. As to plants I believe protective or useful resemblances are far commoner than some writers seem to think.

That excellent observer Dr. Burchell, in his "Travels," vol. i., p. 10, remarks:—"On picking up from the stony ground what was supposed a curiously shaped pebble, it proved to be a plant, and an additional new species to the tribe of Mesembryanthemum, but in colour and appearance bore the closest resemblance to the stones between which it was growing. On the same ground was found a species of the Gryllus tribe amongst the stones, and so exactly like them in colour and *even in shape* that it could never have been discovered had it not been observed just at a moment when in motion, and as if more completely to elude notice it seldom stirred, and even then but slowly.

"The intention of nature in these instances seems to have been the same as when she gave to the chameleon the power of accommodating its colour in a certain degree to that of the object nearest it in order to compensate for the deficiency of its locomotive powers. By their form and colour, this insect may pass unobserved by those birds, which otherwise would soon extirpate a species so little able to elude its pursuers, and this juicy little Mesembryanthenum may generally escape the notice of cattle and wild animals."

I may here remark that a great number of Karoo plants have tuberous roots of similar form and colour, and it is especially curious to notice that, amongst the Asclepiadeæ, many species, such as *Raphionueme*, which are found in the grassy country, have their tubers hidden beneath the soil, whilst others, which occur in the stony Karoo, such as, *Brachystelma filiforme*, have them above the soil, and so perfectly do they resemble the stones amongst which they are found, that, when not in leaf, it is almost impossible to distinguish them.

Of imitating plants I may mention *Ajuga ophrydis*, the only species of the genus in South Africa, which bears a striking resemblance to an orchid, as also does *Impatiens capensis*, another solitary species. I mention these especially because they are very striking, although I am not aware that they are in any way specially useful, noting, however, that the latter plant is much frequented by insects, often by similar species to those which frequent *Angræcum* and *Mystacidium*, plants affecting similar localities.

I know of many similar resemblances between plants belonging to most different orders, but cannot say how far they are merely fortuitous, and I am anxious to avoid Mr. Bennett's reproach of being "ultra-Darwinian."

With respect to the orthopterous insect mentioned by Burchell, that excellent observer, Mrs. Barber of Highlands near Grahamstown, communicated a most interesting paper to the Albany Natural History Society on a "Stone Grasshopper," which varies according to the nature of the soil of the district it inhabits; and I have in my collection numerous species of Locustidæ collected in the Karoo, whose successful imitation to the soil is most remarkable. I may remark here, that many most singular species of insects lose half their interest when penned down in a collector's cabinet.

As mimicry amongst mammals and reptiles is noted as rare, I may mention instances of what appear to me to be cases.

The plains of S. Africa are characterised by numerous animals generally known to the Boers as "Mierkatjes" (not little monkeys as Burchell renders it, but literally "ant cats"). Most of these are Viverrine Herpestes, Suricates, &c., and all have nearly similar habits. Early in the morning these pretty animals may be seen in numerous groups sitting up on their hind legs warming themselves in the sun, and, when startled, scampering away to their holes, with their tails cocked high up in the air. They are likewise more or less coloured like the Karoo soil, although when individually examined their colours, of course, differ.

All these feed on mice, small reptiles, and grasshoppers and locusts. They likewise greedily devour birds' eggs. Equally common with these in some parts, but especially noticed by me near Cradock, is the *Xerus setosus* Gray, or ground squirrel. It is likewise known as a Mierkatje by the Boers. Unlike the other Mierkatjes it feeds on small bulbs and tubers called "uintjes" by the Boers. Its colour, habits, and long tail, cocked up in the air, gives it a striking resemblance to its Viverrine neighbours; while in common with them it forms burrows in the ground. Of reptiles there is a species of *Dasypeltis*, a snake almost devoid of teeth, very common about Algoa Bay, but with whose specific name I am not acquainted. When irritated this species coils itself up and darts angrily at the intruder, puffing and hissing loudly. When I first met with it I was rather cautious in handling it; but discovering its harmlessness I have kept several in confinement.

A few years ago I was examining some Bushman caves in the Nuncazana Valley, Bedford District, when my Hottentot servant told me that there was a "Groste Nacht-adder" in the cave, and I found what I also took to be a large Night adder. It sprang at us, and nearly escaped. On examining it when dead I was much surprised to find it was a very fine specimen of my old friend the *Dasypeltis*, as I had not met with the species since I had left Port Elizabeth. Night adders are common in the Nuncazana, as also here. On comparing the two snakes I was much struck by their general resemblance, although, of course, the *Dasypeltis* has a very dissimilar head; but the curious way it has of blowing itself out, and distending its neck, and darting at intruders, heightened the resemblance. I cannot help thinking that these habits must be serviceable to so harmless a snake.

In general protective resemblance Reptiles and Batrachians offer countless examples, and it is especially noticeable amongst the *Agamidæ*, one species of which varies throughout the eastern part of the colony and Free State.

With respect to butterflies I need not enlarge, Mr. Trimen having done his work most thoroughly. I may mention, generally, that *Philognoma varanes*, when its wings are closed, strikingly resembles a dead leaf, and two of its varieties, ♂ and ♀ *P. Agathina*, are found in forests, and their flight strongly resembles falling leaves, as does also that of *P. poppea*. In his work on the "Rhopalocera" he mentions the resemblance between the larva of *Diadema bolina* and *Danais chrysippus*. Amongst birds which I have noticed capturing Lepidoptera I may mention that I have seen *Tchitrea cristata* darting at *P. Agathina*; *Cypselus caffer* I have seen take small moths from the grass, and dart at *Terias Rahel* on our open flats; *Motacilla capensis* I have seen take moths and *P. hellica*; *Dicrurus musicus* is a voracious bird amongst insects, and takes moths, though I cannot state I have seen it capture Rhopalocera, yet I think it also attacks *Pieridæ*.

Mantidæ and some species of flies are, together with spiders, I believe, the greatest persecutors of Rhopalocera. One large kind of fly pursues *Satyridæ* and *Tulbaghia meneris*. I have found a Phasma in the crop of *Strix affinis*.

The most perfect cases of mimicry I know of are two spiders (specific nature unknown to me) which bear the closest resemblance to ants. They belong to the *Salticidæ*, and are, apparently, related to *Salticus formicarius*. The one is smooth, black, and shining, and runs rapidly on the ground and bark of trees, and resembles the ant which builds its nest in *Acacia horrida*, and is used by the Kafirs for the purposes of torture. The other is larger and has its cephalothorax dull black, and its abdomen covered with short yellowish hairs. It is generally found running on the stems of herbaceous plants and small bushes, and closely resembles an ant found in similar situations. The fore legs in both species are larger than the second pair, and are frequently held up, when they closely resemble the antennæ of ants. So exceedingly close is the resemblance that, at first sight, ve almost always taken them for the imitated ants.

singular cases of protective resemblances in other of *Pephricus paradoxus* Sparr.; particularly remarked on in that traveller's work on South Africa; of another Heteropter in my possession apparently allied to the genus *Phlœa*, and which I discovered by the greatest fluke, so closely did it resemble a knot of the twig on which it was; of two species of Phasma, one believed by Mr. Trimen to be *Palophus Haworthii*, Gray, both of which were found in the Karoo on dead branches. *Acacia horrida*, known as the "Doorn Boom," or Mimosa throughout the Eastern province, is a perfect museum of protective resemblances.

I have the larva of a moth, which forms a case so exactly like the thorns of that tree that no one could detect it when not in motion; Mr. Trimen will, I hope, determine the species. A caterpillar belonging to a geometric moth, which piles the blossoms on its back, and doubles itself up so as exactly to resemble the flower heads; a larva of a species of *Neæra*, which exactly resembles the young thorn leaves on which it feeds, and which forms a pupa in a case resembling an excrescence of the bark; an Epeira which has two excrescences on its abdomen exactly resembling two old broken thorns. I could recount a number of other similar instances, but for the fear of trespassing too much on your space.

Lastly, I would remark how long it often is before frugivorous birds discover fruit not indigenous to the country.

In the drought of 1865 I was much struck by this. We had a solitary damson tree, which had not previously borne much fruit, but which that year had abundance. The mousebirds (*Coleus*) and the Red-wing (*Inida morio*) had devoured the almond and peach blossoms and the figs. With two double-barrelled guns we could not keep them from the latter. Meanwhile the damson tree was never touched, but ripened its fruit beautifully. On the morning of Christmas Day my friend intended taking them to his sister-in-law. Two hours later there was not a damson on the tree; the birds had just found them out, and had strewn the ground with their stones.

Now it has struck me that slight variations in insects may be of much greater value than one might at first sight imagine, and such would Mr. Weir's experiments lead one to expect, the birds selecting certain kinds of dull coloured larvæ from amongst others.

The beautiful instances afforded by the plants and insects I have enumerated can surely be only interpreted by Natural Selection. There can be no hybridism nor instinctive accumulation of resemblances in plants or between plants, grasshoppers and stones, and why should we go out of the way to call in a theory or theories, for which there are no facts as supporters, when we have at hand an explanation so much simpler, and which readily embraces every case?

I have myself no doubt that instinct may come into play, as in the case of the thorn imitator, &c.; and I believe something of the kind is noticeable in savage man, in the disguises he uses. It is a well-known fact that our soldiers often fired at aloes in the Fish River bush, mistaking them for Kafirs, as their red paint is an excellent disguise in the Bush. The North American Indian and the Bushmen adopted numerous disguises in hunting their prey, or in tracking their enemy, and no one who has not seen the latter can appreciate his wonderful power of imitating all kinds of animals.

In conclusion, I may remark that whilst many species of Rhopalocera are most abundant in S. Africa, it is very rarely that one finds their larvæ or pupæ. I have been astonished at the small success I have had in cases where the imagines are most abundant.

J. P. MANSEL WEALE

Brooklyn, near King William's Town, Kaffraria

Sexual Selection

MR. DARWIN in his recent work on the "Descent of Man" has shown that throughout the animal kingdom the male generally displays the stronger passions and is always the most eager. The males, moreover, whenever secondary sexual characters occur, are, as a rule, the possessors of weapons for defence or offence, brilliant colours, or other ornamental appendages, all of which Mr. Darwin supposes to have been acquired through sexual selection, either for the purpose of charming the female or for struggling with other males for the possession of the females. In a few exceptional cases among birds, the females are the wooers, and these are then more brilliantly coloured than the males. In Westwood's work on insects* I met with the following passage:—"M. Donzel has published a curious memoir upon the flight of butterflies whilst coupling (Ann. Soc. Ent. de France, 1837, p. 77.) showing that whilst the males of *Pontia Brassicæ*, &c., *Colias*, and *Polyommatus* support the females, it is the latter which support their partners in the genera *Thais*, *Thecla*, *Argynnis*, *Melitæa*, *Hipparchia*, and *Pieris*." Now this is strictly analogous to these exceptional birds, because, among our British representatives of these genera whenever a considerable sexual difference of colour occurs, *the female is always the more brilliantly coloured.* Thus, the female *Thecla Quercus* has the bright purple patch, and the female *Thecla Betulæ* the brilliant orange blotch on the fore wing, while the females of *Hipparchia* (*Satyrus*), *Janira*, and *H. Semele* are considerably brighter than their partners. The female *H. Megæra* is rather brighter than the male, and the same is true of *Colias Edusa* and *C. Hyale*, since the females of these species have orange or yellow spots in the black marginal border, represented in the males by thin streaks only. The females of the whole genus *Pieris* also are ornamented with black spots on the fore wings, which are only partially present in the males. I must confess that I am not convinced of the action of sexual selection in producing the colours of insects, but it cannot be denied that these facts are strikingly corroborative of Mr. Darwin's views. With few exceptions the rule holds good throughout the exotic species of these genera.

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* "An Introduction to the Modern Classification of Insects" Vol. ii. p. 336.

R. MELDOLA

The Irish Fern in Cornwall

OWING to an accident I did not see NATURE for the 23rd of February till yesterday. In a note which appears in it, on the report of the Cheltenham College Natural History Society, a doubt is expressed as to the accuracy of the statement that the fern, *Trichomanes radicans*, has been found in Cornwall. Knowing that it had not yet been recorded from that county, I have, for some years past, intended to take an early opportunity to make the following facts public; time has, however slipped away, and I have never yet done it.

In August of the year 1867, at St. Knighton's Kieve, a romantic ravine and waterfall on the northern coast of Cornwall, about two miles from Tintagel Castle, I obtained an undoubted specimen of this fern. It grew on a rock overhanging the water, about a quarter of a mile below the fall. It was an exceedingly small patch, and I accordingly contented myself with a small root bearing two fronds. Wishing to grow this specimen instead of drying it, and having unfortunately placed it in a hot-house, the plant died. I have, however, preserved it, withered and dried up as it is, and when I return to London, where my herbarium is, I shall be glad to prodnce it for the satisfaction of any sceptics. In the following year (1868) I paid an exceedingly hurried visit to the same spot, but failed to find the fern; never having been in the neighbourhood since, I have been unable to confirm or to dispel my fear that the plant has been discovered by some ruthless collector. I may add that I have long since mentioned this fact to various friends interested in Botany.

Morebattle, Kelso, April 6 EVERARD F. IM THURN

Fertilisatōn of Hazel

IN a recent number of NATURE Mr. Bennett makes some remarks on the above. What he says leads to the belief that the male flowers of any one plant discharge their pollen just at the very time the stigmas of the female flowers of the same plant are receptive. My observations made this spring, and extending over a number of specimens, quite agree with those of Mr. Marcus Hartog, and therefore break through Mr. Bennett's law, and show that although the hazel is apparently monœcious, yet, practically, it is diœcious. On one plant which I pointed out to several gentlemen, the fertile flowers had their pretty red styles protruded beyond the scales and the receptive stigmas long before a grain of pollen was discharged from the adjoining catkins, whilst on another plant a hundred yards distant from the first all the barren flowers were withered up and ready to fall before the females could be seen.

Lexington, Kentucky JOHN DUNCAN

Thunderstorm at Preston

ON the 25th of last month a violent thunderstorm occurred at Preston, in Lancashire. The spire of St. Walburge's Church, which is, perhaps, the highest point in the town, was struck by lightning, some curious phenomena resulting. The lightning conductor, a rope composed of forty-two copper wires in six strands, was ruptured at about sixty feet from the ground. The wires were untwisted and spread out, the ends fused, and some of them turned up like hooks. The discharge passed obliquely through the tower wall for a distance of about three yards to a gas-pipe inside. In its passage it wrenched a piece of stone weighing 66lb. from an immense block, casting it to a distance of a hundred feet, besides literally tearing off other large masses. Before reaching the gas-pipe it projected some bricks against the opposite interior wall, shattering them to pieces. The gaspipe was severed and the lower portion curled completely round. Thence it passed to the gas-meter and dislocated many pipes beyond. The effect upon one leaden pipe was singular, holes were fused into it, in some cases right through, the molten metal being scattered about. Another discharge passed down a conductor at the west end of the church. The conductor itself was not ruptured, but a gas-pipe close to it was broken and a piece about a yard long was projected to a distance of forty feet, the gas meanwhile having ignited. Within the church, too, where there happened to be a leakage, the gas also ignited. It was further observed next morning that round about the church hundreds upon hundreds of worms were dead or dying.

Stonyhurst College STEPHEN WILLIAMS

Meteorology in Asia

IN NATURE, vol. iii. p. 473, it is said, "from Asia Minor we get no scientific records of weather." Perhaps you will pardon my informing your readers that the Turkish Government has eight telegraphic meteorological stations, besides its Central Observatory at Constantinople, under M. Aristide Coumbary. The stations are Sulina, Varna, Salonika, Fao, Bagdad, Smyrna, and Beyrout.

Two of these are in Asia Minor, and Beyrout is not far off. The observations are published monthly. In addition Mr. E. Purser, C.E., of the Smyrna and Aidin Railway Company, has published his observations for several years.

ROBERT H. SCOTT

A Wind Direction Rain Gauge

REFERRING to the paragraph in your number of March 30, p. 433, and Mr. Lyall's letter, April 6, p. 488, on Mr. Napier's "Wind Direction Rain Gauge," I beg to say that a gauge of a similar construction is in use at the Meteorological Observatory, Army Medical Department, Aldershot Camp. It was devised by the observer, Mr. John Arnold, A.H.C., F.M.S., about three years ago, so it would appear that the credit of the invention, if I may so term it, is due to no less than three parties, to each of whom I believe the matter was entirely original.

JOHN JAMES HALL, F.M.S.

Meteorological Observatory, Fulwell, near Twickenham, April 20

Spectra of Aurora, Corona, and Zodiacal Light

WHILE I am glad on my part to see Mr. Henry R. Procter's letter on the spectra of terrestrial aurora and solar corona in NATURE, p. 468, he may not be displeased on his to receive a confirmatory statement from an independent observer; and to this effect, viz., that whereas, firstly, the spectra of nineteen out of twenty auroras this winter have practically shown me nothing but one bright green line, and secondly, the spectrum of the solar corona, as seen during total eclipses, is said to be mainly characterised also by a similarly single vivid green line, yet it is not the same green line in the two cases, but a something widely, absolutely, physically, different.

I shall be glad, however, to be allowed still further on a branch of this subject, to ask, through the medium of NATURE's useful columns, whether anyone can kindly supply me with recent observations of the spectrum of the zodiacal light?

I have been trying for it in vain all this spring-time, and have now in despair given it up for the season. M. Angström—to whom be all honour for his first observation and correct description of the ordinary auroral spectrum—says that the zodiacal light spectrum shows the same green line as the aurora, viz., 1249 (Kirchhoff); but the eclipse observers, after proving that the corona is cosmical, solar, and the denser part of the zodiacal light,* might expect the latter rather to show the corona's green line, viz., 1474 (Kirchhoff).

This, however, the zodiacal light, according to M. Angström, does not. Is the zodiacal light then telluric and auroral, not solar and coronal? The measurement of the *place* of its green line should settle at once this most important and extensive physical question; and if the line be at any time visible at all, the large spectral distance between 1249 and 1474 (Kirchhoff's scale) would be sensible in the simplest apparatus. Yet, in these high latitudes the zodiacal light is always so extremely faint, so frequently altogether masked by auroral glows, and as yet, for its spectrum, depending, so far as I know, on only one observer, and he residing in quite an aurora-ridden part of the world, that it would seem to be a perfectly fair question to ask, "if any one else, besides the distinguished Natural Philosopher at Upsala, has observed the spectrum of the zodiacal light; and if so, how and where?"

C. Piazzi Smyth

Royal Terrace, Edinburgh, April 14

* [We venture to doubt this.—Ed.]

Aurora by Daylight

I see that more than one of your correspondents in December numbers dispute the possibility of an aurora being seen by daylight. It doubtless is a rare occurrence, but two cases of the kind have come under my notice.

In December last my son saw what he took to be an aurora a little before sunset. He was at the time about three miles from home, and he had it constantly before him during his walk homewards, and saw it more and more developed as the darkness increased, so that there can be no doubt of the appearance before sunset being identical with what proved to be, as the night advanced, one of the most brilliant displays which we have had during a year very prolific in auroras. He states that after the sun had set, but whilst it was still quite light, the bright rose colour which distinguished that aurora was distinctly visible. The streamers that night proceeded from all parts of the heavens, meeting almost in the zenith, but what is very unusual, they were very much brighter in the east and south-east than in any other direction, which is probably the reason why they were so clearly visible whilst the sun was in the opposite quarter.

The other case came under my own observation in September 1849. Immediately after the sun had set, with a perfectly clear sky, I noticed three slightly diverging beams of light on the western horizon. One might almost have taken them for those beams from a setting sun which one sees much more often in pictures than in nature, had it not been that they did not emanate exactly from the spot where the sun had set, that they had an evident motion to the southward, and that two of them extended to the zenith, and finally down to the eastern horizon. It proved to be the most symmetrical auroral arch I ever saw. The perfect horizon I had to the west, and the straightness of its well-defined edges, joined to the facility which the double arch afforded of measuring the distance between the middle of each, immediately struck me as affording an opportunity of calculating its elevation above the earth, upon the supposition that the tapering away towards the horizon was the effect of perspective alone. I took steps therefore to measure the apparent width in the horizon and overhead, and also its rate of motion to the southward in both places, and by both methods the result was about 8 to 1. From these data, upon the assumption that it was a double ring everywhere equidistant from the earth's surface, and moving parallel to itself, I calculated the elevation, which came out 97 miles, and its rate of motion to the southward 656 miles per hour, the auroral meridian being N. 13° 15′ E., and the inclination of the plane of the arch to my horizon 94° 55′. These figures, whatever reliance can be placed on them, have nothing to do with the present question further than to show how bright the object must have been thus to attract my attention, and that, although the sun had set, there was still daylight enough for me to see the second hand of my watch, and to note the objects on an horizon some three or four miles distant, by which I measured the progress of the base of the arch, and the bearings of which I took the next day. Before it became really dark, the arch had become irregular, and detached streamers showed themselves in the usual form.

Ottawa JOHN LANGTON

UNIVERSITY INTELLIGENCE

OXFORD

Queen's College.—Mr. Charles Thomas Blanchard of Clifton College, was elected to a Scholarship in Natural Science in this College on Saturday last. *Proxime accessit*—Mr. William Percy Ashe, of Magdalene College School.

Corpus Christi College.—There will be an Election to a Natural Science Fellowship in this College at the beginning of next Michaelmas Term. The examination will be specially in Chemistry, and will commence on Monday, Oct. 9. Candidates must have passed all the examinations required by the University for the degree of B.A., and must not be in possession of any benefice or property which would disqualify for retaining a Fellowship. Candidates are requested to communicate with the President, either personally or by letter, at their convenience, before the end of Act Term.

Natural Science Lectures

The following Lectures will be given in addition to those noticed in our number for last week:—

Chemistry. Mr. Wyndham on the Elements of the Nitrogen, Boron, and Carbon groups, and their Combinations, on Thursdays and Saturdays at 11 A.M., at the University Museum.

Physiology. On General Physiology, with special reference to the microscopical anatomy and physiological chemistry of the elementary tissues. By Mr. Chapman. Mondays, Wednesdays, and Fridays, at 10 A.M., at the Magdalen College Laboratory.

Physics. On Elementary Mechanics and Hydrostatics. By Mr. Abbay. These lectures are free by mutual arrangement to members of these colleges at which either of these gentlemen is a lecturer, viz., to members of Merton, Magdalen, Jesus, and Wadham Colleges.

CAMBRIDGE

The following lectures in Natural Sciences are to be delivered at Trinity, St. John's, and Sidney Sussex Colleges during Easter term, 1871:—

On Heat. (For the Natural Sciences Tripos.) By Mr. Trotter, Trinity College, Monday, Wednesday, Friday, at 10, commencing Friday, April 28.

On Electricity and Heat. (For the Special Examination of the B.A. Degree.) By Mr. Trotter, Trinity College, Tuesday, Thursday, Saturday, at 10, commencing Tuesday, April 25. Students of Colleges other than Trinity, St. John's and Sidney, can be admitted on payment of a fee.

On Chemistry. By Mr. Main, St. John's College. Tuesday, Thursday, Saturday, at 12, commencing Thursday, April 27. Attendance on these lectures is recognised by the University for the certificate required by medical students previous to admission for the first examination for the Degree of M.B.

Instruction in Practical Chemistry will also be given.

On Geology. By Mr. Bonney, St. John's College.

1. Palæontology. Wednesdays and Fridays, at 9, commencing Friday, April 28.

2. Lyell's Principles of Geology. Tuesdays and Thursdays, at 9, commencing Thursday, April 27.

3. Elementary Lectures. Tuesdays and Thursdays, at 11, commencing Thursday, April 27. Students of other Colleges can be admitted to these lectures on payment of a fee.

On Botany (chiefly systematic and physiological.) By Mr. J. W. Hicks, Sidney College. Tuesday, Friday, Saturday, at 12, beginning Friday, April 28.

On Physiology. The Trinity Prælector in Physiology (Dr. M. Foster) will probably give a short course on Embryology at the New Museums, of which notice will be given.

The Physiological Laboratory will be open for practical instruction in Physiology daily. The lectures will be free to all Members of the University.

An election of a Superintendent of the Museums of Zoology and Comparative Anatomy was held on Tuesday, April 25, at one o'clock in the afternoon. This office was instituted in 1866, and was made tenable for five years; it is in the gift of the members of the electoral roll. The late superintendent, Mr. John Willis Clark, M.A. of Trinity College, was re-elected.

The Syndicate appointed November 17, 1870, "to consider whether any, and if so what, alterations may be made with advantage in the system of University examinations to enable persons who are unacquainted with the Greek language to obtain degrees," have issued an amended report. They recommend the substitution of French and German in place of Greek in the previous examination, at the option of the candidates. Every candidate who selects French and German will have to satisfy the examiners in papers containing passages from French and German authors for translation into English, with plain grammatical questions, and translate passages from English authors into French or German. In addition they will have to answer such questions on the Gospel selected for the Greek Testament subject and on matters collateral thereto as do not require a knowledge of the Greek language. The proposed alteration will come into force at the previous examination in the Lent Term, 1872. It will only affect those persons who proceed to a degree in honours; as a Greek classic the Acts of the Apostles in the original Greek are still retained as subjects in the general examination required of all candidates for the poll degree. A Grace, confirming the report of the Syndicate, will be offered to the Senate to-day.

The Board of Natural Science Studies have issued an amended report, recommending certain alterations in the examination for the Natural Sciences Tripos. They propose to separate the examination into two parts, and to continue it during eight days instead of six as heretofore. They also introduce a *viva voce* examination in addition to that by printed papers. The subjects of examination will be:—1, Chemistry and certain other branches of Physics; 2, Botany, including Vegetable Anatomy and Physiology; 3, Geology and Palæontology; 4, Mineralogy; 5, Comparative Anatomy, Physiology, and Zoology. The questions, exclusive of those which relate to practical work, will be comprised in twelve papers, and be so distributed that each of the papers will contain one or more questions in each of the following branches of science:—1, Chemistry and certain other branches of Physics; 2, Botany, including Vegetable Anatomy and Physiology; 3, Geology and Palæontology; 4, Mineralogy; 5, Comparative Anatomy, Physiology, and Zoology. Some of the questions will refer to objects exhibited at the examination. In the first six papers the questions will be of a more elementary character, and it will be open to the Board of Natural Sciences in any schedules which they may issue to indicate the subjects that shall be suitable for this part of the examination. In the last six papers the questions will take a wider range, yet still, so far as regards those branches for which schedules are issued by the Board of Natural Science Studies, will be confined to subjects indicated in the schedule; each of the last six papers will include a larger number of questions on the several subjects than the first six papers severally contain, and some of the questions will have special reference to the philosophy and history of those subjects. The Board further recommend that the foregoing alterations come into operation at the examination to be held in December, 1872.

EDINBURGH

The half-yearly meeting of the General Council of the University of Edinburgh was held on Tuesday, the Chancellor presiding. A report was given in with reference to the graduation in Arts, and, after some discussion, it was agreed by a majority to represent to the University Court that the degree of M.A. should be remodelled. The report of the committee recommending the establishment of a Chair of Celtic languages and literature was approved. A report was given in proposing that two additional members should be appointed to the Curatorial Court by the University Council. The report was adopted. It was resolved by a majority that the Council should represent to the University Court that it was advisable that the regulations with reference to degrees in law at present in force should be amended to the effect (1) of having two degrees in law attainable by examination, and (2) of not requiring for the lower degree a degree in Arts as a necessary preliminary. Some other business was transacted, after which the meeting separated.

NOTES

A PARAGRAPH has appeared in several papers stating that the venerable Professor of Geology in the University of Cambridge Prof. Sedgwick, purposed shortly vacating the chair. This announcement, we are credibly informed, is, to say the least, premature; and appears to have originated in a paragraph in the Professor's report to the University on the Museum under his charge. The words of this, however, do not necessarily imply more than that he begins to feel the burden of years heavy upon him; and in the University it is not believed that his resignation is imminent. Whether that come soon or late, all who have known how much he has done for the cause of natural science in the University will deeply regret it; and the fine collections in the Woodwardian Museum, amassed in great part during his tenure of the chair, and to no inconsiderable extent at his own expense, will long be a monument not unworthy of a life spent in the pursuit of science and in the service of his University.

THE subject for the Sedgwick prize, to be awarded in 1874, is "The Potton and Wicken phosphatic deposits and their general relation to the strata of Western Europe lying between the Portland beds and the Gault." The prize is open to all graduates who have resided sixty days during the twelve months preceding October 1, 1873. The essays are to be sent to the Registrary on or before October 1, 1873, privately with some motto prefixed, and to be accompanied by a sealed envelope with the same motto on the outside, enclosing a paper containing the name and college of the candidate. The prize was founded in 1865, by some friends of Dr. Adam Sedgwick, to encourage the study of geology. It consists of three years' interest on 500*l.* Scinde Railway Stock, but although subjects were proposed in 1867 and 1870, no essays were sent in.

THE Science and Art Department has issued a circular on the subject of the examinations in Ireland, which are henceforth to be placed on the same footing as those in England.

IN answer to several inquiries from America, we have great pleasure in stating that the report that the eminent astronomer, Leverrier, is reduced to great poverty, is entirely without foundation.

WE regret to record the death of William Wilson, the eminent bryologist, which took place at Warrington on the 3rd of April, in the 71st year of his age. Mr. Wilson is chiefly known as the author of "Bryologia Britannica," the standard work upon British mosses, which was published in 1855, and of which a second edition was in contemplation at the time of his decease. He took a high place as an authority upon mosses among continental botanists, and was in intimate communication with them. In the earlier portion of his life he devoted much attention to

British plants generally, and scattered papers in his name, often containing valuable observations in structure, may be found in the "Phytologist," and other botanical journals. He is frequently quoted by Sir W. J. Hooker, in the "British Flora," and contributed many specimens, accompanied by careful MS. notes, to the Hookerian Herbarium.

WE have received "Zur Erinnerung an Wilhelm Haidinger, von Franz Ritter v. Hauer," an eloquent tribute to the memory of the great mineralogist.

THE photographic papers record the death, at the early age of forty-seven, of Mr. T. R. Williams, one of the earliest photographic portraitists, and occupying the undisputed position of the most successful artist in his particular line. He was a pupil of Claudet's.

IN the first number of the "Zeitschrift für Ethnologie" of this year, is a very valuable list of works bearing on Mr. Darwin's theories, compiled by Spengel, which occupies twelve closely-printed octavo pages. First comes a list of translations into German of "The Origin of Species," of "Fertilisation of Orchids," and of "Variation of Animals and Plants;" next, forty-three original German works, criticising and carrying out the Darwinian theory. These are followed by about an equal number of books which refer to the same subject incidentally, though sometimes at considerable length. Among these we find Kupffer's "Essay on the Relation of Vertebrata to Ascidians," Von Baer's lectures, Carus's "Natur und Idee," and Rütimeyer's "Herkunft unserer Thierwelt." The fourth list is a most valuable one of reviews, magazine articles, and other scattered papers, published in Germany on Natural Selection and the Descent of Man. Books devoted to the latter subject are next enumerated separately; and then translations into German of the kindred writings of Huxley, Lyell, Wallace, Agassiz, and Bates. Last comes a classified catalogue of all the works on Darwinism which have been published outside Germany, in England, France, Holland, and Italy. This list may be advantageously compared with that given by Mr. Darwin himself, in the 5th edition of the "Origin of Species." It will be invaluable to every student of the theory of evolution, and is a remarkable proof of the amount of scientific thought and work (as well as of some that is not scientific) which our great naturalist's writings have called forth.

A COMMUNICATION to the German Society of Anthropology during the past winter invokes the attention of all persons interested in science to the importance of making use of the opportunities for ethnological research furnished by the war between France and Germany; and the author, while acknowledging the difficulty of attending to such matters during the military operations, expresses his earnest hope that every possible effort may be made to secure a good series of the skulls and brains of the African tribes brought by France into the conflict, and especially those of the Turcos. We have not yet heard to what extent this suggestion was heeded by those who had the opportunity.

AT the last meeting of the Scientific Committee of the Horticultural Society, Mr. Alfred Smee introduced to the notice of the committee a new principle of boiler construction for heating hot-houses, &c. The peculiarity consisted in the use of only one pipe for the flow and return of the water, instead of two, as is usually the case. Mr. Smee finds that the difference in specific gravity between the hot and the cold water is quite sufficient to keep the two currents perfectly distinct, and to maintain for any length of time a free circulation at a uniform temperature. The principle is, of course, precisely the same as that of the polar and equatorial ocean currents, with respect to which so much has been said in our columns.

UNDER the title of the South London Microscopical and Natural History Club, a society has been formed to enable microscopists and lovers of natural history residing in the district to meet and interchange communications and specimens; by lectures and papers to afford instruction to the younger members in the use of the microscope, and preparation of objects, and to develop a taste for the study of zoology and botany; and by occasional excursions into the country around to investigate the natural productions of the district and procure fresh materials for observation, which eventually may lead to the formation of a cabinet and herbarium, illustrative of the indigenous fauna and flora of East Surrey. The first meeting was held on April 1, at Gloucester Hall, Brixton, and ordinary meetings are held on the first Tuesday in each month. We understand that the club already numbers more than a hundred members. The President is Mr. Henry Dean, F.L.S., Dr. Braithwaite is one of the Vice-Presidents, and Mr. Frederick Hovenden the Hon. Secretary. The annual subscription has been fixed at ten shillings.

SCIENCE is certainly popular in America. We have received the first number of the *American Journal of Microscopy*, devoted to the elucidation of Scientific and Popular Microscopy, edited by Dr. E. M. Hale, and published at Chicago. A trial number having been issued some months ago met with such a warm reception that the proprietors have determined on a regular monthly issue. The present number contains practical articles on the use of the microscope, and others descriptive of microscopic objects, some of them well illustrated. It promises to be a useful and interesting magazine.

ON February 4, an extraordinary meteor was seen at Pichicani in Peru. It was balloon-shaped, with the pointed end towards the earth, and red-coloured. It descended rapidly to the earth, and its descent was attended by an explosion, leaving a dense cloud over the place. It injured the roofs of several huts, and knocked down a fence for about 500 yards. On the spot, it is reported several dead fish were found of different species, which are supposed to have been lifted out of the river, and dashed against the stones. Similar events happened near Huanochullo and Atucachi.

ON February 22, several shocks of earthquake were felt at Puno in Peru, and on March 6, a slight earthquake of thirty seconds after rain.

IN March a most remarkable electric storm was witnessed at Tacua in Peru. For a few hours the snowy peak of Tacora seemed the centre of conflagration of lightning, nor was the thunder less terrific. The population were the more alarmed as the night before there had been some slight shocks of earthquake.

THE horned toad of Oregon (*Tapaya Douglassii*) is a remarkable creature, plentiful on the open plains of the district among rocks and sand. They are usually grey, but Dr. Cooper states that he met with one example which was of a brick red on the back, but beneath white like the rest. These colours resemble those of the stones among which they live, and it is supposed that they have the chameleon-like property of changing their hue. Though ferocious in appearance, they are perfectly harmless; yet the Indians believe them to have the power of producing a poisonous wound with their blunt spines, just as, among ourselves, the newts are credited with many objectionable qualities. These toads are very slow in motion, and do not attempt to bite. One was kept in captivity for five months without food in a small box, remaining quite lively for the greater part of the time. When irritated, it would spring in a most threatening manner at anything pointed at it, at the same time opening its mouth wide and hissing audibly, after which it would inflate its body, and show other marks of anger.

THE Western gull (*Larus occidentalis*) is very abundant on the whole coast of California, especially on the Farrallone

Islands, where it is a serious hindrance to the men employed in collecting the eggs of the Murre (*Uria Brunnichii* which breeds there in countless numbers. The traffic in their eggs between these islands and San Francisco alone reaches annually the sum of between one and two thousand dollars. The egg-hunters meet at one o'clock every day during the season (from May to July) with the exception of Sundays and Thursdays, and at a given signal, so that each may have an equal chance in gathering the spoil, start off for the most productive egging grounds. The gulls understanding, it would seem, what is to occur, hover overhead, awaiting the advance of the men, who rush eagerly into the rookeries. The affrighted Murres have scarcely risen from their nests, before the gull, with remarkable instinct, flying but a few paces ahead of the hunter, alights on the ground, tapping such eggs as the short time will allow before the egger comes up with him. The broken eggs are passed by the men, who remove only those which are sound. The gull, then returning to the field of its exploits, procures a plentiful supply of its favourite food. Dr. Heermann says that he once saw three gulls scientifically approach a single Murre sitting on her egg. Two of them feigning an attack in front, the Murre raised herself to repel them; instantly the third advancing from the rear seized her solitary egg from beneath her, and flew off with the booty, the two first immediately following to claim their share. The egg was dropped and broke on the rocks, when a general scramble ensued between the three robbers for the valued prize.

AMONG the plants which have received honour and religious veneration among the Hindoos, the Doob-grass (*Cynodon dactylon*) holds a prominent place. Its usefulness, added to its beauty, induced them in their earliest ages to consider it the abode of a benevolent nymph, and it is employed in many of their religious ceremonies. It owes much of its honour to its great tenacity of life, being regarded as an emblem of immortality; and the Veda celebrates it, under one of its names, in words which indicate its supposed mystic origin:—"May *Durvà*, which rose from the water of life, which has a hundred roots and a hundred stems, efface a hundred of my sins, and prolong my existence for a hundred years." The extreme rapidity of growth in this grass is here referred to, and it is stated that, by merely chopping it in pieces and sprinkling these on prepared ground, a verdant sward may be obtained in a few weeks. The Doob-grass is frequently introduced by name in the popular stories of the country, and the roots are esteemed medicinal.

IN the northern district of Alaska the various grasses, which form an important portion of the vegetation, are woven into mats, dishes, articles of summer clothing, such as socks, mittens, and hats, by the Indians and Esquimaux. Mr. W. H. Dall states in his report upon the resources of that region, that in winter the grasses are neatly tied in bunches, and shaped to correspond with the foot; they are then placed between the foot and the sealskin sole of the winter boots worn in that country. "There they serve as a non-conductor, keeping the foot dry and warm, and protecting it from contusion to an extent which the much-lauded moccasins of the Hudson Bay men never do. In fact, I believe the latter to be, without exception, the worst, most uncomfortable, and least durable covering for the foot worn by mortal man."

AMONG other South American districts reported on the point of being reworked are the Cinnabar mines of Santa Barbara in Huancavolico, in Peru, gold and silver mines in Cauca, and copper mines in Doepar, in Colombia or New Granada.

THE Chilian Government has employed the *Cavadonga* war vessel, Captain Gormaz, on surveying duties. Her work begins at Ancud.

THE TRANSITS OF VENUS IN 2004 *AND* 2012 *

WHILE preparations are being made by astronomers of various nations for the observation of the approaching Transit of Venus over the sun's disc in December 1874, it may be of interest to know under what conditions the pair of transits in the twenty-first century will take place. This consideration has induced me to make a careful calculation of the circumstances of the transits in 2004 and 2012, from M. Leverrier's Tables of the Sun and Planet, which at present are extremely accurate, and which there can be little doubt will closely represent the phenomena to be witnessed in those years. The calculations have been made entirely by myself, but with every precaution to avoid error, and I have confidence in the results.

The following are the resulting elements of the transit in 2004:—

Greenwich mean time of conjunction in right ascension 2004, June 7d 20h 51m 28s·8.

	° ′ ″
Right Ascension of Sun and Venus . .	76 50 28·96
Declination of Sun . .	+22 53 20·4
,, Venus .	+22 42 52·3
Horary motion in R.A. . . Sun . .	2 35·07
,, . . Venus .	−1 47·40
Horary motion in declination Sun . .	+0 13·00
,, ,, Venus .	−0 43 83
Semi-diameter . Sun . .	15 45 74
,, . Venus .	28·75
Horizontal parallax . . . Sun . .	8·78
,, . . . Venus .	30 85
Log. distance of Venus from the Earth .	9·46069

Equation of time . 1m 15s·6 (additive to mean time).

Hence, for the centre of the earth,

		d	h	m	s		°	
First external contact =	June	7	17	3	43	at	115·0	For the *direct* image.
from N. towards E.								
First internal contact =	June	7	17	22	35	at	118·0	
from N. towards E.								
Second internal contact =	June	7	23	5	40	at	214·6	
from N. towards E.								
Second external contact =	June	7	23	24	32	at	218·5	
from N. towards E.								

And l being the geocentric latitude, ρ the radius of the earth at any place, and λ the longitude from Greenwich + E, − W., the reductions for parallax will be obtained from

1st ext. cont. = June 7d 17h 3m 43s + [2 2198]ρ . sin l − [2·5932] . ρ . cos l . cos(λ + 176° 32′).

1st int. cont. = June 7d 17h 22m 35s + [2·2571]ρ . sin l − [2 5765] . ρ . cos l . cos(λ + 182° 38′).

2nd int. cont. = June 7d 23h 5m 40s − [2·5090]ρ . sin l + [2·4353] . ρ . cos l . cos(λ + 47° 17′).

2nd ext. cont. = June 7d 23h 24m 32s − [2·4928]ρ . sin l + [2·4631] . ρ . cos l . cos(λ + 54° 35′).

For the Royal Observatory, Greenwich, I find:—

		d	h	m	s	
First external contact,	June	7	17	9	56	Mean times at Greenwich.
,, internal ,,	,,		17	28	51	
Second internal ,,	,,		23	3	24	
,, external ,,	,,		23	22	15	

Therefore the entire transit will be visible at Greenwich.

Similarly, the elements of the transit of 2012 are found to be: Greenwich mean time of conjunction in right ascension 2012, June 5d 13h 4m 44s·3.

	° ′ ″
Right ascension of Sun and Venus . .	74 31 11·9
Declination of Sun . .	+22 40 24·1
,, Venus . .	+22 50 3·0
Horary motion in R.A....... Sun . .	2 34·67
,, ,, Venus . .	−1 37·70
Horary motion in declination. Sun . .	+0 15·23
,, ,, Venus . .	−0 45 37
Semi-diameter...Sun . .	15 46 01
,, . Venus . .	28·77
Horizontal parallax...... ......Sun . .	8·76
,, ,, Venus . .	30·86
Log. distance of Venus from the Earth .	9·46042

Equation of time . 1m 19s·8 (additive to mean time).

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* "Note on the Circumstances of the Transits of Venus over the Sun's Disc in the years 2004 and 2012." From the Proceedings of the Royal Society.

Hence, for the centre of the earth,

	d	h	m	s		
First external contact ... June	5	10	22	11	at 40°3 from N. towards E.	For the *direct* image
First internal contact ... June	5	10	39	56	at 37·8 from N. towards E.	
Second internal contact ... June	5	16	42	6	at 293·1 from N. towards E.	
Second external contact ... June	5	17	0	0	at 290·5 from N. towards E.	

And, with the same notation as before, I find for the reduction for parallax,

1st ext. cont. $=$ June 5d 10h 22m 11s $+ [2{\cdot}4536]\,\rho\,.\sin l - [2{\cdot}4582]\,.\,\rho\,.\cos l.\ \cos(\lambda + 41^\circ\ 28')$.

1st int. cont. $=$ June 5d 10h 39m 56s $+ [2{\cdot}4838]\,\rho\,.\sin l - [2{\cdot}4558[\,.\,\rho\,.\cos l.\ \cos(\lambda + 43^\circ\ 52')$.

2nd int. cont. $=$ June 5d 16h 42m 6s $- [2{\cdot}1301]\,\rho\,.\sin l + [2{\cdot}5968]\,.\,\rho\,.\cos l.\ \cos(\lambda - 10^\circ\ 57')$.

2nd ext. cont. $=$ June 5d 17h 0m 0s $- [2{\cdot}1158]\,\rho\,.\sin l + [2{\cdot}5825]\,.\,\rho\,.\cos l.\ \cos(\lambda - 6^\circ\ 28')$.

At Greenwich the egress only will be visible.

		d	h	m	s	
Last internal contact	June	5 at	16	44	23	Mean times at Greenwich.
,, external	,,	,, at	17	2	15	

The sun will rise at 15h 46m.

J. R. HIND

AMERICAN NOTES

THE year 1871 bids fair to be marked in the history of American science for the great number of exploring expeditions under the auspices of the United States Government. First, that of Captain C. F. Hall, the well-known Arctic traveller, for whose proposed Polar Exploration the United States steamer *Periwinkle* is now being prepared at the Washington Navy-yard. This vessel, of nearly four hundred tons burden, is said to be very staunch and reliable, and her equipment will be of the best order. It is understood that the expedition will start about the end of May, and that Captain Hall's scientific assistants will be Dr. David Walker, formerly known as the surgeon and physicist of Sir Leopold M'Clintock's expedition in the *Fox*, and Dr. Emil Bessels, who has seen Arctic service in a Spitzbergen expedition. Dr. F. V. Hayden, long known to the public as a geologist and explorer, continues his labours of the past season, with the aid of an appropriation by Congress of 40,000 dols. His party is now being fitted out, and will be provided with the necessary assistants in all branches of research. His work will be to the northward of the scene of his last year's explorations. A third expedition is that of Lieutenant G. M. Wheeler, which, under the direction of the War Department, proceeds to explore certain little-known regions of Arizona and Southern Nevada, including the country about the Lower Colorado and Bill Williams Fork. This work will require several years for its completion. Lastly, Major Powell continues his labours during the present season, and expects to make a careful examination of the Canons of Green River as well as of the Colorado.—Attention was called some time ago to the high scientific value of the collection of objects made by the late Dr. Klemm, of Dresden, for use in his "History of the Progress of Human Civilisation;" and it was suggested that in its great extent, and in the harmonious exhibition of illustrations of human art and handicraft in every department, it would constitute an important addition to the means of instruction in the city of New York. An association of parties in Leipsic has accomplished its purchase, at a cost of over ten thousand dollars; and has determined to make it the basis of an international anthropological museum, which, it is expected, will be one of the most complete in the world. Contributions from all parts of the globe, and especially from America, are invited by the committee having the matter in charge, and we trust that the appeal will not be in vain.—Stimulated by the success of the experiment made by the Philadelphians in stocking the Delaware River with black bass, some public-spirited gentlemen of Reading, Pennsylvania, have undertaken to try the same experiment in the Schuylkill, and 350 dollars have already been subscribed for that purpose.—The town of Amherst, in Massachusetts, has followed the example of New York, Philadelphia, Boston, and other places, in intr ducing English sparrows, in the hope of establishing a colony these birds.—The San Francisco papers are calling attention specimens of fossil ivory brought from Alaska; and parties a said to be about entering upon the business of collecting it on large scale. This ivory consists of the tusks of the mammot or fossil elephant (*Elephas primigenius*), the remains of whic are extremely abundant in Alaska, but much more so in Siberi from which latter country, as is well known, an appreciable pe centage of all the ivory now used in the arts is obtained.—Th College of the City of New York in Twenty-third Street, sho a commendable desire to increase its means of instruction natural history, and particularly in the department of osteolog the president having succeeded by unremitting effort in obtaini means to secure quite a large number of specimens, among whic may be especially mentioned a large slab of stone containing well-preserved skeleton of the *Ichthyosaurus*, or fish-like foss lizard from the Lias of Germany. The specimen is about feet long, and, from its perfection and excellence of preservatio is justly entitled to consideration.

EXPERIMENTS ON THE SUCCESSIVE POLA RISATION OF LIGHT, WITH DESCRIPTIO OF A NEW POLARISING APPARATUS*

THE term successive polarisation was applied by Biot denote the effects produced when a ray of polarised light transmitted through a plate of rock-crystal cut perpendicular to the axis, or through limited depths of certain liquids. these cases the plane of polarisation is found to be changed emergence, and differently for each homogeneous ray, so tha when white light is employed, on turning the analyser rou continuously in one direction different colours successive appear, rising or falling in the scale according to the nature the substance.

If, while the analyser is turned from left to right, the tin ascend (*i.e.* follow the order R, O, Y, G, B, P, V), the su stance is said to exhibit right-handed successive polarisation, b if the tints descend, the successive polarisation is said to be le handed.

These phenomena were satisfactorily explained by Fresnel the following way. The incident polarised ray, instead of r solving itself into two plane polarised rays at right angles to ea other, as in the ordinary cases of dipolarisation, resolves itse in these instances into two circularly polarised rays, one righ handed the other left-handed, which are transmitted with differe velocities; each homogeneous ray, thus resolved into two opposi circularly polarised pencils, on emergence composes a ray pola ised in a single plane, the deviation of which from the primiti plane of polarisation depends on the difference of phase of t two circularly polarised rays on emergence.

The rotation of the planes of polarisation is from left to rig or from right to left, according to whether the right-handed left-handed circular rays are transmitted with the greater velocit

The term dipolarisation, proposed by Dr. Whewell to expre the bifurcation which a ray of polarised light suffers when it transmitted through a crystallised plate, is a very appropria one; but as there are different kinds of such separation, we ma designate plane dipolarisation the resolution into two plan polarised rays at right angles to each other, and circular dipola sation the resolution into two circularly polarised rays, one righ handed the other left-handed. In like manner the term ellipt dipolarisation may be employed to represent the phenome shown by transmitting a polarised ray through a plate of roc crystal obliquely to the axis.

The object of the present communication is to make know another means of producing successive polarisation, both righ handed and left-handed, which, equally with the well-know modes, may be proved to arise from the interference of tw opposite systems of circularly polarised rays.

The polarising apparatus which I have employed for the e periments I am about to detail is represented by Fig. 1.

A plate of black glass, G, is fixed at an angle of 3° to t horizon. The film to be examined is to be placed on a diaphrag D, so that the light reflected at the polarising angle from t glass plate shall pass through it at right angles, and after refle tion at an angle of 18° from the surface of a polished silv plate S, shall proceed vertically upwards. N is a Nicol's prism, or any other analyser, placed in the path of the second reflection. The diaphragm is furnished with a ring, moveable in its own plane, by which the crystallised plate to be examined may be placed in any azimuth. C is a small moveable stand, by means of which the film to be examined may be placed in any azimuth and at any inclination; for the usual experiments this is removed.

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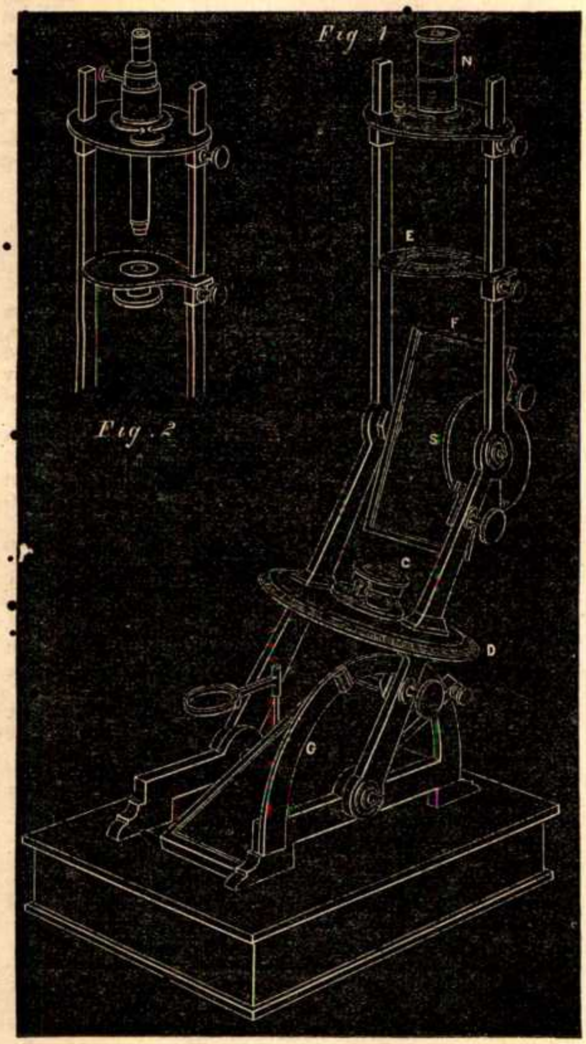
* From the Proceedings of the Royal Society.

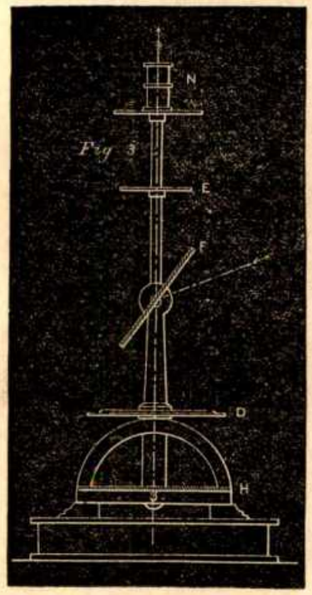
If a lamina of quartz cut parallel to the axis, and sufficiently thin to show the colours of polarised light, be placed upon the diaphragm so that its principal section (*i.e.* the section containing the axis) shall be 45° to the *left* of the plane of reflection, on turning the analyser from left to right, instead of the alternation of two complementary colours at each quadrant, which appear in the ordinary polarising-apparatus, the phenomena of successive polarisation, exactly similar to those exhibited in the ordinary apparatus by a plate of quartz cut perpendicular to the axis will be exhibited: the colours follow in the order R, O, Y, G, B, P, V, or, in other words, ascend as in the case of a right-handed plate of quartz cut perpendicularly to the axis. If the lamina be now either inverted, or turned in its own plane 90°, so that the principal section shall be 45° to the right of the plane of reflection, the succession of the colours will be reversed, while the analyser moves in the same direction as before, presenting the same phenomena as a left-handed plate of quartz cut perpendicularly to the axis. Quartz is a positive doubly refracting crystal, and in it consequently the ordinary index of refraction is smaller than the extraordinary index. But if we take a lamina of a negative crystal, in which the extraordinary index is the least, as a film of Iceland spar split parallel to one of its natural cleavages, the phenomena are the reverse of those exhibited by quartz; when the principal section is on the *left* of the plane of reflection the colours descend, and when it is on the *right* of the same plane the colours ascend, the analyser being turned from left to right.



It has been determined that the ordinary ray, both in positive and negative crystals, is polarised in the principal section while the extraordinary ray is polarised in the section perpendicular thereto. It is also established that the index of refraction is inversely as the velocity of transmission. It follows from the above experimental results, therefore, that when the resolved ray whose plane of polarisation is to the left of the plane of reflection is the quickest the successive polarisation is right-handed, and when it is the slowest the successive polarisation is left-handed; in the order R, O, Y, G, B, P, V, and in the second case in the reversed order.

The rule thus determined is equally applicable to laminæ of biaxal crystals.

As selenite (sulphate of lime) is an easily procurable crystal, and readily cleavable into thin laminæ capable of showing the colours of polarised light, it is most frequently employed in experiments on chromatic polarisation. The laminæ into which this substance most readily splits, contain in their planes the two optic axes; polarised light transmitted through such laminæ is resolved in two rectangular directions, which respectively bisect the angles formed by the two optic axes; the line which bisects the smallest angle is called the intermediate section, and the line perpendicular thereto which bisects the supplementary angle is called the supplementary section. These definitions being premised, if a film of selenite is placed on the diaphragm, with its intermediate section to the left of the plane of reflection, the successive polarisation is direct or right-handed; if, on the contrary, it is placed to the right of that plane, the successive polarisation is left-handed. The ray polarised in the intermediate section is therefore the most retarded; and as that section is considered to be equivalent to a single optic axis, the crystal is positive.



In one kind of mica the optic axes are in a plane perpendicular to the laminæ. They are inclined $22\frac{1}{2}°$ on each side the perpendicular within the crystal, but owing to the refraction, are seen respectively at an angle of 35°·3 therefrom.

The principal section is that which contains the two optic axes. If the film is placed on the diaphragm with its principal section inclined 45° to the left of the plane of reflection, the successive polarisation is right-handed. The ray therefore polarised in the section which contains the optic axes is the one transmitted with the greatest velocity.

Films of uniaxal crystals, whether positive or negative, and of biaxal crystals, all agree therefore in this respect; that if the plane of polarisation of the quickest ray is to the left of the plane of reflection, the successive polarisation is right-handed when the analyser moves from left to right, and if it is to the right of the plane of refraction, other circumstances remaining the same, the successive polarisation is left-handed.

It must be taken into consideration that the principal section of the film is inverted in the reflected image, so that if the plane of polarisation of the quickest ray in the film is to the left of the plane of reflection, it is to the right of that plane in the reflected image.

It may not be uninteresting to state a few obvious consequences of this successive polarisation in doubly refracting laminæ, right-handed and left-handed according to the position of the plane of polarisation of the quickest ray. They are very striking as experimental results, and will serve to impress the facts more vividly on the memory.

1. A film of uniform thickness being placed on the diaphragm with its principal section 45° on either side the plane of reflection, when the analyser is at 0° or 90°, the colour of the film remains unchanged, whether the film be turned in its own plane 90°, or be turned over so that the back shall become the front surface; but if the analyser be fixed at 45°, 135°, 225°, or 315°, complementary colours will appear when the film is inverted from back to front, or rotated in its own plane either way 90°.

2. If a uniform film be cut across and the divided portions be again placed together, after inverting one of them, a compound film (fig 4) will be formed, which, when placed on the diaphragm, will exhibit simultaneously both right-handed and left-handed successive polarisation. When the analyser is at 0° or 90° the colour of the entire film uniform; as it is turned round the tints of one portion ascend, while those of the other descend; and when the analyser is at 45° or *n*90° + 45°, they exhibit complementary colours.

3. A film increasing in thickness from one edge to the other is well suited to exhibit at one glance the phenomena due to films of various thicknesses. It is well known that such a film placed between a polariser and an analyser will show, when the two planes are parallel or perpendicular to each other, and the principal section of the film is intermediate to these two planes, a series of parallel coloured bands, the order of the colours in each band from the thick towards the thin edge being that of their refrangibilities, or R, O, Y, G, B, P, V. The bands seen when the planes are perpendicular, are intermediate in position to those seen when the planes are parallel; on turning round the analyser these two systems of bands alternately appear at each quadrant, while in the intermediate positions they entirely disappear.

Now let us attend to the appearances of these bands when the wedge-form film is placed on the diaphragm of the instrument, Fig. 1. As the analyser is moved round the bands advance towards or recede from the thin edge of the wedge without any changes occurring in the colours or intensity of the light, the same tint occupying the same place at every half revolution of the analyser. If the bands advance towards the thin edge of the wedge, the successive polarisation of each point is left-handed; and if they recede from it the succession of colours is right-handed; every circumstance, therefore, that with respect to a uniform film changes right-handed into left-handed successive polarisation, in a wedge of the same substance transforms receding into advancing bands, and *vice versâ*. These phenomena are also beautifully shown by concave or convex films of selenite or rock-crystal, which exhibit concentric rings contracting or expanding in accordance with the conditions previously explained.

4. Few experiments in physical optics are so beautiful and striking as the elegant pictures formed by cementing laminæ of selenite of different thicknesses (varying from $\frac{1}{2000}$ to $\frac{1}{50}$ of an inch) between two plates of glass. Invisible under ordinary circumstances, they exhibit, when examined in the usual polarising apparatus, the most brilliant colours, which are complementary to each other in the two rectangular positions of the analyser. Regarded in the instrument, Fig. 1, the appearances are still more beautiful; for, instead of a single transition, each colour in the picture is successively replaced by every other colour. In preparing such pictures it is necessary to pay attention to the direction of the principal section of each laminæ, when different pieces of the same thickness are to be combined together to form a surface having the same uniform tint; otherwise in the intermediate transitions the colours will be irregularly disposed.

5. A plate of rock-crystal cut perpendicular to the axis loses its successive polarisation, and behaves exactly as an ordinary crystallised film through which rectilinear polarised light is transmitted.

6. A thick plate of unannealed glass undergoes a series of regular transformation.

The phenomena of successive or rotatory polarisation I have experimentally demonstrated admit of a very simple explanation.

The polarised light incident on the crystallised plate is resolved into two portions of equal intensity polarised at right angles to each other, one in the principal section, the other perpendicular thereto. These resolved portions, when they fall on the silver plate, have their planes of polarisation each at an azimuth of 45°, one to the right, the other to the left of the plane of reflection. These are again resolved in the plane of reflection and the plane perpendicular thereto, and are in consequence of the unequal retardation, which in silver at an angle of 72° amounts to a quarter of an undulation, converted into circularly polarised beams, one right-handed, the other left-handed.

The various homogeneous rays being accelerated differently in their transmission through the two sections of the crystallised plate this difference is preserved after reflection from the silver plate, and the oppositely circularly polarised beams are reflected with the same difference of phase as the two plane-polarised rays are when emerging from the crystallised lamina. The composition of two circular waves, one right-handed the other left-handed, gives for resultant a plane wave, the azimuth of which varies with the difference of phase of the two components.

When the plane of polarisation does not lie equally between the two rectangular sections of the laminæ, these still remaining 45° from the plane of reflection of the silver plate, the beam is resolved into two unequal portions, the amplitudes of which are as sin *a*, to cos *a*.

Each therefore gives rise to a circular undulation of different amplitude. The resultant of two opposite circular undulations of different amplitudes is an ellipse of constant form, the axes of which vary in position according to the difference of phase. The same phenomena of successive polarisation are therefore exhibited in whatever azimuth the laminæ is turned in its own plane, but the tints become fainter and fainter until ultimately, when the principal or perpendicular section is parallel to the plane of reflection of the polarising plate, all colour disappears.

By means of the phenomena of successive polarisation it is easy to determine which is the thicker of two films of the same crystalline substance. Place one of the films on the diaphragm (*a*) of the instrument (Fig. 1 *a*) in the position to show, say, right-handed polarisation, then cross it with the other film; if the former be the thicker, the successive polarisation will be still right-handed; if both be equal there will be no polarisation; and if the crossed film be the thicker, the successive polarisation will be left-handed. In this manner a series of films may be readily arranged in their proper order in the scale of tints.

In the experiments I have previously described the planes of reflection of the polarising-mirror and of the silver plate were coincident; some of the results obtained when the azimuth of the plane of reflection of the silver plate is changed are interesting.

I will confine my attention here to what takes place when the plane of reflection of the silver plate is 45° from that of the polarising-reflector.

When the principal sections of the film are parallel and perpendicular to the plane of reflection of the polarising mirror, as the whole of the polarised light passes through one of the sections no interference can take place, and no colour will be seen, whatever be the position of the analyser.

When the principal sections of the film are parallel and perpendicular to the plane of reflection of the silver plate, they are 45° from the plane of reflection of the polarising mirror.

The polarised ray is then resolved into two components polarised at right angles to each other, one component is polarised in the plane of reflection of the silver plate, the other perpendicular thereto; and one is retarded upon the other by a quarter of an undulation.

When the analyser is 0° or 90° no colours are seen, because there is no interference; but when it is placed at 45° or 135°, interference takes place, and the same colour is seen as if light circularly polarised had been passed through the film. The bisected and inverted film shows simultaneously the two complementary colours.

But when the film is placed with one of its principal sections 22½° from the plane of reflection of the polarising-mirror, on turning round the analyser the appearances of successive polarisation are reproduced exactly as when the planes of reflection of the silver plate and of the polarising-mirror coincide. In this case the components of the light oppositely polarised in the two sections are unequal, being as cos 22½° to sin 22½°; these components respectively fall 22½° from the plane of reflection of the silver plate and from the perpendicular plane, and are each resolved in the same proportion in these two planes The weak component of the first, and the strong component of the second, are resolved into the normal plane, while the strong component of the first and the weak component of the second are resolved into the perpendicular plane.

As bearing intimately on the subject of this paper, I will here quote a passage from a memoir presented by Fresnel to the French Academy of Sciences in 1817, and published, in abstract, in the "Annales de Chimie," t xxviii., 1825:--

"If a thin crystallised plate be placed between two parallelopipeds of glass crossed at right angles, in each of which the light previously polarised undergoes two total reflections at the incidence of 54½°, first before its entrance into the plate (which we suppose perpendicular to the rays), and subsequently after its emergence; and if, besides, the plate be turned so that its axis makes an angle of 45° with the two planes of double reflection, this system will present the optical properties of plates of rock crystal perpendicular to the axis, and of liquids which colour polarised light. When the principal section of the rhomboid with which the emergent light is analysed is turned round, the two images will gradually change colour, instead of experiencing only simple variations in the vividness of their tints, as occurs in the ordinary case of thin crystallised plates; besides, the nature of these colours depends only on the respective inclination of the primitive plane of polarisation and the principal section of the rhomboid, that is to say, of the two extreme planes of polarisation; thus, when this angle remains constant, the system of the crystallised plate and the two parallelopipeds may be turned round the transmitted pencil without changing the colour of the images. It is this analogy between the optical properties of this little apparatus and those of plates of rock-crystal perpendicular to the axis which enabled M. Fresnel to foresee the peculiar characters of double refraction that rock-crystal exerts on rays parallel to the axis."

It does not appear that Fresnel, in any of his published memoirs, has given any further modifications of this experiment, the importance of which has been almost entirely overlooked in elementary treatises on light. He does not seem to have remarked that similar phenomena of successive polarisation are exhibited when the light incident on the crystallised plate is plane polarised, nor that the order of the succession of the colours depends on the position of the principal section with respect to the plane of polarisation. These circumstances are indeed necessarily included in the beautiful theory established by this eminent philosopher; but I am not aware that they have hitherto been specifically deduced or experimentally shown.

The apparatus (Fig. 1) affords also the means of obtaining large surfaces of uncoloured or coloured light in every state of polarisation, rectilinear, elliptical, or circular.

It is for this purpose much more convenient than a Fresnel's rhomb, with which but a very small field of view can be obtained. It must, however, be borne in mind that the circular and elliptical undulations are inverted in the two methods; in the former case they undergo only a single, in the latter case a double reflection.

For the experiments which follow, the crystallised plate must be placed on the diaphragm E between the silver plate and the analyser, instead of as in the preceding experiments, between the polariser and the silver plate.

By means of a moving ring within the graduated circle D the silver plate is caused to turn round the reflected ray, so that while the plane of polarisation of the ray remains always in the plane of reflection of the glass plate, it may assume every azimuthal position with respect to the plane of reflection of the silver plate. The film to be examined and the analyser move consentaneously with the silver plate, while the polarising mirror remains fixed.

In the normal position of the instrument the ray polarised by the mirror is reflected unaltered by the silver plate; but when the ring is turned to 45°, 135°, 225°, or 315°, the plane of polarisation of the ray falls 45° on one side of the plane of reflection of the silver plate, and the ray is resolved into two others polarised respectively in the plane of reflection and the perpendicular plane, one of which is retarded on the other by a quarter of an undulation, and consequently gives rise to a circular ray, which is right-handed or left-handed according to whether the ring is turned 45° and 225°, or 135° and 315°. When the ring is turned so as to place the plane of polarisation in any intermediate position be-tween those producing rectilinear and circular light, elliptical light is obtained on account of the unequal resolution of the ray into its two rectangular components.

Turning the ring of the graduated diaphragm from left to right when the crystallised film is between the silver plate and the analyser, occasions the same succession of colours for the same angular rotation as rotating the analyser from right to left when the instrument is in its normal position, and the film is between the polariser and the silver plate.

To arrange the apparatus for the ordinary experiments of plane-polarised light without the intervention of the silver plate, all that is necessary is to remove the silver plate from the frame F, and to substitute for it a plate of black glass, which must be fixed at the proper polarising-angle.

To convert it into a Norrenberg's polariser, a silver mirror must be laid horizontal at H, and the instrument straightened, as shown at Fig. 3, so that a line perpendicular to the mirror shall correspond with the line of sight. The silver plate must be removed from the frame F, and a plate of transparent glass substituted for it, which must be so inclined that the light falling upon it shall be reflected at the polarising-angle perpendicularly towards the horizontal mirror. The eye will receive the polarised ray reflected from the mirror, and the polarised ray will have passed, before it reaches the eye, twice through a crystallised plate placed between the mirror and the polariser. The result is the same as if, in the ordinary apparatus, the polarised plate had passed through a plate of double the thickness.

Fig. 2 shows the addition to the apparatus when the coloured rings of crystals are to be examined by light circularly or elliptically polarised; *a* is the optical tube containing the lenses, which require no particular explanation, and *b* the condenser over which the plate is to be placed.

C. WHEATSTONE

SCIENTIFIC SERIALS

IN the *Journal of Botany* for April, Mr. Hiern concludes his exhaustive paper On the Forms and Distribution over the World of the Batrachian Section of *Ranunculus*; Mr. J. G. Baker his Monograph of the Liliaceous genus *Xiphion*; and we have also the conclusion of the valuable List of New Species of Phanerogamous plants published in Great Britain during the year 1870. Mr. Hiern's paper concludes with a mathematical statement of the form of the leaf of water-plants dependent on the strength of the current. Mr. Carruthers reviews the Contributions to Fossil Botany published in Britain in 1870, which are very few in number.

IN the *Scottish Naturalist* for April, Dr. Lauder Lindsay commences his second paper on Natural Science Chairs in our Universities, and the editor, Dr. Buchanan White, concludes his details of "Sugaring, how, when, and where to do it." Under the head of zoology, Dr. D. Sharp gives an interesting account of the Coleóptera of the Scotch Fir, and Mr. Robert Gray a history of the Capercaillie. The contributions to phytology are a short paper on Scottish plant-names; and a List of Mosses found in the vicinity of Forres, by the Rev. James Keith. Some of the shorter paragraphs contain also interesting information. We would suggest to the conductors of the *Naturalist* whether it is not possible to avoid the very objectionable practice of dividing their papers in the very middle of a sentence. The present number commences ". . . . waist." in the midst of the Editor's article on Sugaring, and concludes "Knowledge is likely to . . . ." in Dr. Lauder Lindsay's on the University Chairs. It is too great a stretch on the memory to expect an incomplete sentence to be kept in the head for three months; and the previous number is not always at hand to remind one of the connection.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 20.—"Research on a New Group of Colloid Bodies, containing Mercury, and certain members of the series of Fatty Ketones." By J. Emerson Reynolds, M.R.C.P. Edin., &c.

"On the Existence and Formation of Salts of Nitrous Oxide." By Edward Divers, M.D.

Geologists' Association, April 10.*—A numerous party of the members of the Geologists' Association visited Cambridge for the purpose of inspecting the Woodwardian Museum and the exposures of Cretaceous strata in the neighbourhood. On arriving at Cambridge the party proceeded at once to the Woodwardian Museum, where they were met by the Rev. Dr. Cookson, Master of St. Peter's College, the Rev. J. Wiltshire, M.A., President of the Association, Prof. Morris, the Rev. T. G. Bonney, Fellow and Tutor of St. John's College, the Rev. Osmond Fisher, M.A., and Rev. Harry Seeley, F.G.S., as the representative of the venerable Prof. Sedgwick, who, much to his regret, was prevented being present. The fine collection of fossil mollusca was ably described by the Rev. T. G. Bonney, and the reptilian remains formed the subject of an interesting discourse by Rev. H. Seeley; after which Prof Morris, in the Geological Lecture Theatre, delivered an address on the geology of the country around Cambridge, which was listened to with great interest by a large audience, who passed a very cordial vote of thanks to the Professor for his lecture. The afternoon was devoted to a visit to the "coprolite workings" and other excavations at Barnwell. Proceeding along the banks of the Cam a fine section of Pleistocene deposits yielding mammaliferous remains and the usual species of mollusca was reached. This exposure exhibited some very fine examples of false bedding, and many granite and other boulders from the drift were here seen. A very extensive excavation in the Gault here capped by a thin deposit of Upper Greensand lower chalk was next visited. The Gault, excavated for brick making purposes, is exposed to a depth of seventy or eighty feet, and from the evidence of well-sinkers it is probably from 150 to 200 feet thick. Fossils are rarely met with in the Gault clay at this place, though in other localities this formation is very fossiliferous. Lying on the Gault at its junction with the Upper Greensand beds before mentioned occurs the stratum containing the phosphatic nodules or "Coprolites" for which this locality is famous, and which, from another locality, were first noticed as being valuable for agricultural purposes by Prof. Henslow. At a short distance from this excavation the coprolite workings are found on all sides. Indeed, the whole of the land in the neighbourhood is being systematically explored for these valuable nodules which lie at an average of six or seven feet from the surface. One field after another is taken in hand, the coprolite bed of about twelve inches in thickness is taken out, the soil is carefully replaced on the surface, and the field is once again ready for tillage. The "coprolites" are washed by horse power to remove the sand and loam in which they are imbedded, and they are then ready for conversion into manure. A considerable number of fossils, chiefly Brechiopods, were obtained. The party returned to Cambridge, and in the evening were most hospitably entertained at St. John's College by the Rev. T. G. Bonney. On the following day Upware, between Cambridge and Ely, was visited. At this place, situated in the fens and near to the river Cam, very interesting sections have been exposed in consequence of the search for "coprolites." The Gault, which has become very thin, has been cut through and Lower Greensand strata reached. In the Lower Greensand, as at the top of the Gault, a bed abounding in the so-called coprolites is found, and this bed contains characteristic Lower Greensand fossils, together with several new species of Brachiopods described by Rev. J. F. Walker, B.A., F.G.S. Cropping out within a very short distance of this exposure of Gault and Lower Greensand is a remarkable calcareous rock full of corals, which has hitherto been called Coral Rag, but which Rev. Harry Seeley, who has paid great attention to the strata of this district considers to be of Kimmeridgian age, and to which he has applied the name Upware Limestone. This rock is underlain by "Ampthill clay," which would appear to be in this district the equivalent of the Coral Rag of Oxfordshire, Gloucestershire, Dorsetshire, &c. The Brachiopods in the Lower Greensand coprolitic bed are abundant, especially *Terebratula sella*, *Terebratula prælonga*, and *Waldheimia* (*Terebratula*) *Davidsoni*. The next excursion of the Association will be to Belvedere and Erith on Saturday next, and during May visits will be paid to Oxford, Grays in Essex, and Yeovil in Somersetshire.

Anthropological Institute, April 17.—Sir John Lubbock, Bart., president, in the chair. Messrs. Marsden Gibson, Adam Murray, Charles Rooke, and Thomas Davey, were elected members. Mr. F. G. H. Price exhibited a pick made from the antler of the red deer. Mr. Charlesworth exhibited an ancient Mexican flake of obsidian, an obsidian core, and a Mexican mirror of iron pyrites.—A paper by Dr. W. H. J. Bleek was read on the position of the Australian Languages. After pointing out the discovery, made by Sir George Grey, that the languages spoken throughout the southern portion of the Australian Continent were derived from one common stock, the author proceeded to inquire what relationship they bore to other languages. It was shown that in structure they were diametrically opposed to Polynesian languages; that they had remarkable resemblances to the Drâvidian or South-Indian languages, and should be placed with the latter in the same class. The author gave a comprehensive sketch of the characteristics of the Turanean languages, their modifications and varieties. From the linguistic point of view Dr. Bleek had arrived at the conclusion that the Australian native was, probably, mainly a degenerate offspring of the South-Indian race, and it was possible that the latter might have some admixture of negro blood, although their physical features would not altogether bear out that conclusion. It was not improbable that some portion of the negro race occupied the tropical districts of India. It would appear from a comparison of the religious customs and observances of the past with those of the present time among the Australian aborigines that those people had fallen from a higher civilisation; and that conclusion would seem to be borne out by a consideration of the artificial nature of their weapons, their knowledge of the art of spinning, the peculiar system of castes existing among them, and from other circumstances tending to confirm that view. It was not too much to say that their having been spread in small numbers over a vast continent produced almost necessarily with them, as with many European settlers in new countries, the loss of many of those acquirements of civilisation which they had original possessed.—The Rev. G. Taplin contributed "A comparative table of the Australian languages."—Mr. C. S. Wake read a paper on the mental characteristics of primitive man, as exhibited in the Aborigines of Australia. With reference to the subject of religion the author maintained that as the aborigines could not be said to have any worship at all the argument that they had been derived from inhabitants of the temperate zone was not of any value. That the Australian aborigines did possess certain points of affinity with other races was, in his mind, unquestionable. Those points of affinity, however, did not prove that the Australians had fallen from a higher state of civilisation, or that as a race they had been derived either from Southern India or from Northern Asia. A race, whatever degradation it might undergo, could never lose all trace in its social condition of that which it once possessed, and sink back to the exact state in which it must have been when it first emerged from a condition of barbarism.

Mathematical Society, April 13.—Mr. W. Spottiswoode, F.R.S., president, in the chair. Mr. C. J. Monro, B.A., late Fellow of Trinity College, Cambridge, was proposed for election, and the following gentlemen were elected members:—The Hon. J. W. Strutt, Major Frederick Close, R.A., and Mr. James Stuart, Fellow and Tutor of Trinity College, Cambridge.—The President briefly alluded to the loss the society had recently experienced by the death of its first President, the late Prof. De Morgan. Dr. Hirst, who had been intimately associated with Mr. De Morgan, when both were Professors in University College, gave a sketch of the work done by the deceased professor, especially dwelling upon the originality and acuteness displayed in all his writings, instancing, in particular, his "Double Algebra," his work on the "Differential and Integral Calculus," and his "Formal Logic." The speaker also dwelt upon the warm interest he had taken in the society from its first establishment, which led him to accept the office of first President, and to deliver the inaugural address, and then proceeded to pay a cordial tribute to his personal character. In conclusion, Dr. Hirst stated that Mr. De Morgan's valuable and unique collection of curious works bearing upon the "History of Mathematics," greatly enriched by the owner's own numerous and characteristic quotations, was, he believed, to be disposed of. Several members present expressed their wish that the collection might not be dispersed, but be secured in its entirety for the London University or for the British Museum.—Prof. Crofton, F.R.S., then explained his diagrams in illustration of the "Stresses in Warren and Lattice Girders." Prof. Henrici and Mr. Merrifield, in their remarks on the communication, drew attention to the fact that Mr. Crofton had been anticipated in his constructions in a work by K. Culmann; "Die Graphische

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* Communicated by Prof. Morris.

Statik" (1866), who had not only treated this particular matter in a freer manner, but applied his methods to a much wider range of subject. Prof. Henrici further illustrated the subject of the communication by a very simple and ingenious construction.—Prof. Cayley, V.P., followed with a brief sketch of the contents of his third memoir on "Quartic Surfaces."

Institution of Civil Engineers, April 18.—Mr. Charles B. Vignoles, F.R.S., president, in the chair.—"On the Archimedean Screw Propeller, or Helix, of Maximum Work," by Sir F. C. Knowles, Bart. In considering the construction and action of the Griffiths' Screw Propeller, the author of this memoir was struck by the fact that the blades worked in great part in the lateral streams of the water, and had no action in the dead water behind the sternpost, where power applied ought to be the most efficient. Again, in the common screw propeller at all points near the axis, the power was almost wholly employed in churning the water, and in producing vibration by alternately lifting and depressing the stem, which no doubt induced Mr. Griffiths to limit the extent of his blades to points without that space. These considerations led to an endeavour to devise some form of blade which should be free from that imperfection, and yet on the whole possess the feathering property of the Griffiths' screw. But no particular form presenting itself which on principle could be pronounced preferable to any other form, the author decided upon proceeding to an *a priori* solution of the question, and assuming the existence of some best form, he was ultimately led to propose this problem: "What is the form of the surface of the screw propeller of which the 'work done' is the greatest possible?" The complete solution of this problem was the subject of the paper, and the following was an outline of the methods employed, and of the results obtained. Referring the required surface to three rectangular co-ordinates x, y, and z, one in the axis of rotation, the other two in the plane of rotation, the author first obtained a general expression for the total "work done" by the blade in propelling the ship, in the form of a double integral in terms of the co-ordinates x and y and of the partial differentials of z with respect to each of them, of the speed of rotation of the blade, and lastly of the speed of the ship. As this integral was to be a maximum for all points of the surface sought, it must be treated by the known methods of the Calculus of Variations. This done, an equation of condition was obtained, which, by the performance of the operations indicated by the symbols, led to an equation involving two factors, each factor being a partial differential equation between the three co-ordinates of the surface. The first of these being integrated gave for its solution the whole family of ordinary helices which were the surfaces of *least* work. The second factor was the differential equation of the required surface, the treatment of which was given in the paper *in extenso*. It led at once, and very simply, to an equation analogous to that of the common

$$\text{helix} \left(\tan.\, \theta = \frac{a \tan.\, a}{r} \right) \text{ namely, } \tan.\, {}^2\theta = \frac{a \tan.\, 2a}{r}$$

From this it was at once deducible, that the surface of the blade at the axis cut the plane of rotation at an angle of 45°, while the common helix cut it at 90°, and therefore acted powerfully in the dead water to propel the ship, just where the common helix had no propulsive power. It was proposed to call this surface the hemi-helix, or hemi-angular helix.—The paper then proceeded to determine the pressure of this blade upon the vessel in the direction of the keel, and thence the whole circumstances of the ship's motion. It was found that there was what was called "a slip," as in the case of the common helix. The author objected to this term, as involving a fallacious theory of the action of the screw,—in effect a denial of the equality of action and reaction. In order fully to expose the fallacy, the motion of a ship impelled by the common helix as a case of variable motion in a resisting medium was investigated, and, from the identity of the conditions and of their algebraical expressions it was proved that what was called "slip" of the screw was neither more nor less than "the ratio of the difference between the velocity which the ship would have in a non-resisting medium and its actual terminal velocity in the water to the former velocity." It was proposed, therefore, to substitute for this objectionable expression the term "ratio of resistance," or "relative resistance," as accurately representing the real phenomena, and measuring the efficiency of the given screw in propelling the given vessel. The author was thus further enabled to explain what had been called "negative slip," and to assign its origin to the joint action of wind and steam, it being impossible in the case of steam alone. In the course of the discussion, objection was taken to the fundamental principles enunciated in the paper; although those principles used to be almost universally promulgated in mathematical treatises of reputed authority, and were commonly even now relied on as the basis of mathematical reasoning, by those whose investigations and experimental researches had not obliged them to detect their unsatisfactory character. Reference was made, in the first place, to what might be called the very foundation of the author's deductive process,—the proposition that when a plane moved obliquely through a fluid at a given velocity, the normal pressure on its surface was as the square of the sine of the angle of obliquity. In the second place, to the hypothesis that when the true law of pressure on a plane thus moving had been in any way determined, the local pressure on each unit of surface of a curved surface moving through a fluid could also be determined, by applying that law to the unit in virtue of the angle presented by its tangent plane to the line of motion. And thirdly to what appeared to be a misconception of the dynamical relations, or inherent conditions of the slip of the propeller.

Zoological Society, April 18.—Dr. E. Hamilton in the chair. The Secretary read a report on the additions that had been made to the society's collection during the month of March, 1871. Amongst these particular attention was called to a young male specimen of the Cape hunting dog (*Lycaon pictus*), a species which had been deficient to the society's collection since 1855.—Mr. H. E. Dresser exhibited a specimen of the American yellow-billed cuckoo (*Coccyzus americanus*), recently killed in England, and Sir Victor Brooke a specimen of the Esquimaux curlew (*Numenius borealis*), lately killed in Ireland.—Prof. Owen, F.R.S., read a paper on the dodo (*Didus ineptus*), containing notes on an articulated skeleton of this extinct bird, recently prepared from bones exhumed by Mr. Clark in the Mauritius, and now exhibited in the Ornithological Gallery of the British Museum.—A paper was read by Mr. Thomas Davidson, F.R.S. (communicated by Mr. J. Gwyn Jeffreys), containing a revised account of the recent Brachiopoda dredged by Mr. Arthur Adams in the Japanese Seas.—Messrs. Sharpe and Dresser pointed out the characters of a new form of long-tailed titmouse, which occurs in Southern Spain and in Italy, and which they proposed to call *Acredula irbii*.—Mr. R. B. Sharpe read the second part of his "Contributions to the Ornithology of Madagascar," in which was given an account of a collection of birds recently made by Mr. Crossley in that island. Among these were a specimen of a new swift proposed to be called *Cypselus gracilis*.—A communication was read from Dr. A. Gunther, F.R.S., containing the description of a new form of percoid fishes from the Macquarie River, Australia, which he proposed to call *Ctenolates macquariensis*.—A communication was read from Dr. James C. Cox, containing descriptions of some new species of land and marine shells, from Australia and the Southern Pacific.—Two communications were read from Mr. J. Brazier. The first contained descriptions of some new land-shells from New South Wales—the second notes on some species of shells recently described by other authors from the Australian region.

Chemical Society, April 20.—Prof. Odling, F.R.S., vice-president, in the chair. The following gentlemen were elected fellows: C. C. Grundy, J. B. Lee, G. Sutcliffe, W. Ward. Mr. C. Haughton Gill read a paper on some saline compounds of cane sugar. The author having succeeded in obtaining a crystalline compound of sodic chloride with cane sugar, mixed a number of sugar solutions with different salts and set them to crystallise spontaneously, or when no crystals were obtained by these means a more rapid evaporation was tried. The salts employed were the chlorides of potassium, sodium, lithium, and ammonium; the bromides of potassium and sodium, and the iodides of potassium, sodium, lithium, and ammonium. None of the potassium salts gave compounds of a definite composition. The sodium salts gave much better results; there were two varieties of sodic chloride compounds formed, constituted respectively $2\,(C_{12}H_{22}O_{11})\,.\,3\,Na\,Cl\,.\,4\,H_2O$ and $C_{12}H_{22}O_{11}\,.\,Na\,Cl\,.\,2\,H_2O$; the solutions containing sodic bromide gave crystals of the composition $C_{12}H_{22}O_{11}\,.\,Na\,Br\,.\,1\tfrac{1}{2}\,H_2O$, they were probably not quite pure; the sodic iodide combination, $2\,(C_{12}H_{22}O_{11})\,.\,3\,NaI\,.\,3\,H_2O$, formed very fine crystals. The mixture containing lithium gave only crystals of pure cane sugar. The constitution of the sodic iodide compound makes it seem probable that the true molecular weight of cane sugar should be represented by $C_{24}H_{44}O_{22}$. The measurements of the crystals mentioned in Mr. Gill's paper were executed by Prof. Miller.

PARIS

Academy of Sciences, April 3.—The account of this sitting is printed in the *Journal Officiel*, the first time since the beginning of the investment of Paris. The account was signed and written by M. Henry de Parville.—M. Simon Newcomb was present at the sitting, and read a communication on the perturbations in the motion of the moon, owing to the attraction of the different planets. The American astronomer proposes to solve this great question, by a new method. The problem is to be solved by one hundred and fifty different equations! M. Newcomb admits it is quite impossible to solve it, without some startling simplification. The present communication is to show the Institute the way through which the author hopes to realise these 'simplifications. It is not the final work, which, if it is successful, will prove M. Newcomb has made a great analytical discovery. But in such a matter *experience passe science.* M. Delaunay offered no remark on the subject, although he has devoted to it the greater part of his life. He remains on the defensive.—M. Robin, the celebrated microscopist, handed a note on the analysis of blood taken from a man attacked by scurvy. The author shows, from observations made at Necker and Gros Caillou Hospitals, that under these circumstances many white globules are mixed in the blood. But leucocithemic having been observed in other affections, it cannot be considered as a specific character of that dangerous affection so common in besieged towns, and which was frequently observed in Paris towards the end of 1870.

The candidates for the great prize for curing cholera are not easily frightened by the civil war. The premium of 4,000*l.*, given by the late M. Breant, is worth incurring any risk. A M. Drouet sends a note giving the true cure. The process is simple enough. It is sufficient to cover the stomach of the patient with the film of a collodion, prepared by a mixture of ordinary collodion and castor oil. This collodion film is a specific against hysterical fits, typhoid fever, &c. It must be admitted that the total stopping of all perspiration may be considered as being of some help in different affections. The process may be considered as worth a trial at least, under some peculiar circumstances.—It was stated that the Institute will not interrupt its sittings at any price. If there remains only a single member, that member will sit in order to keep a regular register of communications addressed to the learned company. It is the only means for securing intellectual prosperity of scientific discoveries, and it cannot be stopped even by Communalists. We have no news of the sitting of the 10th.

BOOKS RECEIVED

English.—The Illustrated History of British Butterflies: E. Newman (W. Tweedie).—Select Methods of Chemical Analysis: W. Crookes (Longmans).

American.—Medical and Surgical Electricity: G M. Beard and A. D. Rockwell (New York, W. Wood and Co.).

Foreign.—(Through Williams and Norgate)—Die Anwendung des Spectral-apparatus: K Vierordt.

PAMPHLETS RECEIVED

English.—On the Gibraltar Currents, the Gulf Stream, and the General Oceanic Circulation: Dr. W. B. Carpenter.—Preparatory Programme of the National University for Industrial and Technical Training, and Report of Provisional Committee—Transactions of the Scottish Arboricultural Society, Vol. vi, part 1.—The Iron and Steel Institute: Address of the President, Mr. H. Bessemer.—The Sicilian Eclipse Expedition: Lecture by Dr. T. E. Thorpe—Barometer, Manual, Board of Trade: R. H. Scott.—Annual Report of the Manchester Scientific Students' Association for 1870.—Protest against Mr. J. H. Walshs Decision in the Bedford Level Survey: W. Carpenter.—Gas, its high Price in the Metropolis, and the Way to Reduce it: C. G. Cleminshaw—Explanatory Memoir to accompany sheets 86, 87, 88, and eastern part of 85 of the maps of the Geological Survey of Ireland: G. H. Kinahan and R G. Symes.—Common Salt, a Remedy against Small-ox.—On the Uniform Flow of a Liquid: Canon Moseley.—On the Mean Thickness of the Sedimentary Rocks of the Globe: James Croll.—The Proceedings of the Cotteswold Naturalists' Field Club for 1869.—On the Post Glacial Deposits of Western Lancashire and Cheshire: C. E. de Rance.—On the Glacial Phenomena of Western Lancashire and Cheshire: C. E. de Rance.—A Lecture on Vegetarianism: Prof. F. W. Newman.—Science Lectures for the People delivered at Hulme Town Hall, second series.

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Foreign—Die Tangentialwage und ihre Anwendung zur Bestimmung der Dichte fester u. flussiger Körper mittelst directer Ablesung: K. W. Zenger.—Ueber die Steinsalzablagerung bei Stassfurt: C. Reinwarth.—Zur Erinnerung au Wilhelm Haidinger: F. Ritter v Hauer.

DIARY

THURSDAY, April 27.

Royal Society, at 8.30.—On the Increase of Electrical Resistance in Conductors with Rise of Temperature, and its application to the measure of Ordinary and Furnace Temperatures; also on a simple Method of Measuring Electrical Resistances: C. W. Siemens, F R.S.

London Institution, at 7.30.—On Economic Botany: Prof Bentley.

Royal Institution, at 3—On Sound: Prof. Tyndall.

FRIDAY, April 28.

Quekett Microscopical Club, at 8.

Royal Institution, at 9.—On the revived Theory of Phlogiston: Prof. Odling, F.R.S.

SATURDAY, April 29.

Royal School of Mines, at 8.—Geology: Dr. Cobbold.

Zoological Society, at 1.—Anniversary Meeting.

Royal Institution, at 3.—On the Instruments Used in Modern Astronomy: J. N. Lockyer, F.R.S.

MONDAY, May 1.

Entomological Society, at 7.

Royal Institution, at 2.—Annual Meeting.

London Institution, at 4.—On Astronomy: R. A. Procter, F.R A S. (Educational Course.)

TUESDAY, May 2.

Zoological Society, at 9—On the Birds of the Island of Celebes: Viscount Walden.—On Speke's Antelope and the allied species of the genus *Tragelaphus*: Sir V. Brooke, Bart.

Society of Biblical Archæology.—On a Hieroglyphic Tablet of Alexander, son of Alexander the Great, recently discovered at Cairo: S. Birch, LL.D., F.S.A.—Some notice of three new bronze Himgaritic Tablets.

Royal Institution, at 3—On the Geology of Devonshire, especially of the New Red Sandstone System: William Pengelly, F.R.S.

WEDNESDAY, May 3.

Royal Microscopical Society, at 8—On the Structure of Lepidopterous Scales as bearing on the Structure of Lepidocyrtus curvicollis: Dr. Maddox.—On the Foot of Dityscus marginalis: B. T. Lowne.

Society of Arts, at 8—On the Production of Artificial Cold: Prof. J. Gamgee

THURSDAY, May 4

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Linnean Society, at 8—The phenomena of Protective Mimicry, and its bearing on the Theory of Natural Selection as illustrated by the Lepidoptera of the British Islands: Raphael Meldola, F.C.S.

Chemical Society, at 8.—On the Productive Powers of Soils in relation to the Loss of Plant Food by Drainage: Dr. Voelcker, F.R.S.

Royal Institution, at 3—On Sound: Prof. Tyndall.

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APOLOGY.

SIR, *16th February*, 1871.

I have to offer you a most full apology, and to express my most sincere regret for having, through the medium of my Printing Establishment, been the means of circulating a certain Newspaper, called the "Armourer," ostensibly published "for the defence of God's truth against man's impostures, whether in divinity, science, or political and social Economy," of which Mr. John Hampden, of Swindon, Wilts, is Editor and Proprietor; and also a Pamphlet, entitled "Is Water Level or Converse, after all?" both of which were printed by me for the said John Hampden, and contain certain Gross Libels against you.

I have intimated to Mr. C. E. Shea, your Solicitor, that I am willing to insert this Apology, at my own expense, in the following public papers, viz.:—"The Times," "Field," "North Wilts Herald," and one other Wiltshire paper, "The Weston-Super-Mare Gazette," NATURE, and "The English Mechanic."

I am, Sir, your obedient servant,
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